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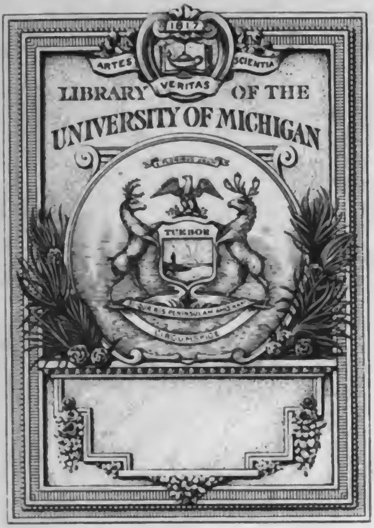

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JOURNAL OF SCIENCE.

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> "Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATUM IN VOL. VIII.

In Table I., p. 421, in the second line of the column headed "Masses," *for* 20^{21} *read* 10^{21}.

ERRATA IN VOL. IX.

Page 243, foot note, last line, *for* against *read* with; and *for* DC *read* AB.
— — last line but two, *for* with *read* against.
— — — *for* AB *read* DC.
— — line 6 from foot, *for* A *read* D.
— — — *for* B *read* C.
Page 354, line 23, *for* $\frac{1}{9}$th *read* $\frac{1}{12}$th.

ALERE FLAMMAM.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
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[FOURTH SERIES.]

JANUARY 1855.

I. *On a Method of increasing certain Effects of Induced Electricity.* *By* W. R. Grove, *Esq.**

IN the course of last year I observed that, by connecting the coatings of a Leyden phial with the extremities of the secondary coil of Ruhmkorff's apparatus, a great increase in the brilliancy of the discharge could be obtained. Circumstances diverted my attention from it at the time, and I did not publish the experiment, though I believe Mr. Gassiot mentioned it in one of his papers. I have since heard that M. Sinsteden in France had made the same observation, though I do not know when, nor whether he has published his experiments.

The point which I now think may be worth insertion in the Philosophical Magazine, is the conversion, by means of a Ruhmkorff coil, of an indefinite amount of voltaic power into static electricity. If a small Leyden phial have its coatings connected respectively with the extremities of the secondary wire of a Ruhmkorff coil (the primary being, as usual, connected with the condenser of M. Fizeau, and two wires being attached to the terminals and brought within striking distance), the noise and brilliancy of the discharges are greatly increased, with generally a slight, but a very slight, diminution in their length. If now the voltaic battery be increased, the coil and Leyden phial remaining the same, but little increase in the length or brilliancy of the sparks will ensue, that is, provided the battery was in the first instance sufficiently powerful to give the maximum effect of the coil without the phial. For instance, if with a Ruhmkorff coil of the size now usually made, 10 inches long by 4 diameter, four cells of 2 inches by 4 of the nitric acid battery be used, and a pint Leyden phial, but little increase of effect will be obtained by using eight or more cells, and the platinum at the contact breaker would be rapidly destroyed by the sparks.

---

* Communicated by the Author.

But substitute for the pint Leyden phial one of double the capacity, and it will be found, that though this second phial was inferior to the first with a battery of four cells (giving shorter sparks, and fewer in a given time, though somewhat denser), yet it is far superior to the first with the battery of eight cells, and the sparks at the contact breaker are no longer injurious.

By adding more coated surface, for instance, another phial, four more cells may be added, and increased effects will be obtained, and thus with *the same coil* the brilliancy of the discharge may be increased to an extent to which I have not yet found a limit. I obtained this result some months back; but not having a large battery, I did not go beyond ten cells, which I found would well bear a jar of one square foot coated surface.

By the aid of Mr. Gassiot's more powerful apparatus, I have, with him, used thirty cells of the nitric acid battery, 2 inches by 4, and five square feet of coated surface; the effects were very striking,—a roar of voluminous discharge of 0·6 of an inch long, increased to 1·5 inch when the flame of a spirit-lamp was placed between the terminals. I have never witnessed such a torrent of electrical discharges; the noise could not be borne long without great discomfort.

With the same voltaic battery, and an additional square foot of coated surface, the effect was somewhat diminished. Mr. Gassiot had not more than thirty cells available at the time of our experiments, so that I have not yet ascertained the limit to which this increase of power can be carried. I presume, however, there is a limit, for reasons which will be presently apparent.

The following precautions are essential to the success of the experiment:—

1st. The wire proceeding from the outer extremity of the secondary coil must be connected with the inside or insulated coating of the Leyden battery, if the battery is not wholly insulated. The reason of this is, that the outer extremity of the coil is the better insulated portion, and also that to which electricity of tension flies off; a good spark can, under ordinary circumstances, be obtained from the outer, but scarcely any from the inner terminal of the coil.

2nd. The distance between the hammer of the contact breaker and the soft iron core should be made as great as practicable, at least one-eighth of an inch; this is an important point as to the theory and experimental results of the Ruhmkorff coil. Time, as is well known, is necessary for the development of electro-magnetism; and M. Matteucci, in his recent valuable book on induced electricity, has shown some remarkable results flowing from this fact. If the hammer be too near the core, the former is raised before the latter has time to be fully magnetized; and when a Leyden condenser is used, further time is required for this to be charged. This demand of time indicates the probable limit to the increase of power to which I have above alluded.

It is very curious to see the *absorption,* so to speak, of voltaic power by the Leyden battery: when the maximum effect for a given Leyden jar has been passed, the contact breaker shows by its sparks the unabsorbed induced electricity which now appears in the primary wire; an additional jar acts as a safety-valve to the contact breaker and utilizes the voltaic power, and so on.

It is a question of some interest why a jar charged in the ordinary way by temporary contact of the terminals of a secondary coil will only receive a very slight charge, and give a discharge of scarcely measurable length, yet when permanently connected with the terminals, will give a long and powerful discharge. The following is the best theory I can offer. At the moment of the inductive action or wave of electricity, the same wire which is affected by the electric impulse is unable to conduct it back again, and thus to discharge the jar; while, when the jar is attempted to be charged in the ordinary way, the contact, however apparently of short duration, lasts longer than the single impulse of electricity, and so the coil in great part discharges the jar. Some such state of the wire as that I have suggested must exist at the moment of an induced current, as otherwise the wire would discharge itself, or in other words, would never receive a charge or state of opposite electricity of great tension at its extremities. At one time I considered the explanation to be, that at the moment of breaking contact a portion of the induced electricity flies off across the discharging interval in the form of a spark, and thus enables the jar to discharge itself just as the voltaic arc will pass across the path of an electric spark, though it will not pass through a measurable distance of interposed air without the spark. This theory, however, does not satisfactorily explain the great increase in the charge of the Leyden phial, as compared with the charge by contact.

3rd. It must be borne in mind that each coating of the Leyden phial must be connected with each terminal; the jar is not, as many have tried the experiment, to be interposed in the secondary circuit.

The number of discharges in a given time will depend upon the intensity of the battery, and its relation to the amount of coated surface; the eye cannot estimate this, but a rough measurement of the rapidity of succession may be made in the following way. Move across the line of discharge with a steady hand a strip of writing-paper, it will be punctured with a row of holes, which will be the more closely approximated in proportion as the succession of discharges is more rapid. By a disc of paper attached to an axis moving with a given rate of revolution, this measurement may be made very accurate and useful.

Those who possess the coil apparatus will find it very convenient to have a plate of glass, coated on each side with tinfoil, placed on the base of the machine, and having strips leading from each coating to binding-screws, with which the terminals of the secondary coil can be corrected at will.

December 7, 1854.

II. *On the Elastic Forces of Vapours* in vacuo *and in Gases, at different Temperatures; and on the Tensions of the Vapours furnished by mixed or superposed Liquids.* *By* V. Regnault*.

[Concluded from vol. viii. p. 280.]

Part III.—*On the Elastic Force of Vapours in Gases.*

PHYSICISTS in general admit that vapours behave in the same manner in gases as *in vacuo*, with this difference only, that in gases the equilibrium of tension is slowly established, whilst *in vacuo* it is produced almost instantaneously. An apparatus invented by Gay-Lussac is usually employed in lectures on physics, to show that the elastic force of vapours is exactly the same in gases as *in vacuo*. Nevertheless there is no record of any exact experiments having been made for the establishment of this law, which is of great importance, especially in meteorology.

During my hygrometric investigations (published in the fifteenth volume of the *Annales de Chimie et de Physique*†), I very carefully determined the weight of the aqueous vapour existing in the atmosphere at its different states of saturation. I ascertained that, even during long-continued rains, which must have maintained the air in a state of saturation, the quantity of water found by experiment was always less than that determined by my calculations, based on the one hand upon the elastic force which I had found for aqueous vapour *in vacuo*, and on the other upon the theoretical density of this vapour.

This circumstance might arise from two causes:—

1. The elastic force of vapour of water might be exactly the same in the air as *in vacuo*;

---

* From the *Comptes Rendus* for August 21 and 28, 1854.

† Taylor's Scientific Memoirs, Part XVI.

2. The actual density of aqueous vapour in the air might differ from that obtained by calculations founded upon Mariotte's law and the theoretical density; for this density was only verified by Gay-Lussac at a temperature of 100° C., and under pressures which differed but little from the ordinary pressure of the atmosphere.

I wished first of all to determine as exactly as possible the weight of vapour which might be contained in air saturated with humidity at different temperatures; for it might be objected, that in the experiments in which I derived the saturated air directly from the atmosphere, there remained some uncertainty as to the exact determination of the temperature. My new experiments were free from this objection, for I drew the air, by means of an aspirator, into a series of tubes filled with wet sponge, and kept at a strictly invariable temperature during the whole course of the experiment. Sixty-eight determinations effected in this manner between limits of temperature from 0° to 27° C. all gave the weight of vapour lower than that deduced from calculation. The differences were, however, inconsiderable, rarely rising to $\frac{1}{50}$th of the total weight.

The fact being thus completely proved, its cause remained to be discovered, and for this purpose I undertook experiments to determine directly the density of aqueous vapour within limits of temperature analogous to those existing in my first experiments, as well as the elastic force of this vapour in the air. Unfortunately the direct determination of aqueous vapour in gases at low temperatures presents difficulties which are almost insurmountable, the ponderable quantity of this vapour being too small in proportion to that of the whole elastic fluid. I was therefore compelled to confine myself to making experiments *in vacuo.* As long as the fraction of saturation does not attain $\frac{8}{10}$ths, I found the density of aqueous vapour to be equal to that deduced from the theoretical density, by the application of Mariotte's law. But this density increases rapidly in proportion as it approaches saturation. I think, nevertheless, that this rapid increase is produced principally by a condensation of liquid water upon the walls of the balloon in which my experiments were made; and this condensation commences long before saturation.

I could find no means of getting rid of this cause of error, and I believe that the most exact method of determining the density of a vapour in a gas still consists in condensing, by means of absorbing substances, the vapour existing in a given volume of gas, under known conditions of temperature and pressure. But in this case the law of elastic forces must be thoroughly settled.

I have detailed (at page 134 of my memoir) * the experiments which I made in order to determine directly the elastic force of aqueous vapour at saturation in air and nitrogen gas at different temperatures. The ninety-one determinations which I made between 0° and 40° C. gave, without exception, the elastic force of aqueous vapour as weaker in the air than *in vacuo*. The differences are of the same kind as those previously found between the weight of water which saturates a given volume of air, and the weight deduced from calculation. Nevertheless, in these experiments I endeavoured to vary the circumstances as far as possible, and especially to realize those conditions in which it might be expected that saturation would be obtained.

Aqueous vapour having but a feeble tension at low temperatures, it became important to extend these experiments to more volatile fluids. I accordingly made use of æther, sulphuret of carbon, and benzine. The apparatus employed was that described in the *Annales de Chimie et de Physique,* 3 sér. vol. xv. p. 131 †. Into a balloon, of the capacity of 600 to 700 cubic centims., a small hermetically-closed capsule was introduced, containing the liquid to be operated upon. This balloon, to the neck of which a tube of considerable diameter had been previously attached, communicated with a mercurial manometer. The entire apparatus was placed in a large tub filled with water, which was kept at a constant temperature. A piece of glass was let into one part of the side of the tub, through which the manometer might be observed. The apparatus being arranged, a vacuum was repeatedly produced in the balloon, the air admitted after each exhaustion being perfectly dry. Lastly, the apparatus was hermetically sealed, containing air under the pressure of the atmosphere.

The elastic force acquired by this dry air, kept constantly at the same volume, at different temperatures, having been determined, the capsule was ruptured by the action of heat, and the series of observations was recommenced on the air saturated with vapour. The liquid enclosed in the capsule had previously been kept from contact with the air, in order that it should not have a tendency to absorb the air of the balloon. The volume of the capsule in proportion to that of the balloon may also be varied in different experiments, so as to make sure that the absorption or disengagement of air by the liquid has not caused any sensible perturbation. The following are some of the results that I have obtained:—

---

* Scientific Memoirs, vol. iv. p. 609.

† Ibid. p. 706.

First Series.

Capacity of the balloon, 668 cub. centims. Capsule containing 7·4 cub. centims. of liquid. Elastic force of the air at 0°C., 731·89 millims.

Temperatures.	Elastic force of the vapour in the gas.	Elastic force of the vapour *in vacuo.*	Difference.
° 5·17	225·94	232·5	mm 6·6
14·42	340·15	345·3	5·1
14·38	336·48	344·5	8·0
20·78	439·50	445·6	6·1
20·78	439·78	445·6	5·8
11·09	297·10	300·2	3·1
11·11	296·78	300·6	3·8
19·37	414·02	423·1	9·1
12·22	311·30	315·0	3·7

Second Series.

Capacity of the balloon, 690 cub. centims. Weight of liquid contained in the capsule, 7·7 grms. Elastic force of the air alone at 0° C., 724·22 millims.

In this series the operations were carried on with a descending temperature, so as to realize saturation with greater certainty.

33·51	702·69	722·8	20·1
33·62	705·09	726·0	20·9
30·97	645·62	659·0	13·4
26·52	552·67	559·2	6·5
22·83	479·63	484·0	4·4
20·05	429·69	433·9	4·2
19·99	428·88	433·0	4·1
14·26	337·71	341·0	3·3

These tables, in which I have only inserted the greatest tensions observed at given temperatures, show that the elastic force of vapour of æther in the air is constantly less than that obtained *in vacuo*; the difference becomes greater in the same proportion as the elastic forces themselves.

Fearing that in the preceding experiments the tube by which a communication was established between the balloon and the manometer might be too small to allow of the necessary diffusion of the vapour, I replaced it by one of the same diameter as the manometric tube itself, that is to say, 22 millims. Under these conditions the following series of experiments was made. A small quantity of the liquid was always distilled over into the manometric tube before the commencement of the observations.

Third Series.

Elastic force of the air at 0° C., 706·91 millims.

Temperatures.	Elastic force of the vapour in the air.	Elastic force *in vacuo*.	Difference.
16°·73	368·81	379·7	mm 10·9
16·75	369·39	379·8	10·4
20·78	435·21	445·8	10·6
20·79	435·26	445·9	10·7
29·31	583·81	619·0	36·2
29·31	588·68	619·0	30·3
29·31	593·03	619·0	26·0
22·84	470·13	483·8	13·7
22·86	466·03	483·9	17·9
22·84	467·54	483·8	16·3

It will be observed that this series presents still greater differences than the preceding, but this arises from the maximum determinations only having been inserted. The observations at the same temperature were made at intervals of half an hour.

It would take up too much time to indicate here the different circumstances under which the partial determinations for these three series were effected,—these circumstances exercise a great influence upon the values found for the elastic force of the vapour. I shall content myself with saying, that very often a determination was not made until after the bath had been kept for several hours at a constant temperature and in continual agitation. In other cases the bath was allowed to cool very slowly, keeping it constantly agitated, and the observations were made during this period of descent in the temperature, which was very favourable to saturation.

The greatest value was obtained for the elastic force at a given temperature, by suddenly cooling the balloon so as to induce an abundant condensation of the vapour, and then rapidly re-establishing the original temperature. By making observations afterwards every quarter of an hour, the temperature of the bath remaining exactly the same, the elastic force will be found to diminish gradually during several hours. If, on the contrary, the temperature of the bath be raised gradually up to the stationary point, and observations be then made every quarter of an hour, it will be seen that the tension of the vapour constantly increases, without, however, attaining, not only the tension which it presents *in vacuo*, but even that observed at the same temperature in the experiments by refrigeration immediately after the condensation of the excess of vapour. I shall give the explanation of these facts hereafter.

The general tendency of these experiments is to show that the elastic force of vapour of æther in the air is constantly weaker than *in vacuo*. The absolute difference is greater than with water, and increases with the elastic force itself.

The experiments which I have made with sulphuret of carbon and benzine lead to the same result. The difference between the elastic force presented by the same vapour in the air and *in vacuo*, diminishes in proportion to the volatility of the liquid, as may be seen from the following tables:—

Tensions of the vapour of sulphuret of carbon in the air.

Temperatures.	Elastic force of the vapour in the air.	Elastic force of the vapour *in vacuo*.	Difference.
13°·45	227·38	229·3	mm 2·0
22·79	328·66	332·8	4·1
22·81	328·90	333·0	4·1
30·65	441·41	445·7	4·3
Tensions of the vapour of benzine in the air.			
10·64	46·80	48·2	1·4
10·67	46·98	48·3	1·4
10·68	46·92	48·3	1·4
11·37	47·96	50·3	2·4
11·38	48·57	50·3	1·7

In order to investigate this same subject, I have employed a method which presents many advantages over the preceding. It allows us to study with precision the influence exerted upon the elastic force of the vapour by the total pressure of the gaseous atmosphere which acts upon the volatile liquid, and the quantity of liquid in excess by which the walls of the vessel are moistened. For this purpose I made use of the eudiometer which I applied to the analysis of gases, and which is described in the *Annales de Chimie et de Physique*, vol. xxvi. p. 333.

I replaced the measuring tube of this apparatus, which usually consists of a glass tube traversed by two platinum wires, by another glass tube which was not furnished with wires, and which was graduated in centimetres so as to indicate the volume of gas. Into this tube the gas in which the volatilization was to be effected was introduced, with the precautions indicated in the memoir just quoted; then, keeping the bath which surrounded the apparatus constantly at a temperature of 7°·7 C., which was very near that of the surrounding atmosphere, the elastic force presented by this dry gas was determined by the cathetometer, the surface of the mercury in the measuring tube being brought successively to each of the divisions marked upon the tube.

This done, a certain quantity of the volatile liquid was introduced into a test tube, which had been previously filled with dry mercury, exhausted *in vacuo*; then, after the communication between the two tubes had been established, the gas was passed from the measuring tube into the other, under a pressure sufficiently low to allow the whole of the liquid introduced to volatilize in the gaseous space thus afforded it. Lastly, the gas mixed with vapour was repassed into the measuring tube.

A new series of determinations as to the elastic force of the gaseous mixture was then commenced, by causing this to occupy successively the same volumes that had been occupied by the dry gas in the first series. It is clear that by deducting from the numbers of the second series the corresponding numbers of the first, we get the elastic forces of the vapour at a constant temperature, but under variable pressures and different volumes, and consequently in variable states of saturation.

From these different observations it may be deduced:—

1. From the first series, whether the dry gas follows the law of Mariotte. I shall give in another memoir the observations which I have made in this manner upon the compressibility of a great number of gases. The method, when executed with the necessary care, allows us to prove that atmospheric air itself departs sensibly from this law under the slightest variations of volume. The differences belong to the order of those deduced from the curve which I have already published, and which includes a great interval of pressure. By the same method I have determined the law of compressibility followed by mixtures, in known proportions, of atmospheric air with a gas, which, like carbonic acid, departs greatly from Mariotte's law.

2. By combining the two series, we may ascertain how far the gas, more or less charged with vapour, departs from Mariotte's law, when it is more or less distant from the state of saturation.

3. We may determine how the elastic force of the vapour varies from the moment when the liquid begins to be deposited in drops upon the walls of the tube, and consequently ascertain whether the elastic force of this vapour varies in proportion to the quantity of liquid condensed.

I give here the series of experiments which I have made upon vapour of æther in atmospheric air and hydrogen gas. I have subjoined other experiments by which I have determined the elastic force of the same vapour in carbonic acid gas, although I do not think that the latter can be employed with confidence in the determination of the tension of the vapour, in consequence of the great solubility of carbonic acid gas in æther, which in this case produces a considerable perturbation.

On the tension of the vapour of æther in the air.

(Tension of the vapour *in vacuo* at 7°·7 C.=260 millims.)

Numbers of the divisions.	Elastic force of dry air.	Elastic force of air with the vapour.	Elastic force of the vapour of æther.
54	611·03	775·93	164·90
52	630·93	801·33	170·40
50	652·13	828·18	176·05
48	675·03	857·08	182·05
46	699·23	887·58	188·35
44	725·58	921·08	195·50
42	753·68	956·68	203·00
40	784·53	995·18	210·65
38	817·83	1036·98	219·15
36	853·73	1082·08	228·35
34	893·13	1131·48	238·35
32	936·68	1182·13	245·45
30	984·58	1232·28	247·70
28	1037·13	1287·58	250·45
26	1097·03	1348·73	251·70
24	1163·83	1416·23	252·40
22	1238·13	....	
54	......	775·83	
A fresh quantity of æther was introduced into the laboratory tube and made to dissolve in the air.			
54	......	862·63	251·60
52	......	883·63	252·70
50	......	905·63	253·50
48 on the following day.	......	929·38	254·35
48	......	929·28	254·25
46	......	954·23	255·00
44	......	981·08	255·50
42	......	1010·18	256·50
40	......	1041·28	256·75
38	......	1075·18	257·35
36	......	1111·58	257·85
34	......	1151·31	258·18
32	......	1195·68	259·00
30	......	1243·98	259·40
28	......	1297·28	260·15
26	......	1357·03	260·00
24	......	1423·93	260·10
The æther was in excess from the commencement of this second series.			

Tensions of the vapour of æther in hydrogen gas.

(The measures were all made at a temperature of 7°·7 C.

Numbers of the divisions.	Hydrogen alone.	Hydrogen with æther.	Tensions of the vapour of æther.
48	570·73	813·58	242·85
	after ½ h.	816·18	245·45
46	591·63		
44	613·68	864·13	250·45
42	637·78		
40	663·68	916·48	252·80
38	691·33		
36	721·93	977·08	255·15
34	755·88		
32	787·38	1048·38	261·00
30	832·68		
28	877·88	1135·08	257·20
26	927·68		
24	984·38	1242·98	258·60
22	1048·33		
20	1121·13	1380·88	259·75
18	1154·13		
48	570·73		

The volume of gas is augmented, the temperature raised 1°·5 C., the apparatus left for two hours and then brought again to 7°·7 C.

48	……	816·88	246·15
24	……	1243·68	259·30
24	……	1243·38	259·00 aft. ½ h.
20	……	1381·73	260·60

Second Series.

Numbers of the divisions.	Hydrogen alone.	Gas with vapour.	Elastic force of the vapour.
60	633·63	885·28	251·65
56	672·43	926·33	253·90
52	716·13	971·43	255·30
48	766·28	1022·43	256·15
44	823·88	1081·08	257·20
40	890·48	1149·58	259·10
36	959·63	1228·63	259·10
32	1063·48	1324·08	260·60
28	1178·33	1439·98	261·65

The æther had remained in the gas for fifteen hours before the commencement of observations.

Tensions of the vapour of æther in carbonic acid gas. Temperature 7°·7 C.

Numbers of the divisions.	Gas alone.	With æther.	Elastic force of the vapour.
59	535·38	674·18	138·80
57	551·38		
56	560·28	704·68	144·40
52	596·38	749·98	153·60
48	637·98	801·40	163·52
44	685·38	860·88	175·50
40	740·38	929·48	189·10
36	805·88	1009·88	204·10
32	883·38	1105·83	222·45
28	977·58	1213·83	236·25
24	1095·68	1333·93	238·25
22	1165·78	1404·08	238·30
20	1245·98		
18	1337·93		
	A fresh quantity of æther introduced.		
59	......	769·78	234·40
56	......	799·33	239·05
52	......	839·33	242·95
48	......	882·33	244·35
44	......	930·36	244·98
40	......	986·18	245·80
36	......	1050·28	244·40
32	......	1126·53	243·15
28	......	1219·21	241·63
24	......	1333·53	237·85
22	......	1401·21	235·43

In these experiments I have noted the precise moment when the vapour began to be deposited in drops on the walls of the tube. I have constantly found that the vapour is then still far from possessing the elastic force of 260 millims., which it presents *in vacuo*. If the compression of the gas be continued, the condensed liquid becomes more abundant and the tension of the vapour increases, approaching more and more to that observed *in vacuo*. But an equality is not attained until a thick stratum of liquid has collected on the surface of the mercury. Moreover this equality is only to be observed immediately after the reduction in volume, for the tension afterwards diminishes, and this decrease continues to progress for several hours, although the temperature may be carefully maintained.

The explanation of these various facts appears to me to be very simple. When a volatile liquid in a limited space is in contact at the same time with a gas which fills this space and with the substance of which its walls are composed, it has a tendency to produce vapour until the tension of the latter shall be equal to that acquired by the vapour *in vacuo* at the same temperature.

But this vapour comes in contact with the walls of the vessel, which by their hygroscopic affinity condense a portion of it on their surface. The space is consequently below saturation as long as its walls have not condensed the quantity of vapour necessary to satisfy their attractive action upon the vapour. This quantity may be valued by the thickness of the liquid stratum. On the vertical portions of the wall the liquid stratum cannot increase indefinitely in thickness, as its weight tends to make it run towards the bottom. It will be understood, therefore, that if the stratum which remains adherent to the wall be too thin to neutralize the attractive action of the wall upon the vapour, the space must lose vapour; and if the lost vapour be not supplied with the same rapidity by the excess of liquid, the elastic force must always appear less than that observed *in vacuo*.

In this manner is explained why the greatest elastic forces corresponding with a given temperature are always observed immediately after a diminution of volume, or after refrigeration, because then the liquid which is just condensed has saturated the wall, and has not had time to run down by its own weight. This drainage gradually takes place, and the elastic force of the vapour is diminished in consequence. A similar effect cannot be produced *in vacuo*, because in this case the vaporization of the liquid is very rapid, and immediately replaces the vapour which has condensed upon the walls.

Many facts observed by physicists may also be explained in this manner. The air saturated with vapour by an abundant rain, descends below saturation soon after the cessation of the rain, because the bodies which are immersed in it deprive it of vapour by a hygroscopic action. If air be drawn by an aspirator from a space where it is saturated with vapour, and passed through a dry glass tube kept at the same temperature as the air, the walls of the tube will become covered with an abundance of drops, &c.

To sum up, I believe we may conclude from my observations in general, that Dalton's law regarding mixtures of gases and vapours may be regarded as a theoretical law, which would probably be verified most strictly if we could enclose the gas in a vessel, of which the walls might be formed of a certain thickness of the volatile fluid itself. But this law can only be very imperfectly realized in our apparatus; the hygroscopic affinity of their walls reducing the vapour to a variable tension, which is always lower than that corresponding with saturation.

PART IV.—*On the Elastic Force of the Vapours emitted by volatile Liquids mixed by mutual solution, or superposed.*

It is generally admitted in works on physics, that a mixture of several volatile substances which do not combine chemically, evolves complex vapours, of which the total elastic force in a state of saturation is equal to the sum of the tensions which would be produced by each of the liquids separately. This proposition, moreover, is nothing but a particular case of the general law known as *Dalton's law,* and which would apply to every mixture of volatile fluids, permanent gases, or vapours. This law is said to have been established by Dalton, as the consequence of direct experiments; and M. Biot, in his *Traité de Physique* (vol. i. p. 308), has described the process employed for this purpose. This process is very imperfect, and it is difficult to imagine how Dalton can have obtained exact results from it, especially in a question which requires exact measurements, like that at present under consideration.

Gay-Lussac is also said to have verified Dalton's law upon the elastic forces of mixed vapours, but I have nowhere found traces of the experiments of this celebrated physicist. M. Biot (*op. cit.* vol. i. p. 297) cites the experiments made by Gay-Lussac to determine the density possessed by the complex vapour, furnished by mixtures of variable proportions of alcohol and water, which were completely vaporized in the same space. Gay-Lussac found that the weight of the complex vapour is exactly the same as if the vapours of the liquids were isolated; and he concluded from this, that the two substances dissolved in the first instance undergo a molecular separation in acquiring the form of vapour, and that they then behave as though each of them were separate, without any mutual influence. But it must be observed, that in Gay-Lussac's experiments the vapours were not in a state of saturation, for they were at a temperature of 212° F., and under a pressure less than that of the atmosphere; and besides, they were not in contact with an excess of the liquid from which they had been produced. Gay-Lussac's experiments, as transmitted to us by M. Biot, have consequently no bearing upon the present question.

It may be said that Dalton's law has been accepted, because it appeared to be a natural consequence of the ideas already formed upon the constitution of the aëriform fluids, and which have been developed by Laplace, Poisson, and other mathematicians. Physicists have not thought it necessary to verify this law by direct experiment.

I have made my experiments only upon mixtures of two liquids and *in vacuo.* The case of three, or a greater number of liquids in the mixture, would have given too much complication to the question. I have, of course, only operated upon liquids which do not combine chemically by simple mixture. But we must commence by defining exactly what must be understood by substances which react, or do not react chemically upon each other. By substances which react chemically, we must understand those which, when mixed, give rise to a new substance, endowed with properties distinct from those of the primitive substances, and constituted according to the rules of definite proportions. When bodies are mutually soluble, they do not give rise to new compounds; nevertheless, in the phænomenon of solution, particular molecular actions take place, which distinctly modify certain individual physical properties.

I shall divide the binary mixtures of volatile substances upon which I have operated into three classes:—

First class. Binary mixtures of volatile substances which are not mutually soluble.

Second class. Binary mixtures of volatile substances which are mutually soluble in larger or smaller proportions, but which are employed in such quantities, that, after reciprocal saturation, there remain two separate liquids.

Third class. Binary mixtures of volatile substances which are mutually soluble in all proportions, and therefore always furnish a single fluid.

The process employed in these experiments is the same as that applied to the determination of the elastic force of the vapours furnished by isolated substances; the results are therefore perfectly comparable.

When the substances exercised little or no dissolving action upon each other, a single capsule was filled with nearly equal volumes of the two liquids, and then hermetically closed by the lamp. To deprive the liquids of air, they were placed separately for a few moments under the receiver of the air-pump, so as to expel the air by ebullition under a low pressure. The capsule was put into the balloon of the apparatus, and the operation was then carried on exactly as in the determination of the elastic force of the vapour of a homogeneous substance.

In other cases a much larger quantity of the two liquids was introduced into the balloon, and the air was completely expelled from the apparatus by boiling the liquids under a low pressure by the aid of the air-pump. However, these two processes gave the same results, so that they may be employed indifferently. Care was taken also, before making an observation, to keep the bath at a constant temperature for a quarter of an hour, and often longer still. This rendered it certain that a state of equilibrium was obtained.

FIRST CLASS.—*Binary mixtures of volatile substances which exercise no sensible solvent action upon each other.*

The number of binary mixtures of this class which may be experimented upon with certainty is much more restricted than might be supposed at the first glance, since one essential condition in experiments of this kind is, that the substances employed must be perfectly pure, and they must also both be sufficiently volatile to enable their individual tensions and that of their mixture to be determined with the necessary exactness. Lastly, it is probable that there are in reality no two volatile substances which are absolutely exempt from those reciprocal molecular actions which produce the phænomenon of solution. Everyone knows that water which has remained for a considerable time in contact with essential oils, which are considered insoluble, acquires a very distinct odour of these substances, without its being possible to detect the mechanical interposition of the smallest particle of essence.

The only mixtures of this class upon which I have operated are,—

The mixture of water and sulphuret of carbon.
The mixture of water and chloride of carbon, $C^2 Cl^6$.
The mixture of water and benzine.

The following tables give the results which I have obtained. With regard to each temperature at which an observation has been made, we find the elastic force of the vapour given by the mixture; the elastic forces of the vapours of each of the substances separately, taken upon the curves of which I have previously given the elements, or which I have determined directly for this purpose; and lastly, the sum of the two individual elastic forces.

Mixture of sulphuret of carbon and water.

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of water.	Elastic force of the vapour of sulphuret of carbon.	Sum of the elastic forces.	Differences.
$\overset{\circ}{8}$·85	196·81	8·48	189·2	197·7	mm 0·9
12·07	225·93	10·51	216·7	227·2	1·3
18·85	299·52	16·20	285·2	301·4	1·9
26·87	412·28	26·32	388·7	415·0	2·7
14·10	247·43	11·99	236·0	248·0	0·6
22·43	347·17	20·17	328·5	348·7	1·5
38·35	634·60	50·26	584·9	635·2	0·6
31·80	498·74	34·96	464·8	499·8	1·1

Mixture of chloride of carbon, C^2Cl^6, *and water.*

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of water.	Elastic force of the vapour of sulphuret of carbon.	Sum of the elastic forces.	Differences.
7°·79	63·49	7·90	52·5	60·4	mm 3·1
11·39	75·37	10·05	62·5	72·5	2·8
16·75	97·25	14·19	80·4	94·6	2·6
20·49	115·69	17·92	95·2	113·1	2·6
25·66	146·58	24·49	119·7	144·2	2·4
29·12	170·77	30·00	138·6	168·6	2·2
34·42	214·67	40·50	172·2	212·7	2 0
38·59	256·42	50·92	203·7	254·6	1·8
44·59	328·38	69·91	256·6	326·5	1·9
Mixture of water and benzine.					
10·10	54·92	9·23	47·0	56·2	1·3
12·38	61·93	10·72	52·4	63·1	1·2
15·26	72·34	12·91	60·5	73·4	1·1
18·01	83·00	15·36	69·2	84·6	1·6
19·88	91·49	17·26	75·7	93·0	1·5
22·53	104·28	20·80	86·0	106·3	2·0
10·53	56·03	9·49	48·2	57·7	1·7

It will be seen from these tables, that the elastic forces observed for the mixtures are very nearly equal to the sum of the elastic forces given by the separate substances. With sulphuret of carbon and benzine the tension is constantly a little weaker than the sum of the tensions. The opposite result is presented by the mixture of water and chloride of carbon. I nevertheless regard this latter fact as an anomaly, which I attribute to the circumstance, that, notwithstanding all the care which had been taken, the chloride of carbon was not absolutely pure. I have already said, that a very small quantity of a foreign substance, inappreciable by our ordinary chemical reagents, exerts a very sensible influence upon the elastic force of the vapour.

From these experiments I think we may conclude, as a theoretical law, that *two volatile substances, which are not capable of mutual solution, give a tension of vapour equal to the sum of the tensions presented by these substances separately,* although it is probable that this will never be verified absolutely upon natural substances, because there certainly exist no two substances entirely deprived of all reciprocal solvent action.

This is consequently a verification of Dalton's law; but we shall see from what follows, that this is the only case to which Dalton's law applies.

Second class.—*Binary mixtures of volatile substances which dissolve each other in larger or smaller, but not unlimited proportions.*

It is very difficult to make mixtures of substances which come in this category. I have only operated upon mixtures of æther and water. They gave the following results :—

Temperatures.	Elastic force of the mixture.	Elastic force of pure water.	Elastic force of æther.
15°·56	362·95	13·16	361·4
20·40	440·32	17·83	440·0
26·73	562·79	26·09	563·6
33·08	710·02	27·58	711·6
27·99	589·38	28·08	590·0
24·21	510·08	25·30	510·0

In this case the mixture, far from giving a vapour possessing a tension equal to the sum of the individual elastic forces of the separate substances, scarcely presents that of the æther alone.

Third Class.—*Binary mixtures of liquids mutually soluble in every proportion.*

This class includes the most numerous binary mixtures. I have operated upon three mixtures in various proportions of sulphuret of carbon and æther, two liquids which are particularly adapted to investigations of this nature, as they both possess vapours of considerable tensions at low temperatures :—

Upon two mixtures of chloride of carbon, $C^2 Cl^6$, and sulphuret of carbon ; and

Upon a mixture of benzine and alcohol.

Mixture of nearly equal weights of æther and sulphuret of carbon.

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of æther.	Elastic force of the vapour of sulphuret of carbon.
0°	182·92	182·3	126·5
−16·71	80·59	80·3	56·3
−11·36	105·26	106·0	74·0
−11·02	107·67	108·0	75·2
− 8·94	119·18	119·6	83·4
− 8·53	121·77	122·2	84·8
− 8·44	122·73	122·5	85·2
− 7·15	130·40	131·4	90·8
− 7·14	130·82	131·3	90·8
− 4·01	151·76	152·3	106·0
+ 8·93	271·38	274·0	190·2
+ 8·94	271·26	274·1	190·3
+ 8·96	270·92	274·4	190·5
+ 9·07	274·02	275·7	191·3

Mixture of 62 *volumes of sulphuret of carbon and* 38 *volumes of æther.*

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of æther.	Elastic force of the vapour of sulphuret of carbon.
4°·72	207·58	228·2	158·8
9·31	252·33	278·8	193·8
12·60	288·96	319·5	221·8
17·00	344·14	384·0	264·5
20·54	395·52	442·3	305·0
24·07	451·79	508·7	349·5
27·19	506·63	573·2	392·8
30·79	575·90	654·3	448·3
33·28	627·82	717·2	489·0
36·01	688·73	786·5	539·5
39·44	772·49	927·0	607·2
Mixture of 56 *volumes of æther and* 41 *volumes of sulphuret of carbon.*			
8·01	253·76	263·0	183·0
9·46	268·81	280·0	195·0
11·17	288·55	301·4	209·2
12·88	309·50	323·3	224·0
16·22	253·33	371·8	257·0
18·62	388·42	410·8	282·6
21·12	434·88	452·3	312·0
23·68	478·38	500·0	344·0
26·89	538·77	566·7	388·8
30·09	603·93	638·0	436·2
32·65	659·45	699·3	478·8
35·61	730·33	777·2	531·6
38·18	795·80	856·2	581·5

These tables show that mixtures of sulphuret of carbon and æther, far from giving vapours of a tension equal to the sum of the tensions of the two separate vapours, exert an elastic force which is generally less than that which would be produced by the most volatile of the two liquids. The difference increases in proportion to the volume of the less volatile liquid.

As it was my intention only to study the phænomenon in its general bearings, I did not take great care in the determination of the quantities of liquids mixed, particularly as their proportions change by the mere fact of vaporization. I have had an apparatus constructed by means of which I proposed to continue these experiments, operating with exactly known and suitably graduated mixtures, and which would allow the determination, at any moment, of the constitution of the atmosphere of vapour. Time has not hitherto permitted me to make use of it.

Mixture of equal volumes of chloride of carbon, $C^2 Cl^6$, *and sulphuret of carbon.*

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of sulphuret of carbon.	Elastic force of the vapour of chloride of carbon.
8°·75	151·24	189·3	55·4
13·32	183·32	228·6	68·7
18·84	229·15	285·0	88·3
22·81	267·53	333·3	105·8
26·44	307·41	382·2	123·7
30·64	358·81	445·4	147·3
33·78	400·47	497·6	168·0
36·53	444·34	549·5	187·3
42·35	542·21	668·8	236·0
48·43	661·95	814·0	296·2
Mixture of 60 *parts of sulphuret of carbon and* 145 *parts of chloride of carbon,* $C^2 Cl^6$.			
11°·98	143·82	216·1	64·4
21·09	206·71	311·0	98·2
35·12	349·23	522·3	177·6
41·50	436·52	649·5	228·5
The second day 18·70	188·39	283·6	87·8
The third day 13·16	149·97	227·0	68·2

The two mixtures of sulphuret and chloride of carbon consequently present the same fact that we have already seen to occur with mixtures of æther and sulphuret of carbon; the tensions of their vapours are weaker than those of the most volatile liquid, and intermediate between the tensions of the two liquids composing the mixture.

Mixture of benzine and alcohol.

Temperatures.	Elastic force of the vapour of the mixture.	Elastic force of the vapour of benzine.	Elastic force of the vapour of alcohol.
7°·22	43·17	40·4	20·1
9·98	50·22	46·8	24·2
13·11	59·66	54·4	29·2
16·05	69·43	62·7	35·0
18·59	79·35	71·0	41·0

The mixture of benzine and alcohol consequently gave vapours of greater tension than those of the most volatile liquid, which was not observed with the preceding mixtures in the proportions of which they were composed.

The preceding experiments relate to the elastic force of the vapours furnished by mixtures of volatile liquids *in the static condition*, that is to say, when the space containing the liquid and the vapour is kept at a constant temperature. It became interesting to study them in the dynamic state, that is to say, to determine the temperatures presented by the vapour and the liquid when the mixture is boiled under different pressures. My experiments have hitherto been made only upon liquid mixtures of the first class, *i. e.* upon those composed of liquids which exert no sensible solvent action upon each other, and which consequently remain superposed.

Here we must distinguish two cases; that in which the more volatile liquid forms the upper stratum, and that in which it forms the lower. It is to the latter that I have principally directed my attention, as it is for this that we may hope to obtain the most constant results; the vapour of the more volatile liquid is then compelled to traverse the less volatile supernatant liquid, and is therefore placed in favourable conditions for becoming saturated with it. Nevertheless the phænomenon appears to be very complex, because it depends upon the greater or less vivacity of the ebullition. When the ebullition is weak, its temperature is found to be that at which the sum of the tensions of the two vapours is in equilibrium with the pressure opposed to the ebullition; but if the fire be rendered more active, the ebullition becomes very violent, the temperature rises, and finally attains the point at which the more volatile liquid would boil under the same pressure if it were alone. It is probable that passages are then formed in the upper stratum of liquid, and that the vapour passing without obstacle does not sensibly carry up vapour from the less volatile liquid. These irregularities are particularly remarkable when ebullition takes place under low pressures.

PART V.—*Researches undertaken to determine whether the solid or liquid state of bodies exerts any influence upon the elastic force of the vapours which they emit at the same temperature* in vacuo.

In my preceding researches upon the elastic force of aqueous vapour, I have endeavoured to ascertain whether the solidification which water undergoes when its temperature descends below 0° C. exerts any influence upon the tension of its vapour. For this purpose I made a great number of determinations of the elastic force of the vapour emitted by ice between −32° and 0° C. I have ascertained that the curve constructed upon these experiments presented a perfect continuity with that given by the elastic forces of the vapours furnished by liquid water at temperatures above 0° C.

I have since made similar experiments upon two volatile liquids which solidified at a readily obtainable temperature, whilst still presenting a tension sufficiently strong to be measured with precision. These are hydrocarburet of bromine and benzine.

We may therefore admit, as proved by experiment, that *the molecular forces which determine the solidification of a substance exert no sensible influence upon the tension of its vapour* in vacuo.

But I attached particular interest to similar researches made upon monohydrated acetic acid. This acid is solid up to a temperature of +16° C.; but when once liquefied, its congelation presents great difficulty. It may be cooled sometimes to −8° or −10° C. without solidifying, even when the flask containing it is briskly shaken. The solidification takes place immediately when the surface of the liquid is touched with a glass point, or when a crystal of solid acid is thrown into it. Monohydrated acetic acid consequently presented me with an instance of a substance, the tensions of the vapour emitted by which, both in a solid and liquid state, might be determined through a considerable extent of the thermometric scale.

The acetic acid employed in the two first series of experiments was taken from a mass of 1 kilogramme, which to all appearance solidified completely; but, for more certainty, only the last solid fragments which remained after the liquefaction of the greater part of it, were taken.

First Series, January 12, 1844.

Temperatures.	Elastic forces.
°	mm
6·55	6·37
4·36	5·63
7·62	6·83
10·09	7·80
14·43	10·02
17·09	11·61
19·91	13·56

The acid remained liquid through all these temperatures. To make it crystallize, the water was taken out of the trough and the balloon was made to vibrate strongly, but without success. These vibrations at last fractured the mastic which established the communication with the manometer, and the experiment was obliged to be stopped.

Second series, January 18.

In this second series the balloon was surrounded by a freezing mixture to cause the congelation of the acid.

	Temperatures.	Elastic forces.		Temperatures.	Elastic forces.
	°	mm		°	mm
Liquid acid.	1·36	5·23	Liquid acid.	−0·69	4·27
	3·81	5·99		−2·40	3·90
	9·09	7·81		−5·11	3·35
	10·95	8·48			
	13·13	9·29	Solid acid.	−7·55	3·25
	14·74	10·23		−5·83	3·56
	18·23	12·34		−4·24	3·93
	7·21	6·64		−2·56	4·26
	5·52	5·97		−0·82	4·71
	8·19	7·01		−0	4·89
	10·91	8·12			
	15·92	10·87			
	3·09	5·27			
	1·53	4·83			

The solidification of the acid only took place towards −7° C. At the moment when it took place, the tension suddenly increased considerably, which must be attributed to the instantaneous disengagement of the latent heat of fusion.

If the curve of the elastic forces be constructed from these elements, it appears that the solid and liquid acid give two separate curves which probably unite at the point of fusion; the curve of the solid acid is constantly above that of the liquid acid, the tensions of which are always weaker at equal temperatures.

Third Series.

As it was feared that the acid employed in the two former series might have contained a little water, it was distilled over anhydrous phosphoric acid. But it was found that a certain quantity of acetone was then always formed. To separate this the substance was again distilled, only the last quarter of the product being collected; upon this the experiments were made.

Dates, 1844.	Temperatures.	Elastic forces.		Observations.
	°	mm		
February 1.	7·40	6·22	Liquid acid.	On the 1st of February the acid resisted a temperature of −5°·38 C. without solidifying; whilst on the 2nd of February it solidified in melting ice.
	10·33	7·28		
	12·25	8·05		
	15·18	9·39		
	18·79	11·37		
	21·57	13·26		
	12·24	8·03		
	− 5·38	3·21		
February 2.	11·09	7·52		
	8·61	6·57		
	7·01	6·06		
	5·35	5·59		
	3·03	4·96		
	0·00	3·78	Solid acid.	The experiments could not be continued further, because one of the tubes of communication was broken accidentally.
	1·23	4·02		
	3·51	4·59		
	5·52	5·18		

The solid and liquid acid still present two curves, but the curve of the solid acid is constantly below that of the liquid acid.

Fourth Series, February 3.

The fourth series was made with acetic acid distilled a second time over anhydrous phosphoric acid. The same fact was observed as in the third series; that is to say, two separate curves, that of the liquid acid having the larger ordinates.

	Temperatures.	Elastic forces.		Temperatures.	Elastic forces.
	°	mm		°	mm
Liquid acid.	7·04	5·61	Liquid acid.	1·32	3·96
	7·09	5·53		3·54	4·50
	7·17	5·57		5·77	5·14
	9·71	6·42			
	12·12	7·33	Solid acid.	0·00	3·23
	14·87	8·59		3·57	4·06
	17·23	9·85		6·92	5·08
	22·37	13·15		9·96	6·28
	25·28	15·36		11·49	6·97
	19·84	11·47		12·43	7·48
	19·84	11·44		13·14	7·86
	8·07	5·79	Liquid acid.	14·33	8·42

I think that the anomalies presented by monohydrated acetic acid may be explained by the presence of very small quantities of foreign substances. The acid obtained by simple crystallization probably contained a very small quantity of water in excess, whilst the distilled acid contained a little acetone. As long as the acid is liquid, the foreign matter is disseminated through its whole mass, and does not exert any sensible influence upon the tension of the vapour. But this is by no means the case when the acid becomes solid; then the foreign matter separates, in combination with a smaller quantity of acetic acid, and its influence upon the tension of the vapour thus becomes much more remarkable.

III. *On the Heat of Vapours.*

By Sir J. W. Lubbock, *Bart., F.R.S.*[*]

IN my Treatise on the Heat of Vapours, p. 2, I gave the following expression:

$$1+\alpha\theta'=(1+\alpha\theta)\frac{(p^{\frac{1-\gamma}{\gamma}}-E)}{(p'^{\frac{1-\gamma}{\gamma}}-E)}.$$

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[*] Communicated by the Author.

In this expression α, γ, and E are constants, θ and p are corresponding temperatures and pressures.

If $\frac{1-\gamma}{\gamma}=\beta$, and if $\frac{1}{\alpha}+\theta=\tau$, τ being the absolute temperature, the following equation is easily obtained:

$$p^{\beta}=E\left\{1+\frac{(p'^{\beta}-E)\tau'}{E\tau}\right\};$$

if p' be taken equal to unity at the boiling-point of the fluid, τ' is the absolute temperature at that boiling-point,

$$p^{\beta}=E\left\{1+\frac{(1-E)\tau'}{E\tau}\right\};$$

and if

$$\frac{(1-E)\tau'}{E}=F,$$

$$p^{\beta}=E\left\{1+\frac{F}{\tau}\right\},$$

$$\log p=\frac{1}{\beta}\log E+\frac{1}{\beta}\log\left\{1+\frac{F}{\tau}\right\}.$$

This formula resembles that given by Mr. Rankine in the last Number of this Journal, inasmuch as it expresses the logarithm of the pressure in a series proceeding according to the negative powers of τ.

In my treatise above referred to, the following numerical values are given for water:

$$E=1{\cdot}17602, \quad \beta={\cdot}0134;$$

but having gone over the work again, I find the slightly different values

$$E=1{\cdot}18028, \quad \beta={\cdot}01372;$$

$\tau=448°+$ number of degrees of Fahrenheit, $\tau'=660$, and the pressure being reckoned in inches of mercury,

$$\log p=6{\cdot}72106+[1{\cdot}8626459]\log\left\{1-\frac{[2{\cdot}0035047]}{\tau}\right\},$$

or the pressure being expressed in millimetres of mercury, and the temperatures in Centigrade degrees,

$$\log p=8{\cdot}12766+[1{\cdot}8626459]\log\left\{1-\frac{[1{\cdot}7482322]}{\tau}\right\}.$$

From the same three observations of M. Regnault that Mr. Rankine used in his paper in the Edinburgh Philosophical Journal, vol. xlvii. p. 31, to determine the constants in his formula, I found

$\beta = {\cdot}02191$,

$E = 1{\cdot}29226$ (the pressure reckoned in atmospheres),

$$\log p = 7{\cdot}96295 + [1{\cdot}6593576] \log \left\{ 1 - \frac{[1{\cdot}9272914]}{\tau} \right\};$$

the pressure being expressed in millimetres of mercury, and the temperature in Centigrade degrees, the absolute zero being supposed to be 274° Cent. below the temperature of melting ice; the quantities in square brackets throughout being the logarithms of the numbers whose places they occupy. But although this expression represents all Regnault's observations very well, it deviates in places more than Mr. Rankine's formula.

These expressions appear to have nothing in common with those given by Mr. Waterston in the Philosophical Transactions, part 1, 1852.

IV. *On the Brine-springs of Worcestershire. By* A. BEAUCHAMP NORTHCOTE, *Senior Assistant in the Royal College of Chemistry*[*].

THE deposits of rock-salt which occur in the earth's crust, and the brine-springs which at various places rise to its surface, appear to be confined to no particular series of strata, but to be distributed somewhat indiscriminately. Thus, the inexhaustible mines of Wielitzka in Gallicia, the deposits at the base of the Carpathian mountains, as well as those in many other places in central Europe, are found in the Tertiary: in some parts of Germany, again, salt is obtained from strata equivalent to our New Red Sandstone; in the Austrian Alps, an impure deposit mixed with clay and gypsum occurs in oolitic limestone; whilst in many countries, and even in England, salt-springs burst out of the carboniferous and older rocks[†].

The great English deposits, however, exist in the upper members of the New Red Sandstone, in the red and green marls of that system; which has occasioned the application of the term "saliferous" to the whole series of rocks of which it is composed; a term which Sir R. Murchison, in his 'Siluria,' shows to be incorrect, inasmuch as the middle and upper members of that group are destitute both of rock-salt and saline waters. These salt-producing strata of the New Red Sandstone lie both in Cheshire and Worcestershire in a basin of Lias: at Northwich in Cheshire, masses of rock-salt exist, 60 feet in thickness, 1400 yards in breadth, and a mile and a half long; and from a bed still lower, salt has already been removed to a depth of 110 feet[‡]:

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[*] Communicated by the Author.

[†] Murchison's Silurian System, vol. i. p. 31.

[‡] Geological Transactions, Old Series, vol. i. p. 46.

in Worcestershire, the deposits, although large, do not appear to be on so gigantic a scale; the whole depth of the rocks constituting the matrix has been estimated at upwards of 600 feet; and from some observations published by Sir Charles Hastings some years since, on the sections disclosed by the sinkings at Stoke Prior, the layers of salt in this region vary from mere veins to deposits of more than 30 feet in thickness*.

The brine-springs of Droitwich are stated by Sir C. Hastings to have been in use in the time of the Romans; but the existence of deposits of rock-salt does not appear to have been suspected until about thirty years ago, when a person acquainted with the features of the salt district of Cheshire, upon examining the geological peculiarities of the marls at Stoke Prior, a place nearly four miles to the N.E. of Droitwich, gave it as his opinion that rock-salt would be found there in sufficient quantity to make the working profitable. Shafts were accordingly sunk, and both salt and brine found: of the latter they speedily availed themselves, but I am not aware that the rock-salt has at any period been quarried, as in Cheshire; probably the extent of the deposits found on sinking the shafts did not warrant such an undertaking, or perhaps the abundance of the issuing salt-spring presented a more immediately available and less expensive source of the desired substance. At both places (Stoke and Droitwich) a shaft of considerable diameter is sunk and lined with an iron casing; in this the pumps which remove the brine and convey it to the reservoirs are fitted, and from the bottom of this upper tube a narrower pipe is carried down to a considerable depth, until, in fact, it reaches the subterranean brine chamber. The depth of the iron casing at Droitwich is 150 feet, that of the boring below 25 feet; and so abundant is the supply of brine, that if the pumps cease working, the shaft is speedily filled to within 9 feet of the surface. At Stoke the supply appears now to be equally inexhaustible, although I believe that in former years it failed; but the present proprietor has bored deeper than his predecessors, and has by means of a shaft 225 feet deep, and a boring of 348 feet more, succeeded in breaking through the drier and more superficial deposits, and has no doubt penetrated a larger and better supplied reservoir than had previously been opened.

The brine, thus pumped out of the pits as fast as it is supplied, is treated at both places in a precisely similar manner; it is conveyed by pipes directly from the pumps into large but shallow reservoirs, which act as feeders to the numerous pans in which the evaporation is carried on. These pans vary in length from 60 to 100 feet, and are about 20 feet broad and 18 inches

* Murchison's Silurian System, vol. i. p. 31.

deep: the evaporation is conducted rather rapidly in the manufacture of ordinary salt, to prevent its deposition in crystals of too large a size; in the case of bay salt, the crystallization is allowed to be slower; in the large pans, however, such is the difference of temperature in different parts of the pan, from the furnace being placed at one end, that while the fine-grained salt is produced at the hottest end, bay salt is the result of the diminished temperature at the other. The salt as it crystallizes out sinks to the bottom, and is continually removed by the workmen, who place it in tall boxes having their bottoms pierced; in these the salt is allowed to drain and settle, and is shortly afterwards removed in the form of a consolidated block and placed in the drying-room. The fine salt, which is not sent to the market in blocks, is drained and dried in conical wicker baskets, from which it is easily shaken out as a rough powder, whilst the bay salt is merely removed from the pans by the shovel.

Formerly the brine reservoir beneath Droitwich was drawn upon in several places, each proprietor sinking a shaft for himself; but by degrees these shafts have all, with one exception, fallen into the hands of a single firm, that of Messrs. Clay and Newman, who finding the supply afforded by one shaft quite sufficient for their purpose, have discontinued working the others. This brine I have examined, and the results obtained will be found below: the brine yielded by the other shaft to which I have alluded I have not analysed, since as it is sunk within 50 feet of that belonging to Messrs. Clay and Newman, the probability is that the composition of the two waters is identical. What has been said of the sinkings at Droitwich applies in a great measure also to those at Stoke Prior; at this latter place three or four shafts are in action, all within a very short distance of each other, I therefore considered it useless to examine the water from more than one, thinking them to be not sufficiently far removed from one another to furnish brines having any material difference in their composition. The water which I took at this place for the purpose of analysis was that obtained at the works of Mr. J. Corbett. It will be seen by reference to the tables which follow, that two analyses of the Droitwich spring have been made: the water for the first analysis was kindly sent me by Sir C. Hastings in the January of the present year, that for the second was taken at the same time as the Stoke brine, in August last. This second examination was made chiefly for the more accurate comparison of the two waters, but it also serves to show that the composition of this saline spring is almost invariable, the analyses having been made at the most opposite periods of the year.

It was soon found by the qualitative examination of both waters, that the only bodies existing in quantity sufficient to admit of their amount being determined, were soda, lime and magnesia, chlorine and sulphuric acid; free carbonic acid was indeed present, but it was impossible to take the brine from the pits, and as they issued from the delivery pipes in a state of foam, any attempt at the determination of this gaseous constituent was rendered useless: the existence of other substances was also ascertained, but their amount was so small as effectually to elude detection, unless a considerable quantity of water was operated upon. These waters contain 25 per cent. of solid matter; the evaporation of a comparatively small bulk of brine therefore yielded a residue which soon became cumbersome; nearly two gallons of the Droitwich brine, and rather more than one of that obtained at Stoke, were, however, evaporated. The plan of proceeding was to evaporate to a small bulk, and to test portions of the mother-liquor poured off from the deposited salts; first, for bromine and iodine, by evaporation to dryness and exhaustion with alcohol, repeating the process until a small saline residue was obtained; this was then examined in the usual manner by mixing thoroughly with starch-paste, and adding a single drop of nitric acid. Secondly and thirdly, for potassa and lithia, by the appropriate and well-known methods: the residue separated from the mother-liquor was exhausted with nitric acid, and the acid liquid examined; for iron, by converting the nitric into an acetic solution, and testing with ferrocyanide of potassium; for manganese, by Mr. W. Crum's method, the reaction of binoxide of lead; for phosphoric acid, by the action of molybdate of ammonia; and for alumina, by igniting the solid produced by evaporating the acid solution with nitrate of cobalt. The residue insoluble in nitric acid was further tested for fluorine and strontia by first submitting it to the process for producing hydrofluosilicic acid, and then examining the insoluble residue resulting from that process for strontia. Nitric acid and ammonia, arsenic, antimony, and tin were tested for in separate portions of each water.

As these brines closely resemble each other in their largely-occurring constituents, so do they also in the traces of substances which they contain. Careful examination of a portion of the mother-liquor, equivalent to half a gallon of the water, has shown in the case of each the presence of a minute quantity of bromine, but iodine appears to be totally absent: another portion of the mother-liquor, representing about a quart of brine, has in both instances yielded a distinct trace of potassa, whilst no evidence of the presence of lithia has been obtained. In the insoluble residues a distinct trace of iron has been found; manganese, however, as might have been expected when so small a quantity of iron was present, was not to be recognized. Unmistakeable evidence of the presence of phosphoric acid was obtained in both waters, upon the addition of a small quantity of the nitric acid solution to a quite colourless and nearly dry mixture of nitric acid and molybdate of ammonia. The search for fluorine and strontia has been unsuccessful; nor have any traces of nitric acid or ammonia, antimony, arsenic, or tin been discovered; but on passing sulphuretted hydrogen through portions of the brines acidified with hydrochloric acid, a slight but decided brownish tinge was observed in both cases, such as is produced by the same reagent in a solution containing about $\frac{1}{800,000}$dth of its weight of lead: this I would suggest might be due to the presence of a minute trace of chloride of silver dissolved in the chloride of sodium, since that metal has been found in rock-salt in small quantities*; so exceedingly trifling is its amount, however, that my attempts to isolate it have been fruitless. The following are the preliminary observations made upon the waters, and the direct results of their analysis:—

	Droitwich brine.		Stoke brine.
	January 1854.	August 1854.	August 1854.
Specific gravity	1·1893	1·1792	1·2044
Temp. of air .	...	76°·5 F.	75° F.
Temp. of brine	...	54°·5 F.	55° F.
Reaction . .	...	slightly alkaline.	slightlyalkaline.

Table of direct results of analysis calculated to 100 parts.

	Droitwich brine.						Stoke brine.		
	January.			August.			August.		
	I.	II.	Mean.	I.	II.	Mean.	I.	II.	Mean.
Potassa	......	......	......	......	......	trace	......	......	trace
Soda............	12·5798	12·5929	12·5864	12·1501	12·1217	12·1359	13·7804	13·7754	13·7779
Lime	·1508	·1530	·1519	·1581	·1612	·1596	·1102	·1049	·1075
Magnesia	·0196	·0204	·0200	·0167	·0159	·0163	·0187	·0143	·0165
Sesquioxide of iron ...	......	......	trace	......	......	trace	......	......	trace
Chlorine	14·1344	14·1360	14·1352	13·6167	13·6329	13·6248	15·4479	15·4916	15·4697
Bromine	......	......	trace	......	......	trace	......	......	trace
Sulphuricacid.	·4256	·4269	·4262	·4886	·4876	·4881	·4896	·4880	·4888
Phosphoric acid	......	......	......	......	......	trace	......	......	trace
Silicic acid ...	......	......	trace	......	......	trace	......	......	trace
Residue on direct evaporation...	24·2272	24·1856	24·2064	23·4205	23·4205	23·4205	26·4632	26·4866	26·4749

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* *Annales de Chimie et de Physique*, 3 sér. vol. xxviii. p. 129.

Since gypsum is an invariable constituent of rock-salt strata, it is reasonable to suppose that it dissolves as such in the brine; and as its solubility is increased by the presence of sulphate of soda and chloride of sodium*, I have ventured in the arrangement of results which follows, to calculate all the lime as sulphate, although were it not for the presence of those salts, it would be a larger per-centage than the water would dissolve.

Table of constituents calculated in 100 parts.

	Droitwich brine.		Stoke brine.
	January.	August.	August.
Chloride of sodium	23·2932	22·4521	25·4923
Bromide of sodium	trace	trace	trace
Sulphate of potassa	......	trace	trace
Sulphate of soda	·3712	·3905	·5948
Sulphate of lime	·3690	·3878	·2612
Carbonate of soda	·1382	·1157	·0161
Carbonate of magnesia	·0420	·0343	·0347
Phosphate of lime	......	trace	trace
Phosphate of sesquioxide of iron	......	trace	trace
Silica	trace	trace	trace
	24·2136	23·3804	26·3991
Solid residue by direct evaporation	24·2064	23·4205	26·4749

In the table which follows next, I have given some results obtained by Dr. Daubeny in an examination which he made of the Droitwich brine in 1830†. In his paper he states the constituents in a pint, and in that quantity was unable to detect the presence of bromine or iodine; there is no doubt, however, as he has observed, that if far larger quantities were operated upon, both these elements would be found; of the former of them, indeed, I have shown a slight but still distinctly recognizable trace to exist in about half a gallon of the brine; and there is every probability that iodine also would be found by working on a larger scale. I have also introduced the results of an analysis made by Mr. Horner in the year 1812‡; at that time there were no fewer than five pits in Droitwich from which brine was drawn. The specific gravity of the different specimens he states to vary from 1·20611 to 1·17471, and the amount of salts in a pint to range between 2289·75 and 1922·97 grains; the results calculated in his paper to the pint, I have (as also in the case of Dr. Daubeny's experiments) recalculated to the imperial gallon. It will be seen that the analysis which he made coincides more nearly with mine of the Stoke brine; but it must be remembered that at that time no shafts had been sunk at Stoke, a circumstance which might materially influence the neighbouring brine-springs. Mr. Horner also had the choice of five brines, of which he naturally selected the strongest. Considering, however, the lapse of time (more than forty years) between the two analyses, and the change which the methods of manipulation have undergone in the interim, they seem to me to bear sufficient resemblance to each other to prove that no material alteration has taken place in the composition of the brine; and among many similarities, two are worthy of remark. The temperature which he gives as that of the brine is 55° F., whilst that which I observed, it will be remembered, was 54°·5 F.; and he also states in his qualitative examination, that by the addition of ammonia to the water, a slight turbidity was occasioned, due doubtless to the presence of a small quantity of the phosphates of lime or iron (alumina being absent, and silica virtually so). This is a circumstance which I also observed, and subsequently found enough phosphoric acid to account for its occurrence.

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* Gmelin's Handbook, vol. iii. p. 202.

† Transactions of the Royal Society for 1830.

‡ Geological Transactions, Old Series, vol. ii. p. 94.

Table of constituents calculated to the gallon.

	Droitwich brine.				Stoke brine.
	Horner.	Daubeny.	January 1854.	August 1854.	August 1854.
Chloride of sodium......	21585·5	16910	19392·570	18532·886	21492·048
Chloride of magnesium.	16·8				
Sulphate of soda.........	407·0	402·5	309·097	322·334	501·539
Sulphate of lime.........	363·7	....	307·282	320·113	220·246
Carbonate of soda	....	....	115·123	95·519	13·573
Carbonate of magnesia.	....	....	33·501	28·353	29·263
Solid contents...........	... .	17460			
Total	22373·0	....	20157·573	19299·205	22256·669

In that excellent paper of Dr. Holland's on the deposits of rock-salt in Cheshire, to which I have before referred*, he states that the beds in that county have their longitudinal extension in a line drawn from the N.E. to the S.W., and that their greatest thickness is at the N.E. extremity, from whence they thin out as they approach the S.W. From what is known of the deposits in Worcestershire, it would appear that they also follow somewhat of the same method of arrangement. Stoke and Droitwich stand to each other in the relation of N.E. to S.W.; and from the fact of brine being found at both points, and that of very similar composition, there can be little doubt of a continuity of the strata from which it is produced between the two places; at the same time, the possibility of the extension of the rock-salt is, at least on one side, confined to that direction, since at Forest Hill, near Hanbury*, a place about two miles south of a line joining Stoke and Droitwich, outcroppings of Lias are found, which, as they indicate the termination of the New Red Sandstone, show also of necessity that of the rock-salt. Whether as great a similarity exists between these two great English deposits in their shape as in their direction, cannot so readily be inferred, since it is of course doubtful whether at either of the explored parts the lowest limit of the salt has hitherto been reached; yet since the level of the ground at Stoke is from 300 to 400 feet higher than that at Droitwich, and the productive salt deposits there do not appear to begin until the borings have been sunk to about the level of the former place, it would seem that in the Worcestershire salt beds no such thinning out in a south-westerly direction takes place.

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* Geological Transactions, Old Series, vol. i.

These brines doubtless spring from large subterranean reservoirs, produced probably in the first instance by the solvent action exerted on the rock-salt by spring- or rain-water. That this is so at Droitwich is clear from what Mr. Horner states as having occurred on the first sinking of a shaft at that place; he says, that after penetrating for 150 feet, the borer suddenly descended 22 inches†; the roof of the brine chamber had evidently been broken through, and the water instantly burst into the tube and gushed from the aperture with great force. This account is confirmed by observations of the same kind made at Stoke Prior; but owing to the greater elevation of the land, the brine when "tapped" rose only to within 90 feet of the surface, but its ascension in this case also was very rapid. The idea of extensive cavities filled with brine existing beneath these places, also receives additional support from the distinct sinking of the ground around the salt-works at Droitwich; at that place, *i. e.* at which the subterranean reservoir would be nearer the surface, and where consequently the intervening strata would present a lesser obstacle to the filling up by a more solid material of the space rendered vacuous by the removal of the brine. We can readily conceive the incessant action of the pumps to gain in the course of years upon the supply by which the chamber is replenished, and an occurrence of this sort is exactly that which might have been predicted.

It is thought by persons on the spot, that a stream of brine passes from Stoke to Droitwich, and is at once the source from which the supply at both places is derived; and although at first this might appear most probable, when we remember that (taking into consideration the difference in the level of the localities) the same depth has to be reached before the brine can be obtained, yet I cannot but think that one circumstance tends to show that this view is not the correct one; for if such were the case, the brine should in each instance rise to the same level in the different shafts when left undisturbed by the action of the pumps. Now under these conditions, the height to which it ascends at Droitwich is about 166 feet; whilst at Stoke, starting from very nearly the same level, the column of water rises to a height of 483 feet; this of itself is sufficient to show that no direct communication can take place between these two springs without supposing a violation of one of the first principles of hydrostatics. It might then be thought that a reference to the analyses before given might decide this question; but on comparing the constituents of the two waters, we can easily see that, taken alone, they prove nothing, since the differences are such as might readily be found in the same water when passing over slightly differing deposits. If the stream ran from Stoke to Droitwich, we might suppose that in its course it met with strata containing more sulphate of lime than it had as yet encountered, and at the same time was somewhat diluted with streams poorer in chloride of sodium; but I rather incline to think, that even from the composition of the brines, there is no reason to suppose that such a direct communication exists; their great similarity may easily be accounted for by supposing an equal similarity in the composition of the entire strata of rock-salt from which they are derived. Numerous freshwater streams occur in the higher parts of these deposits; in fact, no less than three are encountered, and have to be guarded against in the Stoke shafts, and these doubtless fed by rain-water permeate the strata in various directions, and dissolve the rock-salt with which they come in contact. Mr. Corbett informs me that the salt from which his brine is drawn is perfectly transparent, which clearly accounts for the fact of the Stoke brine containing less sulphate of lime, while the somewhat diminished strength of the Droitwich brine is probably due to the less perfect exclusion of the freshwater springs of the upper strata from the shaft which reaches the reservoir. Their striking similarity in other respects, the identity of their temperature, and of the traces of substances which they contain, point to an identity also in the composition of the deposit from which they are formed, of the solvent which renders them available, and of the manner in which they are produced.

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* Murchison's Silurian System, vol. i. p. 32.

† Geological Transactions, Old Series, vol. ii.

I cannot conclude without expressing my thanks to Sir Charles Hastings for the assistance which he has afforded me in obtaining the Droitwich water, and also to Messrs. Clay and Corbett for the valuable information which they have given me on the subject.

V. *Note on the Possible Density of the Luminiferous Medium and on the Mechanical Value of a Cubic Mile of Sunlight.* *By* Professor WILLIAM THOMSON*.

THAT there must be a medium forming a continuous material communication throughout space to the remotest visible body is a fundamental assumption in the undulatory theory of light. Whether or not this medium is (as appears to me most probable) a continuation of our own atmosphere, its existence is a fact that cannot be questioned, when the overwhelming evidence in favour of the undulatory theory is considered; and the investigation of its properties in every possible way becomes an object of the greatest interest. A first question would naturally occur, What is the absolute density of the luminiferous æther in any part of space? I am not aware of any attempt having hitherto been made to answer this question, and the present state of science does not in fact afford sufficient data. It has, however, occurred to me that we may assign an inferior limit to the density of the luminiferous medium in interplanetary space by considering the mechanical value of sunlight as deduced in preceding communications to the Royal Society from Pouillet's data on solar radiation, and Joule's mechanical equivalent of the thermal unit. Thus the value of solar radiation per second per square foot at the earth's distance from the sun, estimated at ·06 of a thermal unit Centigrade, or 83 foot-pounds, is the same as the mechanical value of sunlight in the luminiferous medium through a space of as many cubic feet as the number of linear feet of propagation of light per second. Hence the mechanical value of the whole energy, actual and potential, of the disturbance kept up in the space of a cubic foot at the earth's distance from the sun†, is $\frac{83}{192000 \times 5280}$, or $\frac{\cdot 819}{10^7}$ of a foot-pound. The mechanical value of a cubic mile of sunlight is consequently 12050 foot-pounds, equivalent to the work of one-horse power for a third of a minute. This result may give some idea of the actual amount of mechanical energy of the luminiferous motions and forces within our own atmosphere. Merely to commence the illumination of three cubic miles, requires an amount of work equal to that of a horse-power for a minute; the same amount of energy exists in that space as long as light continues to traverse it; and, if the source of light be suddenly stopped, must be emitted from it before the illumination ceases*. The matter which possesses this energy is the luminiferous medium. If, then, we knew the velocities of the vibratory motions, we might ascertain the density of the luminiferous medium; or, conversely, if we knew the density of the medium, we might determine the average velocity of the moving particles. Without any such definite knowledge, we may assign a superior limit to the velocities, and deduce an inferior limit to the quantity of matter, by considering the nature of the motions which constitute waves of light. For it appears certain that the amplitudes of the vibrations constituting radiant heat and light must be but small fractions of the wave lengths, and that the greatest velocities of the vibrating particles must be very small in comparison with the velocity of propagation of the waves. Let us consider, for instance, plane-polarized light, and let the greatest velocity of vibration be denoted by v; the distance to which a particle vibrates on each side of its position of equilibrium, by A; and the wave length, by λ. Then if V denote the velocity of propagation of light or radiant heat, we have

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* From the Transactions of the Royal Society of Edinburgh, vol. xxi. part 1; read May 1, 1854.

† The mechanical value of sunlight in any space near the sun's surface must be greater than in an equal space at the earth's distance, in the ratio of the square of the earth's distance to the square of the sun's radius, that is, in the ratio of 46,400 to 1 nearly. The mechanical value of a cubic foot of sunlight near the sun must, therefore, be about ·0038 of a foot-pound, and that of a cubic mile 560,000,000 foot-pounds.

$$\frac{v}{V}=2\pi\frac{A}{\lambda};$$

and therefore if A be a small fraction of λ, v must also be a small fraction (2π times as great) of V. The same relation holds for circularly-polarized light, since in the time during which a particle revolves once round in a circle of radius A, the wave has been propagated over a space equal to λ. Now the whole mechanical value of homogeneous plane-polarized light in any infinitely small space containing only particles sensibly in the same phase of vibration, which consists entirely of potential energy at the instants when the particles are at rest at the extremities of their excursions, partly of potential and partly of actual energy when they are moving to or from their positions of equilibrium, and wholly of actual energy when they are passing through these positions, is of constant amount, and must therefore be at every instant equal to half the mass multiplied by the square of the velocity the particles have in the last-mentioned case. But the velocity of any particle passing through its position of equilibrium is the greatest velocity of vibration, which has been denoted by v; and, therefore, if ρ denote the quantity of vibrating matter contained in a certain space, a space of unit volume for instance, the whole mechanical value of all the energy, both actual and potential, of the disturbance within that space at any time is $\frac{1}{2}\rho v^2$. The mechanical energy of circularly-polarized light at every instance is (as has been pointed out to me by Professor Stokes) half actual energy of the revolving particles and half potential energy of the distortion kept up in the luminiferous medium; and, therefore, v being now taken to denote the constant velocity of motion of each particle, double the preceding expression gives the mechanical value of the whole disturbance in a unit of volume in the present case. Hence it is clear, that for any elliptically-polarized light the mechanical value of the disturbance in a unit of volume will be between $\frac{1}{2}\rho v^2$ and ρv^2, if v still denote the greatest velocity of the vibrating particles. The mechanical value of the disturbance kept up by a number of coexisting series of waves of different periods, polarized in the same plane, is the sum of the mechanical values due to each homogeneous series separately, and the greatest velocity that can possibly be acquired by any vibrating particle is the sum of the separate velocities due to the different series. Exactly the same remark applies to coexistent series of circularly-polarized waves of different periods. Hence the mechanical value is certainly less than *half* the mass multiplied into the square of the greatest velocity acquired by a particle, when the disturbance consists in the superposition of different series of plane-polarized waves; and we may conclude, for every kind of radiation of light or heat except a series of homogeneous circularly-polarized waves, that *the mechanical value of the disturbance kept up in any space is less than the product of the mass into the square of the greatest velocity acquired by a vibrating particle in the varying phases of its motion.* How much less in such a complex radiation as that of sunlight and heat we cannot tell, because we do not know how much the velocity of a particle may mount up, perhaps even to a considerable value in comparison with the velocity of propagation, at some instant by the superposition of different motions chancing to agree; but we may be sure that the product of the mass into the square of an ordinary maximum velocity, or of the mean of a great many successive maximum velocities of a vibrating particle, cannot exceed in any great ratio the true mechanical value of the disturbance. Recurring, however, to the definite expression for the mechanical value of the disturbance in the case of homogeneous circularly-polarized light, the only case in which the velocities of all particles are constant and the same, we may define the mean velocity of vibration in any case as such a velocity that the product of its square into the mass of the vibrating particles is equal to the whole mechanical value, in actual and potential energy, of the disturbance in a certain space traversed by it; and from all we know of the mechanical theory of undulations, it seems certain that this velocity must be a very small fraction of the velocity of propagation in the most intense light or radiant heat which is propagated according to known laws. Denoting this velocity for the case of sunlight at the earth's distance from the sun by v, and calling W the mass in pounds of any volume of the luminiferous æther, we have for the mechanical value of the disturbance in the same space,

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* Similarly we find 15000 horse-power for a minute as the amount of work required to generate the energy existing in a cubic mile of light near the sun.

$$\frac{W}{g} v^2,$$

where g is the number 32·2, measuring in absolute units of force, the force of gravity on a pound. Now we found above, from observation, $\frac{83}{V}$ for the mechanical value, in foot-pounds, of a cubic foot of sunlight; and therefore the mass, in pounds, of a cubic foot of the æther, must be given by the equation,

$$W = \frac{32{\cdot}2 \times 83}{v^2 V}.$$

If we assume $v = \frac{1}{n} V$, this becomes

$$W = \frac{32{\cdot}2 \times 83}{V^3} \times n^2 = \frac{32{\cdot}2 \times 83}{(192000 \times 5280)^3} \times n^2 = \frac{n^2}{3899 \times 10^{20}};$$

and for the mass, in pounds, of a cubic mile we have

$$\frac{32{\cdot}2 \times 83}{(192000)^3} \times n^2 = \frac{n^2}{2649 \times 10^9}.$$

It is quite impossible to fix a definite limit to the ratio which v may bear to V; but it appears improbable that it could be more, for instance, than $\frac{1}{50}$, for any kind of light following the observed laws. We may conclude that probably a cubic foot of the luminiferous medium in the space traversed by the earth contains not less than $\frac{1}{1560 \times 10^{17}}$ of a pound of matter, and a cubic mile not less than $\frac{1}{1060 \times 10^6}$.

If the mean velocity of the vibrations of light within a spherical surface concentric with the sun and passing through the earth were equal to the earth's velocity—a very tolerable supposition—since this is $\frac{1}{10170}$ of the velocity of light, the whole mass of the luminiferous medium within that space would be $\frac{1}{50000}$ of the earth's mass, since the mechanical value of the light within it, being as much as the sun radiates in about eight minutes, is about $\frac{1}{13000}$ of the mechanical value of the earth's motion. As the mean velocity of the vibrations might be many times greater than has been supposed in this case, the mass of the medium might be considerably less than this; but we may be sure it is not incomparably less, not 100,000 times as small for instance. On the other hand, it is worth remarking that the preceding estimate shows that what we know of the mechanical value of light renders it in no way probable that the masses of luminiferous medium in interplanetary spaces, or all round the sun in volumes of which the linear dimensions are comparable with the dimensions of the planets' orbits, are otherwise than excessively small in comparison with the masses of the planets.

But it is also worth observing that the luminiferous medium is enormously denser than the continuation of the terrestrial atmosphere would be in interplanetary space, if rarified according to Boyle's law always, and if the earth were at rest in a space of constant temperature with an atmosphere of the actual density at its surface*. Thus the mass of air in a cubic foot of distant space several times the earth's radius off, on this hypothesis, would be $\frac{1\text{ lb.}}{442 \times 10^{345}}$; while there cannot, according to the preceding estimate, be in reality less than $\frac{1\text{ lb.}}{1560 \times 10^{17}}$, which is 9×10^{327} times as much, of matter in every cubic foot of space traversed by the earth.

VI. *On the Composition of the Felspars of the Granite of the Dublin and Wicklow Mountains.*

By the Rev. Joseph A. Galbraith, *Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Mourne Mountains of the county Down we have a well-known case of the occurrence of albite as a constituent of granite rock, that is to say, of a mineral occurring in the sixth system, whose rational formula is

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* "Newton has calculated (Princ. iii. p. 512) that a globe of ordinary density at the earth's surface, of 1 inch in diameter, if reduced to the density due to the altitude above the surface of one radius of the earth, would occupy a sphere exceeding in radius the orbit of Saturn."—(Herschel's Astronomy, Note on § 559.) It would (on the hypothesis stated in the text) we may now say occupy a sphere exceeding in radius millions of millions of times the distances of any stars of which the parallaxes have been determined. A pound of the medium, in the space traversed by the earth, cannot occupy more than the bulk of a cube 1000 miles in side. The earth itself, in moving through it, cannot displace less than 250 pounds of matter.

$$(KO, NaO)SiO^3 + Al^2O^3, 3SiO^3.$$

I mention this because there has existed a good deal of confusion as to the use of the terms orthose and albite; the former having been commonly appropriated to minerals of the felspathic family in which the alkali potash exists in excess, the latter to those in which soda preponderates. The true distinction unquestionably rests on crystallographic form, as instances are to be found of minerals included under the formula already given, occurring in the fifth system, in which soda preponderates, and to which therefore we should give the name orthose. And *vice versâ* of specimens containing potash in excess occurring in the sixth system, and which should therefore be called albite.

It was very commonly believed until lately that this case of the Mourne Mountains was a singular instance in Ireland. In the Proceedings of the Royal Irish Academy, vol. v. p. 351, I find that Sir Robert Kane announced as a fact, that the felspar of the Dublin and Wicklow granites "was almost exclusively an albitic or soda felspar, containing only in some cases a small quantity of replacing potash." This conclusion he derived from the analyses of some surface waters taken from the Three Rock Mountain in the neighbourhood of Dublin, in which he found a great preponderance of soda, but at the same time stated that "the fact was verified by a great number of analyses of specimens of granites taken from various parts of the great mass which extends from Dublin into the county Wicklow. In all the analyses made, which included both ordinary granites and elvan or granite porphyries, both potash and soda were found present; and the latter almost always so preponderant, as to lead to the conclusion that the potash should in most cases be considered to belong to the mica which the granite contained; and the felspar was almost exclusively an albitic or soda felspar, containing only in some cases a small quantity of replacing potash."

These analyses, which he stated had been made in the Museum of Irish Industry, he did not present to the Academy, nor have they ever, as far as I know, come before the public.

The statement of a fact so interesting and at the same time so novel excited much interest; and as it was suggested at the time by a very eminent authority, Dr. Apjohn, Professor of Chemistry and Mineralogy in the University of Dublin, that a direct investigation of the mineral in question would be of essential service in determining so important a geological fact, I undertook the task of making a systematic investigation of the felspars of the range of mountains which runs in a south-westerly directtion from Dublin to within a few miles of New Ross. As a first contribution I beg leave to send you the results of the analyses of seven specimens taken from different localities in these mountains, pretty evenly distributed over that part of the range which extends from Dalkey to the flanks of Lug-na-quilla, a mountain about thirty-six miles from Dublin. These analyses will, I think, not fail to engage the attention of such of your readers as feel an interest in chemico-geological investigations.

Quarries of Dalkey, County Dublin.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	64·00	1·412		4·01
Al^2O^3	18·11	0·352		1·00
MgO	0·57	0·028	0·396	1·13
CaO	trace	0·000		
KO	12·73	0·271		
NaO	3·00	0·097		
Loss by ignition	0·55			
	98·96			

Specific gravity =2·540.

Three Rock Mountain, County Dublin.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	65·40	1·444		4·19
Al^2O^3	17·71	0·344		1·00
MgO	1·77	0·088	0·420	1·22
CaO	trace	0·000		
KO	10·68	0·227		
NaO	3·26	0·105		
Loss by ignition	0·69			
	99·51			

Specific gravity = 2·562.

Lough Bray, County Dublin.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	65·44	1·444		4·04
Al^2O^3	18·36	0·357		1·00
MgO	0·00	0·000	0·388	1·08
CaO	0·80	0·038		
KO	12·34	0·262		
NaO	2·73	0·088		
Loss by ignition	0·52			
	100·19			

Specific gravity = 2·554.

Lough Dan, County Wicklow.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	65·05	1·436		4·16
Al^2O^3	17·72	0·345		1·00
MgO	trace	0·000	0·381	1·10
CaO	0·23	0·008		
KO	13·42	0·285		
NaO	2·75	0·088		
Loss by ignition	0·36			
	99·53			

Specific gravity = 2·559.

Glenmacanass, County Wicklow.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	64·19	1·417		3·96
Al^2O^3	18·39	0·358		1·00
MgO	0·34	0·017	0·379	1·05
CaO	0·70	0·025		
KO	11·39	0·242		
NaO	2·95	0·095		
Loss by ignition	0·58			
	98·54			

Specific gravity = 2·553.

Glendalough, County Wicklow.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	63·60	1·404		3·83
Al^2O^3	18·84	0·366		1·00
MgO	0·40	0·020	0·385	1·05
CaO	trace	0·000		
KO	14·33	0·305		
NaO	1·92	0·060		
Loss by ignition	0·60			
	99·69			

Specific gravity = 2·453.

Glenmalur, County Dublin.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	64·48	1·423		3·84
Al^2O^3	19·04	0·370		1·00
MgO	1·02	0·051	0·364	0·98
CaO	trace	0·000		
KO	10·74	0·228		
NaO	2·64	0·085		
Loss by ignition	0·78			
	98·70			

Specific gravity = 2·560.

I feel a great desire to see the rock analyses on which Sir Robert Kane has founded so important a statement, and sincerely hope that he will before long make them public, as I feel utterly at a loss to reconcile the statement with the results I have arrived at. The granite rock in question consists of quartz, mica and felspar: the quartz contributes no alkali, the mica is well known to contain potash in excess, the felspar I have shown contains potash in excess over soda in the ratio of 9 : 2; how, then, can a rock which is a mixture of the three be said to contain both potash and soda? "but the latter almost always so preponderant, as to lead to the conclusion that the potash should in most cases be considered to belong to the mica which the granite contained; and that the felspar was almost exclusively an albitic or soda felspar, containing only in some instances a small quantity of replacing potash."

I remain, Gentlemen,
Your obedient Servant,
JOSEPH A. GALBRAITH.

Trinity College, Dublin,
December 12, 1854.

VII. *On the Elastic Forces of Vapours of Mixtures of two Liquids.* *By* Professor MAGNUS*.

IN the communication of his interesting researches on the elastic forces of the vapours of different liquids, M. Regnault states, "It may be said that the law of Dalton has been accepted on sufferance, inasmuch as it seemed to be a natural consequence of the notions formed regarding the constitution of gaseous fluids, and which were developed by Laplace, Poisson, and other mathematicians. Physicists have not deemed it necessary to submit the law to an experimental examination."

This statement contains a complaint which is not well founded. In a memoir published in Poggendorff's *Annalen*, vol. xxxviii. p. 181, in the year 1836, "On the boiling of Mixtures of two Liquids, and the bumping of such mixtures," I have shown that the law of Dalton is not applicable to the vapours of mixed liquids; that is to say, of liquids which mutually dissolve each other by their mixture. It is there stated,—"If a liquid, æther for example, be brought into the vacuum of a barometer tube, and if, after the maximum tension of the vapour developed has been observed, another liquid, for instance alcohol, be introduced, whose vapour at the existing temperature possesses a smaller tension than that of æther; then the tension of the vapours of both liquids is less than that of the æther vapour alone, and is ever feebler the more alcohol is added, until finally, when a large quantity of the latter, liquid in comparison with the æther, is present, the tension almost corresponds with that of the vapour of alcohol alone at the existing temperature. The same is the case if, instead of alcohol, oil of turpentine, or instead of æther, bisulphuret of carbon be used; or if alcohol be first introduced into the barometer tube and water added afterwards."

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* From Poggendorff's *Annalen*; communicated by the Author.

From these experiments it follows, that the elastic force of the vapours of a mixture of two liquids which mutually dissolve each other is smaller than the sum of the elastic forces of the component vapours at the same temperature; and that the elastic force of the vapours of a mixture is dependent on the proportions in which the liquids which produce them are present.

As regards the deportment of the vapours of a mixture of two liquids which do not mutually dissolve each other, but form separate strata, I also made experiments at the period referred to, and found, in their case, the elastic force of the vapours of the mixture to be equal to the sum of the elastic forces of the vapours of both liquids taken separately at the same temperature. That with such mixtures, therefore, the law of Dalton finds full application.

In regard to the boiling of such mixtures, I found that two cases are to be distinguished from each other, according as the more volatile liquid constitutes the upper or the under layer. When the more volatile liquid was uppermost, boiling took place within the latter alone, and at the temperature at which the liquid would have boiled under the existing pressure, had it alone been contained in the vessel. If, on the contrary, the more volatile liquid were below, the temperature of the boiling mixture was always somewhat higher than that at which the more volatile liquid would have boiled if alone. The vapours, on the contrary, which developed themselves from this mixture always possessed a temperature, which was not only lower than that of the liquid mixture, but also lower than that at which the more volatile liquid would have boiled under the existing pressure.

In a mixture, for example, of fresh rectified oil of turpentine and water, which boiled under the pressure of 749·6 millims., the thermometer in the liquid showed a temperature of 102° C., and in the vapour 94°·5 C. In like manner, in a mixture of bisulphuret of carbon and water, which boiled under a pressure of 752·2 millims., the thermometer in the liquid stood at 47° C., and that in the vapour at 43°·5 C.

This difference in the temperatures of the vapour and the liquid is not mentioned by M. Regnault. It is, however, an immediate consequence of the law of Dalton. For the lower liquid, which is the most volatile, must, in order to boil, possess a temperature at which the elastic force of its vapour is capable of overcoming the pressure, not only of the air, but also that of the liquid above. This temperature is therefore a little higher than that which it would be if the liquid boiled under the pressure of the air alone. Further, as the bubbles of vapour from the lower liquid pass through the upper one, the temperature of the latter is nearly the same. But while the bubbles from below pass through the upper liquid, they carry vapour from the latter along with them, so that the escaping bubbles are formed of a mixture of the vapour of both liquids. Without doubt these bubbles at the moment of their escape from the liquid have the same temperature as the latter. But as the elastic force of the vapour of the more volatile liquid would alone be able to overcome the atmospheric pressure, the elastic force of the vapours of both liquids together is greater than the pressure of the atmosphere; hence these vapours expand until the sum of their tensions is just sufficient to overcome the pressure of the atmosphere, and by expansion are brought to the lower temperature observed.

With the exception of this latter phænomenon, namely, that the temperature of the vapour escaping from a mixture of two liquids which do not mutually act upon each other, is less than that of the liquid mixture, which phænomenon is not mentioned by M. Regnault, my results are corroborated by him. They were, however, made public by me eighteen years ago, and since that time it has been known to what extent the law of Dalton is applicable, and the deportment of the vapours of mixtures of two liquids has been understood.

VIII. *On some Extensions of Quaternions.* *By* Sir William Rowan Hamilton, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the French Institute, Hon. or Corr. Member of several other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from vol. viii. p. 269.]

Section IV.

[26.] FOR quines, the equations of condition between the 24 symbols $l_1 \ldots u_3$ amount (as has been already remarked) to 80 in all; namely to 8, 24, 12, 24, and 12 equations, included respectively in the five types last mentioned, and sufficiently developed above, by the formulæ (117) (118) (143) (145) (148) (152) (153) (155): which also enable us, with the help of (141) (144), to determine the values of the four symbols

$a_1 \,..\, a_4$, and of the six other symbols $b_1 \,..\, c_3$, when values of $l_1 \,..\, u_3$ have been found, which satisfy the eighty conditions. And then, if we denote the quine itself by the following expression (compare [1.]),

$$\mathrm{P} = x_0 + \iota_1 x_1 + \iota_2 x_2 + \iota_3 x_3 + \iota_4 x_4, \quad . \quad . \quad . \quad (158)$$

which is a little more symmetric than the form (111), the *laws of multiplication of any two such quines*, P, P′, will be sufficiently expressed by the formulæ

$$\left.\begin{array}{l} \iota_1^2 = a_1, \quad \iota_4^2 = a_4, \quad \mathrm{S}\iota_2\iota_3 = b_1, \quad \mathrm{S}\iota_1\iota_4 = c_1, \\ \mathrm{V}\iota_2\iota_3 = \iota_1 l_1 + \iota_2 m_3 + \iota_3 n_2 + \iota_4 p_1, \\ \mathrm{V}\iota_1\iota_4 = \iota_1 r_1 + \iota_2 s_1 + \iota_3 t_1 + \iota_4 u_1\,; \end{array}\right\} \quad . \quad (159)$$

if we remember that 1, 2, 3 may still be cyclically permuted, and that the law of conjugation (32) gives

$$\mathrm{K}\iota'\iota = \iota\iota', \quad \mathrm{S}\iota'\iota = \mathrm{S}\iota\iota', \quad \mathrm{V}\iota'\iota = -\mathrm{V}\iota\iota'. \quad . \quad . \quad (160)$$

For in this manner, by (41), if ϖ denote, as in (14), the vector part of P, so that

$$\varpi = \iota_1 x_1 + \iota_2 x_2 + \iota_3 x_3 + \iota_4 x_4, \quad . \quad . \quad . \quad . \quad (161)$$

we shall have

$$\begin{aligned} \mathrm{S}\varpi\varpi' &= a_1 x_1 x'_1 + a_2 x_2 x'_2 + a_3 x_3 x'_3 + a_4 x_4 x'_4 \\ &\quad + b_1(x_2 x'_3 + x_3 x'_2) + \&\text{c.} + c_1(x_1 x'_4 + x_4 x_1') + \&\text{c.}, \quad (162) \\ \mathrm{V}\varpi\varpi' &= (\iota_1 l_1 + \iota_2 m_3 + \iota_3 n_2 + \iota_4 p_1)(x_2 x'_3 - x_3 x_2') + \&\text{c.} \\ &\quad + (\iota_1 r_1 + \iota_2 s_1 + \iota_3 t_1 + \iota_4 u_1)(x_1 x'_4 - x_4 x'_1) + \&\text{c.}, \quad . \quad (163) \end{aligned}$$

each "&c." representing terms obtained by the permutations already mentioned; and if the constants *abclmnprstu* have been chosen so as to fulfill the conditions above developed, we may then conclude (compare (51)) that the following EQUATIONS OF ASSOCIATION hold good, for the multiplication of any *three* such *vector-units* ι, or *quadrinomial vectors* ϖ, or *quinquinomial expressions* P, whether equal or unequal among themselves:

$$\iota \,.\, \iota'\iota'' = \iota\iota' \,.\, \iota''\,; \quad \varpi \,.\, \varpi'\varpi'' = \varpi\varpi' \,.\, \varpi''\,; \quad \mathrm{P}\,.\,\mathrm{P}'\mathrm{P}'' = \mathrm{P}\mathrm{P}'\,.\,\mathrm{P}''\,; \quad (164)$$

which it has been the main object of our recent investigations to establish.

[27.] Without pretending to do more, on the present occasion, than merely to *exemplify the possibility* of *satisfying*, for *quines*, the foregoing equations of association, I may here remark that if we restrict the question by assuming (with the usual permutations),

$$(\mathrm{A}, \mathrm{B})^{*} \ldots \qquad n_1 = m_1, \quad p_1 = 0, \quad u_1 = 0, \quad . \quad (165)$$

---

* This line is lettered thus, because it contains the conditions common to the two systems (A) and (B) of associative quines, which are deduced a little further on.

then numerous simplifications take place, and the 80 equations between the 24 symbols *lmnprstu* are found to reduce themselves to 44 equations between the 15 symbols *lmrst*, obtained from the five types I. to V. of recent paragraphs, which may be thus denoted and arranged:

from type I. . . $m_1^2=l_2l_3$, $r_2^2-r_1^2=s_3t_1-s_2t_3$; . (166)
from II. and III. . . $m_1l_1=m_2m_3$, $m_1r_1=m_2s_1=m_3t_1$, . (167)
and $m_1r_2=l_2s_2$, $m_1r_3=l_3t_3$, $m_1(r_1+r_2+r_3)=0$; (168)
from IV. . . $s_2(r_2+r_3)=-t_1t_2$, $t_3(r_2+r_3)=-s_3s_1$, (169)
and $m_1s_1=l_2t_1$, $m_1t_1=l_3s_1$; . . . . . . (170)
and from V. . . $m_1s_2=l_3r_2$, $m_1t_3=l_2r_3$, $l_1(r_1+r_2+r_3)=0$. (171)

Now these conditions may all be satisfied in each of two principal ways, conducting to *two distinct systems of associative quines*, which may be called Systems (A) and (B), but which are *not the only possible systems* of such quines, because we *need* not have commenced by assuming the equations (165), although that assumption has simplified the problem. For first we may suppose that the constants l and m are different from zero, but that the constants r are connected by the relation

(A) . . $r_1+r_2+r_3=0$; . . . . . . (172)

or secondly, we may reject this relation between the constants r, and suppose instead that the six constants l and m all vanish, so that

(B) . . $l_1=l_2=l_3=m_1=m_2=m_3=0$. . . . (173)

With the first supposition, (172), we are to combine the nine relations between the fifteen constants *lmrst*, which are sufficiently expressed by the formula (167), or by the following:

(A$_1$) . . $l_1=m_1^{-1}m_2m_3$, $s_1=m_2^{-1}m_1r_1$, $t_1=m_3^{-1}m_1r_1$; (174)

and then all the other conditions of association will be found to be satisfied, if we equate each of the ten symbols *abc* to zero, or if we establish this other formula,

(A$_2$) . . $a_1=0$, $b_1=0$, $c_1=0$, $a_4=0$: . . . . (175)

while there will still remain *five arbitrary constants* of the system, for instance $r_1r_2m_1m_2m_3$. With the second supposition, (173), we are to combine four distinct relations between the nine constants *rst*, contained in the formula (169), or in the following:

(B$_1$)* . . $r_1+r_2=-s_1^{-1}t_1t_3$, $s_1s_2s_3=t_1t_2t_3$; . . . (176)

---

* It must be observed that these equations (176), which are part of the *basis* of the system (B), are *true* in the system (A) also, as corollaries from (174) and (172), which last equation does not hold in (B); and which allows us to reduce, for (A) but not for (B), the relation (177) to the simpler form $r_1r_2r_3=s_1s_2s_3$.

which give also, as a consequence, this other relation:

$$(r_1+r_2)(r_2+r_3)(r_3+r_1)=-s_1s_2s_3; \quad . \quad . \quad (177)$$

and then the other conditions of association will all be satisfied, if we make, instead of (175),

$$(\mathrm{B}_2) \ldots \quad a_1=b_1=c_1=0, \quad a_4=(r_1+r_2+r_3)^2: \quad . \quad . \quad (178)$$

this system also involving five arbitrary constants, for example $s_1s_2t_1t_2t_3$. The assertion respecting *quines,* which was made near the end of [16.], has therefore been fully justified.

[28.] Finally, as regards the system (A) of quines, it may be observed,—1st, that *in this system,* by (162) and (175), we have generally,

$$(\mathrm{A}_3) \ldots \qquad \mathrm{S}\varpi\varpi'=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (179)$$

or that "the product of any two quadrinomial vectors ϖ, ϖ', reduces itself to a pure vector;" and 2nd, that, by (163) (165), "this vector product, $\varpi\varpi'$, is of trinomial form, involving no part with ι_4 for a factor." This product is therefore already seen to be of the form

$$\varpi\varpi'=\iota_1\mathrm{X}_1+\iota_2\mathrm{X}_2+\iota_3\mathrm{X}_3; \quad . \quad . \quad . \quad . \quad (180)$$

but I say, 3rd, that "its three coefficients, or *coordinates,* X_1, X_2, X_3 have constant ratios," or that "the product $\varpi\varpi'$ may be constructed by a right line in space of which the direction though not the length is fixed," and which may therefore be conceived to "coincide in position with one fixed *axis* (ξ) *of the system.*" In fact, by (163) (165) (174), we have

$$m_1\mathrm{X}_1=m_2\mathrm{X}_2=m_3\mathrm{X}_3=\mathrm{X}, \quad . \quad . \quad . \quad . \quad (181)$$

and therefore

$$(\mathrm{A}_4) \ldots \qquad \varpi\varpi'=\mathrm{X}\xi, \quad . \quad . \quad . \quad . \quad . \quad . \quad (182)$$

if we make for abridgement

$$(\mathrm{A}_5) \ldots \begin{cases} \mathrm{X}=m_2m_3(x_2x'_3-x_3x'_2)+m_3m_1(x_3x'_1-x_1x'_3)+m_1m_2 \\ \qquad\qquad (x_1x'_2-x_2x'_1) \\ \quad +m_1r_1(x_1x'_4-x_4x'_1)+m_2r_2(x_2x'_4-x_4x'_2)+m_3r_3 \\ \qquad\qquad (x_3x'_4-x_4x'_3), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (183) \\ \text{and} \qquad \xi=m_1^{-1}\iota_1+m_2^{-1}\iota_2+m_3^{-1}\iota_3 \quad . \quad . \quad . \quad . \quad (184) \end{cases}$$

In the 4th place, "if any quadrinomial vector ϖ be multiplied by or into the axis ξ, the product vanishes;" or in symbols,

$$(\mathrm{A}_6) \ldots \qquad \xi\varpi=0, \quad \varpi\xi=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (185)$$

because by (172) the scalar coefficient X becomes $=0$, if we change either x_1, x_2, x_3, and x_4, or x'_1, x'_2, x'_3, and x'_4, to m_1^{-1},

m_2^{-1}, m_3^{-1}, and 0, respectively. This coefficient X vanishes also, when we equate $x'_1x'_2x'_3x'_4$ to $x_1x_2x_3x_4$ respectively; and hence, or from (179), we may infer, 5th, that "in this system of quines (A), the square of every quadrinomial vector vanishes." And finally, by an easy combination of the formulæ (182) (185), or of the 3rd and 4th of the foregoing properties of this system, we see, 6th, that, in it, "every product of three quadrinomial vectors vanishes;" or that

$$(\mathrm{A}_7) \ldots \qquad \varpi\varpi' \,.\, \varpi'' = 0, \quad \varpi \,.\, \varpi'\varpi'' = 0. \quad . \quad . \quad . \quad . \quad . \quad (186)$$

[29.] The *associative property* (164) is therefore verified for the system (A), by showing that, *in it*, each of the two ternary products of vectors, which ought to be equal, vanishes. In the system (B), it is easy to see that any such ternary product must be itself a vector; because, in (B), no binary product of vectors involves ι_4, nor does any such product involve a scalar part, except what arises from ι_4^2. We have, therefore, *here*, this new result,

$$(\mathrm{B}_3) \ldots \qquad \mathrm{S}(\varpi\varpi' \,.\, \varpi'') = \mathrm{S}(\varpi \,.\, \varpi'\varpi'') = 0. \quad . \quad . \quad . \quad . \quad (187)$$

And when we proceed to develope these two ternary products, the associative property of multiplication is again found to be verified, under the form,

$$(\mathrm{B}_4) \ldots \varpi\varpi' . \varpi'' = \varpi . \varpi'\varpi'' = a_4(\varpi x'_4 x''_4 - \varpi' x''_4 x_4 + \varpi'' x_4 x'_4); \, (188)$$

where it is worth observing that, by the laws of the system in question, the result may be put under this other and somewhat simpler form:

$$(\mathrm{B}_5) \ldots \varpi\varpi' . \varpi'' = \varpi . \varpi'\varpi'' = \varpi\mathrm{S}\varpi'\varpi'' - \varpi'\mathrm{S}\varpi''\varpi + \varpi''\mathrm{S}\varpi\varpi'. \, (189)$$

Indeed, this last expression might have been *foreseen*, as a consequence from the *general principles* of this whole theory of *associative polynomes**, combined with the particular property (187) of the quines (B). For, by that property, each of the two ternary products is equal to its own vector part; but by (101) we have, *generally*, in the present theory, as in the calculus of quaternions, the following expression for the *vector part of the product of any three vectors*, of any such associative polynomes as we are considering:

$$\mathrm{V} \,.\, \rho\sigma\tau = \rho\mathrm{S}\sigma\tau - \sigma\mathrm{S}\tau\rho + \tau\mathrm{S}\rho\sigma; \quad . \quad . \quad . \quad . \quad (190)$$

---

* It will hereafter be proved *generally* that for *all* associative polynomes which satisfy the law of conjugation (though *not exclusively* for such *associative* polynomes), the *tensor*, as defined in [6.], is also a *modulus*; which theorem can be verified without difficulty for the quines (A) and (B), and for the quadrinomes and tetrads so lettered in [13.], as well as for the trinomes [11.].

which is a formula of continual application in Quaternions*, and in these extensions also is important.

Observatory of Trinity College, Dublin,
August 16, 1854.

[This little paper, which I propose to call (as above) *Section the Fourth* of these Extensions, has been in type for a couple of months, but other occupations have prevented me from revising it till now. Although very unwilling to encroach on the valuable space of the Philosophical Magazine, I have drawn up sketches of some continuations, which I shall perhaps shortly copy and send, to be printed at such times as may suit the convenience of the Editors, whose constant courtesy is hereby thankfully acknowledged.—W. R. H.

Observatory, Dec. 22, 1854.]

[To be continued.]

IX. *On a case of disturbed Elliptic Motion.* *By the* Rev. A. Thacker.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

You may perhaps find a place in your Journal for the following investigation, which was suggested by a recent discussion on the subject of the lunar theory. The problem I propose to solve may be stated thus:—A body is acted on by a central force equal to $\frac{\mu}{r^2}+\mu' r$, μ' being small; to determine the motion *when the orbit is not nearly circular.*

The equations of motion are

$$\frac{d^2x}{dt^2}+\left(\frac{\mu}{r^2}+\mu' r\right)\frac{x}{r}=0,\qquad \frac{d^2y}{dt^2}+\left(\frac{\mu}{r^2}+\mu' r\right)\frac{y}{r}=0;$$

and from these we deduce immediately,

$$\frac{d\theta}{dt}=\frac{h}{r^2}, \quad \ldots \ldots \ldots \quad (1)$$

$$v^2+\mathrm{C}=\frac{2\mu}{r}-\mu' r^2, \quad \ldots \ldots \ldots \quad (2)$$

$$\left(\frac{dr}{dt}\right)^2+\mathrm{C}+\frac{h^2}{r^2}-\frac{2\mu}{r}+\mu' r^2=0\dagger. \quad \ldots \quad (3)$$

---

* Towards the close of Section III., namely of the communication which appeared in October, 1854, an accent was inadvertently omitted in the last term of the important formula (67), $\mathrm{V}\iota\mathrm{V}\iota'\iota''=\iota''\mathrm{S}\iota\iota'-\iota'\mathrm{S}\iota''\iota$, which had been correctly printed in Section II., for August, 1854. [Section I. was published in the Supplement for July, 1854.] Also in the first formula (116), of par. [17.], for a_1 read a_4. And in (148), of [22.], for s_2r_3 read s_2n_3.

† This is the equation given by Professor Challis.

Since the force is central, there cannot be more than two apsidal distances. Let them be α and β, the former being the greater. For each of these values of r, $\frac{dr}{dt}$ must be zero. We have, therefore,

$$0=-C\alpha^2-h^2+2\mu\alpha-\mu'\alpha^4,$$
$$0=-C\beta^2-h^2+2\mu\beta-\mu'\beta^4;$$

whence

$$C=\frac{2\mu}{\alpha+\beta}-\mu'(\alpha^2+\beta^2),$$
$$h^2=\frac{2\mu\alpha\beta}{\alpha+\beta}+\mu'\alpha^2\beta^2.$$

If these values be substituted for C and h^2, it will be found that

$$-Cr^2-h^2+2\mu r-\mu' r^4=(\alpha-r)(r-\beta)\left\{\frac{2\mu}{\alpha+\beta}+\mu'(r+\alpha)(r+\beta)\right\},$$

whence

$$\frac{dt}{dr}=\pm\frac{r}{\sqrt{(\alpha-r)(r-\beta)\left\{\frac{2\mu}{\alpha+\beta}+\mu'(r+\alpha)(r+\beta)\right\}}},$$

the upper or lower sign being taken according as the body is moving from or towards the nearer apse. If the expression $\left\{\frac{2\mu}{\alpha+\beta}+\mu'(r+\alpha)(r+\beta)\right\}^{-\frac{1}{2}}$ be expanded in a series ascending by powers of μ', the value of t in terms of r may be found by tegration to any required degree of accuracy. Neglecting powers of μ' above the first, putting k for $\frac{\mu'}{\mu}\cdot\frac{\alpha+\beta}{2}$, and U for $(\alpha-r)(r-\beta)$, we have

$$\sqrt{\frac{2\mu}{\alpha+\beta}}\cdot\frac{dt}{dr}=\frac{r}{\sqrt{U}}-\frac{1}{2}k\cdot\frac{r^3+(\alpha+\beta)r^2+\alpha\beta r}{\sqrt{U}}.$$

Integrating, and supposing t to begin when the body is at the nearer apse, we obtain

$$\sqrt{\frac{2\mu}{\alpha+\beta}}.t=\frac{1}{2}(\alpha+\beta)\cos^{-1}\frac{\alpha+\beta-2r}{\alpha-\beta}-\sqrt{U}$$
$$-\frac{1}{32}k(\alpha+\beta)(11\alpha^2+10\alpha\beta+11\beta^2)\cos^{-1}\frac{\alpha+\beta-2r}{\alpha-\beta}$$
$$+\frac{1}{48}k\{8r^2+22(\alpha+\beta)r+33\alpha^2+74\alpha\beta+33\beta^2\}\sqrt{U}.$$

If we make $\cos^{-1}\frac{\alpha+\beta-2r}{\alpha-\beta}=u$, it follows that

$$r=\frac{1}{2}(\alpha+\beta)-\frac{1}{2}(\alpha-\beta)\cos u;\quad . \quad . \quad . \quad \text{(A)}$$

and, eliminating r and restoring the value of k, we have

$$\left.\begin{aligned}\frac{2\sqrt{2\mu}}{(\alpha+\beta)^{\frac{3}{2}}}.t=u-\frac{\alpha-\beta}{\alpha+\beta}\sin u-\frac{\mu'}{\mu}\cdot\frac{\alpha+\beta}{32}.(11\alpha^2+10\alpha\beta+11\beta^2)u\\ +\frac{\mu'}{\mu}\cdot\frac{\alpha-\beta}{192}\{(93\alpha^2+198\alpha\beta+93\beta^2)\sin u-15(\alpha^2-\beta^2)\sin 2u\\ +(\alpha-\beta)^2\sin 3u\}.\end{aligned}\right\}.\quad(\mathrm{B})$$

Again,

$$\frac{d\theta}{dr}=\frac{h}{r}\cdot\frac{1}{\sqrt{(\alpha-r)(r-\beta)\left\{\frac{2\mu}{\alpha+\beta}+\mu'(r+\alpha)(r+\beta)\right\}}}.$$

Integrating this in the same way, and to the same degree of approximation, we shall have

$$\theta=\cos^{-1}\frac{2\alpha\beta-(\alpha+\beta)r}{(\alpha-\beta)r}-\frac{1}{2}k\alpha\beta\left\{-\sqrt{\mathrm{U}}+\frac{3}{2}(\alpha+\beta)\cos^{-1}\frac{\alpha+\beta-2r}{\alpha-\beta}\right\}.$$

Let $\cos^{-1}\frac{2\alpha\beta-(\alpha+\beta)r}{(\alpha-\beta)r}=v$, and $\cos^{-1}\frac{\alpha+\beta-2r}{\alpha-\beta}=u$; then it is easily proved that

$$\tan\frac{u}{2}=\sqrt{\frac{\beta}{\alpha}}\tan\frac{v}{2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{C})$$

and, consequently,

$$\theta=v-\frac{1}{8}\cdot\frac{\mu'}{\mu}\sqrt{\alpha\beta}\{3(\alpha+\beta)^2u-(\alpha^2-\beta^2)\sin u\}. \quad . \quad (\mathrm{D})$$

The equations (A), (B), (C), (D) determine the motion; and as there is no limitation with regard to the relative magnitudes of α and β, it follows that those equations are true, however much the orbit differs from a circle.

The apsidal angle is the value of θ corresponding to $r=\alpha$, and is therefore equal to $\pi\left\{1-\frac{3}{8}\cdot\frac{\mu'}{\mu}\sqrt{\alpha\beta}(\alpha+\beta)^2\right\}$, or

$$\pi\left(1-\frac{3\mu'}{n^2}\cdot\frac{\sqrt{\alpha\beta}}{\alpha+\beta}\right),$$

if n be the mean angular motion of the body.

In the lunar theory, the expression for the radial disturbing force contains a term $-\frac{1}{2}n'^2r$, where n' is the mean motion of the earth, and the effect of this on the moon's motion may be found by substituting $-\frac{1}{2}n'^2$ for μ' in the preceding equations. For example, if we denote by m the ratio of the earth's mean motion to that of the moon, the apsidal angle becomes

$$\pi\left(1+\frac{3}{2}m^2\cdot\frac{\sqrt{\alpha\beta}}{\alpha+\beta}\right),$$

or

$$\pi\left(1+\frac{3}{4}m^2\sqrt{1-e^2}\right),$$

if $\alpha=a(1+e)$ and $\beta=a(1-e)$.

On referring to Pontécoulant (vol. iv. p. 566), it will be seen that the value of $1-c$, which measures the rate of progression of the moon's perigee, involves the terms

$$\frac{3}{4}m^2-\frac{3}{8}m^2e^2-\frac{3}{32}m^2e^4,$$

which are identical with the first three terms of the expansion of $\frac{3}{4}m^2\sqrt{1-e^2}$.

I will only add, that if the approximation be carried a step further, the ratio of the apsidal angle to π will be found to be

$$1-\frac{3}{8}\cdot\frac{\mu'}{\mu}(\alpha+\beta)^3\sqrt{\alpha\beta}\left\{1+\frac{1}{64}\cdot\frac{\mu'}{\mu}(\alpha+\beta)(25\alpha^2+54\alpha\beta+25\beta^2)\right\}.$$

I am, Gentlemen,
Your obedient Servant,
Trinity College, Cambridge. A. Thacker.

X. *Notices respecting New Books.*

On the Atmospheric Changes which produce Rain and Wind, and the Fluctuations of the Barometer. Second Edition, with additional Essays and Diagrams. By Thomas Hopkins, *M.B.M.S.* London: John Weale. 1854.

THE chief feature of Mr. Hopkins's Essay is the importance he attaches to one fact, which he affirms has not hitherto been sufficiently attended to; and this is, the conveyance of heat to different regions of the earth's surface by *vapour*, and the effects of its liberation when the vapour is condensed. He denies the competency of the commonly received theories of the trade-winds, &c., and maintains that the principal cause of these and other similar phænomena is to be found in the fact above mentioned. After referring to the common theory of the trade-winds, originally advanced by Hadley, and now generally adopted, he says, "That the effects of the unequal heating power of the sun on the surface of the globe, and of the varying rotatory velocities of the different parts of the earth between the equator and the poles, must be of the nature described in this theory, is sufficiently evident. But it is not equally evident that the causes pointed out are adequate to the production of those general winds which are known to exist. The difference in the rotatory velocities of different latitudes must certainly have the kind of effect described upon any winds which pass from the equator to the poles, and from the poles to the equator; and the degree of effect will be proportioned to the rapidity of the passage of the wind from one latitude to another. But the unequal heating of the surface of the globe, and consequently of the air near it, by the sun, does not, as will be hereafter shown more fully, produce those palpable or strong winds which blow in some parts towards, and in other parts from, the tropics. The heating of the surface of the globe and of the air near it, by the sun, is not a sufficiently powerful cause to produce these winds, though, like the law of diffusion, such surface-heating may, and no doubt does, to a certain extent, bring into active operation another and a more powerful cause, and as we shall see hereafter, one that is fully adequate to their production." This other "and more powerful cause" is the heat taken up in the process of evaporation, and after being carried away to other regions, liberated on the condensation of this vapour. "In the following pages," says the author, "it is proposed to show that the heat which is thus taken up by vapour, in the process of evaporation, is carried away to various parts of the atmospheric regions, and in those parts is liberated on the vapour being condensed; and that it is this liberated heat which produces that inequality of temperature which causes the greater part of those aërial movements called winds on different parts of the surface of the globe." (P. 6.)

On a subject so intricate, and of which so little is known as the present one, it is dangerous to make any positive assertions involving the general laws of the phænomena. And still more dangerous is it to assert that certain causes are, or are not, sufficient to account for certain effects, whilst neither the causes nor the effects have yet been submitted to accurate measurement and calculation. We agree with Mr. Hopkins in thinking the common theory of the trade-winds unsatisfactory in some respects; and for this very reason, viz. that we possess no accurate estimates of the elements involved in the problem. Mr. Hopkins asserts, that, "if no other cause of rain or wind existed than those yet named" (viz. in the ordinary theories), "there would be no copious rain nor strong wind in any part of the world, *as those causes would evidently all operate mildly and gently*. Inequality of temperature, on different parts of the earth's surface, in no place directly produces a strong wind, though there are parts where that inequality is great within small distances." (P. 11.) Now here, we think, Mr. Hopkins has made an assertion difficult of proof; and, further, that his own theory is open to the very same objection, viz. that the action of the heat of vapours, pointed out by him as the most *powerful* cause of winds, &c., would seem at first sight to be just as likely to "operate mildly and gently" as the inequalities of temperature, &c.

His reasonings from page 14 to page 18 are rather obscure, and, as relating to operations of which we have little or no experimental knowledge, cannot be considered satisfactory; and as these pages appear to contain the main part of our author's theories, we fear that the whole work is open to the same remark. Still, it is highly probable that the heat of vapour plays a more important part in meteorological phænomena than has hitherto been generally supposed, and Mr. Hopkins has done a real service to the science by calling attention to this point. If he has attributed rather too much to this particular cause, it is only what is generally done, and might naturally be expected, whenever a person discovers the importance of some cause hitherto neglected.

In treating of other points, such as the fluctuations of the barometer, dew, &c., our author does not appear to have availed himself sufficiently of the recent researches of Dove, Melloni, and others. The subject of 'dew' in particular has received the greatest accession of illustration and explanation by the labours of Melloni, to which Mr. Hopkins makes no reference whatever. We also meet with some expressions in the course of the work which indicate rather loose notions of elementary scientific principles; as, for instance, at page 72, where the author speaks of a current being "*forced up*" the sides of elevated land "by its *inertia*."

Upon the whole, however, the work is written in a scientific spirit, and indicates considerable care and labour in the collection of facts. Moreover, it is eminently *suggestive*, and therefore likely to be of still greater service by leading to further inquiries.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. viii. p. 553.]

June 15, 1854.—The Earl of Rosse, President, in the Chair.

THE following papers were read:—

"On the Structure of certain Microscopic Test-objects, and their Action on the Transmitted Rays of Light." By Charles Brooke, M.A., F.R.S.

In order to arrive at any satisfactory conclusions regarding the action of any transparent medium on light, it is necessary to form some definite conceptions regarding the external form and internal structure of the medium. This observation appears to apply in full force to microscopic test-objects; and for the purposes of the present inquiry it will suffice to limit our observations to the structure of two well-known test-objects, the scales of *Podura plumbea*, and the siliceous loricæ or valves of the genus *Pleurosigma*, freed from organic matter: the former of these is commonly adopted as the test of the *defining* power of an achromatic object-glass, and the several species of the latter as the tests of the *penetrating* or *separating* power as it has been termed. The defining power depends only on the due correction of chromatic and spherical aberrations, so that the image of any point of an object formed on the retina may not overlap and confuse the images of adjacent points; this correction is never theoretically perfect, since there will always be residual terms in the general expression for the aberration, whatever practicable number of surfaces we may introduce as arbitrary constants; but it is practically perfect, when the residual error is a quantity less than that which the eye can appreciate. The separation of the markings of the Pleurosigmata and other analogous objects, is found to depend on good defining power associated with large angle of aperture.

The Podura scale appears to be a compound structure, consisting of a very delicate transparent lamina or membrane, covered with an imbricated arrangement of epithelial plates, the length of which is six or eight times their breadth, somewhat resembling the tiles on a roof, or the long pile of some kinds of plush. This structure may be readily shown by putting a live Podura into a small test-tube, and inverting it on a glass slide; the insect should then be allowed for some time to leap and run about in the confined space. By this means the scales will be freely deposited on the glass, and being subsequently trodden on by the insect, several will be found, from which the epithelial plates have been partially rubbed off, and at the margin of the undisturbed portion, the form and position of the plates may be readily recognized. This structure appears to be rendered most evident by mounting the scales thus obtained in Canada balsam, and illuminating them by means of Wenham's parabolic reflector. The structure may also be very clearly recognized when the scale is seen as an opake object under a Ross's $\frac{1}{12}$th (specially adjusted for uncovered objects), illuminated by a combination of the parabola and a flat Lieberkuhn, as the writer has elsewhere described*. The underside of the scale thus appears as a smooth glistening surface with very slight markings, corresponding probably to the points of insertion of the plates on the contrary side. The minuteness and close proximity of the epithelial plates will readily account for their being a good test of *definition*, while their prominence renders them independent of the *separating* power due to large angle of aperture.

The structure of the second class of test-objects above mentioned differs entirely from that above described; it will suffice for the present purpose to notice the valves of three species only of the genus *Pleurosigma*, which, as arranged in the order of easy visibility, are, *P. formosum*, *P. hippocampus*, *P. angulatum*.

These appear to consist of a lamina of homogeneous transparent silex, studded with rounded knobs or protuberances, which, in *P. formosum* and *P. angulatum*, are arranged like a tier of round shot in a triangular pile, and in *hippocampus*, like a similar tier in a quadrangular pile, as has frequently been described; and the visibility of these projections is probably proportional to their convexity. The "dots" have by some been supposed to be depressions; this however is clearly not the case, as fracture is invariably observed to take place *between* the rows of dots, and not *through* them, as would naturally occur if the dots were depressions, and consequently the substance thinner there than elsewhere.

This in fact is always observed to take place in the siliceous loricæ of some of the border tribes that occupy a sort of neutral, and not yet undisputed, ground between the confines of the animal and vegetable kingdoms; as for example the *Isthmia*, which possesses a reticulated structure, with depressions between the meshes, somewhat analogous to that which would result from pasting together bobbinnet and tissue paper.

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* See British Association Reports for 1850.

The valves of *P. angulatum* and other similar objects have been by some writers* supposed to be made up of two substances possessing different degrees of refractive power; but this hypothesis is purely gratuitous, since the observed phænomena will naturally result from a series of rounded or lenticular protuberances of one homogeneous substance. Moreover, if the centres of the markings were centres of greatest density, if in fact the structure were at all analogous to that of the crystalline lens, it is difficult to conceive why the oblique rays only should be visibly affected. When *P. hippocampus* or *P. formosum* is illuminated by a Gillett's condenser, with a central stop placed under the lenses, and viewed by a quarter-inch object-glass of 70° aperture, both being accurately adjusted, we may observe in succession, as the object-glass approaches the object, first a series of well-defined bright dots; secondly, a series of dark dots replacing these; and thirdly, the latter are again replaced by bright dots, not however as well defined as the first series. A similar succession of bright, dark, and bright points may be observed in the centre of the markings of some species of *Coscinodiscus* from Bermuda.

These appearances would result if a thin plate of glass were studded with minute, equal and equidistant plano-convex lenses, the foci of which would necessarily lie in the same plane. If the focal surface or plane of vision of the object-glass be made to coincide with this plane, a series of bright points would result from the accumulation of the light falling on each lens. If the plane of vision be next made to coincide with the surfaces of the lenses, these points would appear dark, in consequence of the rays being refracted towards points *now* out of focus. Lastly, if the plane of vision be made to coincide with the plane *beneath* the lenses that contains their several foci, so that each lens may be, as it were, combined with the object-glass, then a second series of bright points will result from the accumulation of the rays transmitted at those points. Moreover, as all rays capable of entering the object-glass are concerned in the formation of the second series of bright focal points, whereas the first series are formed by the rays of a conical shell of light only, it is evident that the circle of least confusion must be much less, and therefore the bright points better defined, in the first than in the last series.

If the supposed lenses were of small convexity, it is evident that the course of the more oblique rays only would be sensibly influenced; hence probably the structure of *P. angulatum* is recognized only by object-glasses of large angular apertures, which are capable of admitting very oblique rays.

The writer has recently, in an address to the members of the

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* Vide Quarterly Journal of Microscopical Science, No. V. pp. 9, 10.

Royal Institution, proposed to explain the extreme darkness of the dots, under certain conditions of focus and illumination, by the hypothesis that some of the oblique rays are thrown out of the field by internal reflexion, being incident at the upper surface at an angle too large for emergence; but this does not appear to invalidate the present hypothesis respecting the course of the transmitted rays.

It does not appear to be desirable that objects should be illuminated by an entire, or, as it may be termed, a *solid* cone of light of much larger angle than that of the object-glass. The extinction of an object by excess of illumination may be well illustrated by viewing with a one-inch object-glass the *Isthmia* illuminated by Gillett's condenser. When this is in focus, and its full aperture open, the markings above described are wholly invisible; but as the aperture is successively diminished by the revolving diaphragm, the object becomes more and more distinct, and is perfectly defined when the aperture of the illuminating pencil is reduced to about 20°. The same point may be attained, although with much sacrifice of definition, by gradually depressing the condenser, so that the rays may diverge before they reach the object; and it may be remarked generally that the definition of objects is always most perfect, when an illuminating pencil of suitable form is accurately adjusted to focus, that is, so that the source of light and the plane of vision may be conjugate foci of the illuminator. If an object-glass of 120° aperture or upwards be used as an illuminator, the markings of Diatomaceæ will be scarcely distinguishable, with any object-glass; the glare of the central rays overpowering the effects of structure on those that are more oblique.

"On the Formation of Powers from Arithmetical Progressions." By C. Wheatstone, Esq., F.R.S.

The same sum n^a may be formed by the addition of an arithmetical progression of n terms in various ways. Hence we are enabled to construct a great variety of triangular arrangements of arithmetical progressions, the sums of which are the natural series of square, cube and other powers of numbers. Among these there are several which render evident some remarkable relations.

Each of the following triangles is formed of a series of arithmetical progressions, the number of terms increasing successively by unity.

The first term of an arithmetical progression of n terms having a common difference δ, and whose sum is n^a, is equal to

$$n^{(a-1)}+\frac{\delta}{2}(1-n).$$

§ 1. SQUARE NUMBERS.

If $S=n^2$, the first term $=n+\frac{\delta}{2}(1-n)$.

A.

Every square n^2 is the sum of an arithmetical progression of n terms, the first term of which is unity and the difference 2.

$$\begin{array}{r}
1 \quad \dots\dots\dots\dots\dots\dots = 1^2 \\
1+3 \quad \dots\dots\dots\dots\dots = 2^2 \\
1+3+5 \dots\dots\dots\dots\dots = 3^2 \\
1+3+5+7 \quad \dots\dots\dots\dots = 4^2 \\
1+3+5+7+9 \quad \dots\dots\dots = 5^2 \\
1+3+5+7+9+11 \quad \dots\dots = 6^2 \\
1+3+5+7+9+11+13 \dots\dots = 7^2
\end{array}$$

Thus, every square number is formed by the addition of a series of odd numbers commencing with unity; a result universally known.

The difference of any two squares is either an odd number, or the sum of consecutive odd numbers.

Each series may be resolved into two others consisting of alternate odd numbers, the respective sums of which are two adjacent triangular numbers, the addition of which it is well known forms a square. *Ex.*:

$$\begin{array}{r}
1+5+9+13=28 \\
3+7+11=\underline{21} \\
49=7^2
\end{array}$$

B.

Every square n^2 is the sum of an arithmetical progression of n terms, the first term of which is $\frac{n+1}{2}$, and the common difference 1.

$$\begin{array}{r}
1 \quad \dots\dots\dots\dots\dots\dots = 1^2 \\
1\tfrac{1}{2}+2\tfrac{1}{2} \quad \dots\dots\dots\dots\dots = 2^2 \\
2+3+4 \quad \dots\dots\dots\dots = 3^2 \\
2\tfrac{1}{2}+3\tfrac{1}{2}+4\tfrac{1}{2}+5\tfrac{1}{2} \quad \dots\dots\dots = 4^2 \\
3+4+5+6+7 \quad \dots\dots\dots = 5^2 \\
3\tfrac{1}{2}+4\tfrac{1}{2}+5\tfrac{1}{2}+6\tfrac{1}{2}+7\tfrac{1}{2}+8\tfrac{1}{2} \quad \dots\dots = 6^2 \\
4+5+6+7+8+9+10 \quad \dots\dots = 7^2
\end{array}$$

This arrangement renders evident that every square of an odd number is the sum of as many consecutive natural numbers as the root has units.

Every square of an odd number is the difference between two triangular numbers the bases of which are respectively $(3n+1)$ and n. For, the sum of any series of natural numbers is the difference of two series of natural numbers commencing with unity; and since, as it is shown above, every square of an odd number is the sum of a series of natural numbers, it is also the difference between two triangular numbers.

It is also evident that series, the sums of which are squares of odd numbers, may be so taken that, when placed in succession, they will form an uninterrupted progression of natural numbers commencing with unity, the sum of which is a triangular number;

$$(1)+(2+3+4)+(5+6+7+8+9+10+11+12+13)\ldots\&c.=$$
$$(1^2+3^2+9^2+27^2\ldots\ldots+(3^n)^2)=$$

a triangular number the base of which is the series

$$(1+3+9+27\ldots\ldots+3^n).$$

§ 2. CUBE NUMBERS.

If $S=n^3$, the first term $=n^2+\frac{\delta}{2}(1-n)$.

C.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is unity, and the difference $2(n+1)$.

1 .... $=1^3$
$1+7$ .... $=2^3$
$1+9+17$ .... $=3^3$
$1+11+21+31$ .... $=4^3$
$1+13+25+37+49$ .... $=5^3$
$1+15+29+43+57+71$ .... $=6^3$
$1+17+33+49+65+81+97$ ...... $=7^3$

D.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is the root n, and the difference $2n$.

1 .... $=1^3$
$2+6$ .... $=2^3$
$3+9+15$ .... $=3^3$
$4+12+20+28$ .... $=4^3$
$5+15+25+35+45$ .... $=5^3$
$6+18+30+42+54+66$ ...... $=6^3$
$7+21+35+49+63+77+91$ .... $=7^3$

The last terms of these series are the alternate triangular numbers. If they be respectively divided by the first terms, the quotients will be the series of odd numbers.

E.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is (n^2-n+1), and the difference 2.

1 .... $=1^3$
$3+5$ .... $=2^3$
$7+9+11$ .... $=3^3$
$13+15+17+19$ .... $=4^3$
$21+23+25+27+29$ .... $=5^3$
$31+33+35+37+39+41$ ...... $=6^3$
$43+45+47+49+51+53+55$ .... $=7^3$

This, it will be observed, is a triangular arrangement of the uneven numbers in their regular order.

Every cube is the sum of as many consecutive odd numbers as there are units in the root*.

The known theorem, that the sum of the cubes of any succession of the natural numbers commencing with unity is equal to the square of the sum of the roots, or, in other words, to the square of the corresponding triangular number, is an immediate consequence of the above.

$$(1^3+2^3+3^3+4^3\ldots\ldots+n^3)=(1+2+3+4\ldots+n)^2=\left(\frac{n^2+n}{2}\right)^2.$$

The sum of any series of odd numbers commencing with unity being equal to the square of the number of terms (A.), the sum of the numbers in any triangle formed as above is necessarily equal to the square of a triangular number. It is also easy to see that each cube is the difference between the squares of two consecutive triangular numbers; and, that the difference between the squares of any two triangular numbers whatever is the sum of consecutive cubes. The following equations have been found by ascertaining what differences of the squares of two triangular numbers are equal to single cubes:—

$$3^3+4^3+5^3=6^3$$

$$11^3+12^3+13^3+14^3=20^3.$$

F.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is a triangular number $\frac{n^2+n}{2}$, and the difference $=n$.

$$\begin{array}{r}
1\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots=1^3\\
3+5\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots=2^3\\
6+9+12\ \ldots\ldots\ldots\ldots\ldots\ldots=3^3\\
10+14+18+22\ \ldots\ldots\ldots\ldots\ldots=4^3\\
15+20+25+30+35\ \ldots\ldots\ldots\ldots=5^3\\
21+27+33+39+45+51\ \ldots\ldots\ldots=6^3\\
28+35+42+49+56+63+70\ \ldots\ldots=7^3
\end{array}$$

Each number contained in this triangle is itself the sum of an arithmetical progression of n terms. Thus, taking the fifth row for example:—

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* Since the present note was communicated to the Royal Society, I have found that this relation has been already noticed by Count d'Adhémar (*Comptes Rendus*, tom. xxiii. p. 501). Cauchy observes, "quoiqu'elle puisse, comme on le voit, se déduire des principes déjà connus, toutefois, elle est assez curieuse et très simple."

$$\begin{array}{r r}
1+2+3+4+5= & 15\\
2+3+4+5+6= & 20\\
3+4+5+6+7= & 25\\
4+5+6+7+8= & 30\\
5+6+7+8+9= & \underline{35}\\
 & 125=5^3
\end{array}$$

The sum of all the numbers contained in a square thus formed is equal to the cube of the number which occupies the upper right-hand and lower left-hand corners. The sum of the numbers in either of the diagonals is the corresponding square, and in the case of the odd numbers the sum of the middle horizontal or vertical line is also the square.

This last-mentioned relation was pointed out by Lichtenberg*, who stated the theorem thus:—If a be a whole number, and A be the sum of all the natural numbers from 1 to a, then:

$$a^3=A+(A+a)+(A+2a)+(A+3a)\ldots\ldots+(A+[a-1]a).$$

G.

Every cube n^3 above 1 is the sum of an arithmetical progression of n terms, the first term of which is $(n-2)^2$, and the difference $=8$.

$$\begin{array}{r l}
0+8 & \ldots\ldots\ldots\ldots\ldots\ldots=2^3\\
1+9+17 & \ldots\ldots\ldots\ldots\ldots=3^3\\
4+12+20+28 & \ldots\ldots\ldots\ldots=4^3\\
9+17+25+33+41 & \ldots\ldots\ldots=5^3\\
16+24+32+40+48+56 & \ldots\ldots=6^3\\
25+33+41+49+57+65+73 & \ldots\ldots=7^3
\end{array}$$

Each progression of this triangle, consisting of an uneven number of terms, contains two consecutive odd square numbers.

An *uninterrupted* arithmetical progression commencing with unity and proceeding by the constant addition of 8, arranged in a triangular form, presents some curious results. 1st. The first terms of each line are the squares of the odd numbers in their regular sequence. 2nd. The sum of all the numbers in any two adjacent lines is the cube of an odd number.

$$\begin{array}{r l}
1\ \ldots\ldots\ldots\ldots & \\
 & \}\ldots\ldots=3^3\\
9+17\ \ldots\ldots\ldots & \\
 & \}\ldots\ldots=5^3\\
25+33+41\ \ldots\ldots & \\
 & \}\ldots\ldots=7^3\\
49+57+65+73\ \ldots\ldots & \\
 & \}\ldots\ldots=9^3\\
81+89+97+105+113\ \ldots & \\
 & \}\ldots\ldots=11^3\\
121+129+137+145+153+161 &
\end{array}$$

It is evident from the preceding arrangement that

$$(2n+1)^2=1+8\left(\frac{n^2+n}{2}\right).$$

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* G. C. Lichtenberg's *Vermischte Schriften*, vol. ix. p. 359. Göttingen, 1806.

Thus any triangular number multiplied by 8 with 1 added is equal to the square of an odd number; or, any square of an uneven number minus 1 is divisible by 8, and the quotient is a triangular number.

§ 3.

Of the higher powers I will confine myself to one example.

H.

Every fourth power n^4 is the sum of an arithmetical progression of n terms, the first term of which is n^2, and the difference $2n^2$.

$$\begin{array}{rl}
1 \quad \ldots\ldots\ldots\ldots\ldots & = 1^4 \\
4+12 \quad \ldots\ldots\ldots\ldots & = 2^4 \\
9+27+45 \quad \ldots\ldots\ldots\ldots & = 3^4 \\
16+48+80+112 \quad \ldots\ldots\ldots & = 4^4 \\
25+75+125+150+225 \quad \ldots\ldots & = 5^4 \\
36+108+180+252+324+396 \quad \ldots\ldots & = 6^4
\end{array}$$

This triangle consists of the progressions in (D.) multiplied respectively by n, or of those in (A.) multiplied by n^2.

"On the Structure and Functions of the Rostellum in *Listera ovata.*" By J. D. Hooker, M.D., F.R.S.

The author first gives an account of the form and structure of the rostellum of *Listera ovata*, and its relation and position to the anther and stigma. He finds that the rostellum is divided by parallel septa (at right angles to the plane of that organ) into a series of longitudinally elongated loculi, which gradually taper from the base upwards, and terminate at two opake cellular spots, one on each side of the apex of the rostellum, towards which latter the loculi also converge. When the flower is fully expanded, these loculi are distended with a viscid grumous fluid, full of chlorophyll granules. Their external walls, and the septa dividing them, are formed of a delicate, transparent tissue, which is cellular at the base and apex of the rostellum only.

Their grumous contents, when examined at the earliest period of development, present the appearance of opake club-shaped compressed bodies, with areolated surfaces; a form and appearance that may be restored at a later period by coagulating with alcohol.

At the period of impregnation the slightest irritation of the rostellum causes the sudden and forcible discharge of the contents of these loculi (through the rupture of the cellular tissue at the apex of the rostellum) and its protrusion in the form of two viscid glands, which coalesce into one, after which the rostellum rapidly collapses and contracts.

The pollen-masses, when freed from the anther-case, fall naturally upon the rostellum; they are retained there by their viscid gland-like contents, and, breaking up, the pollen-grains become (by the contraction of the rostellum) applied to the subjacent stigmatic surface.

The author adds remarks on the structure of the rostellum in allied genera of Orchideæ, and indicates some of the more important morphological changes to which that organ is subjected, in connexion with the development of various appendages to the column and pollen in the same natural family.

"On the Immediate Principles of the Excrements of Man and Animals in the Healthy Condition," by William Marcet, M.D.

The author describes a new method of extracting the immediate chemical constituents of the excrements of Man and animals, and gives an account of the substances obtained by its employment.

Healthy human fæces are boiled to exhaustion in alcohol. The residue is insoluble in æther, and yields to boiling water nothing but ammoniaco-magnesian phosphate. The strained alcoholic solution deposits, on standing, a sediment, from which it is decanted and then mixed with milk of lime. The subsiding lime is of a yellow-brown colour; it is dried on filtering-paper and treated with æther, cold or hot, and the solution thus obtained yields, on spontaneous evaporation, beautiful silky crystals, which are purified by solution in a mixture of alcohol and æther, repeated filtration through animal charcoal and recrystallization; they then appear in circular groups, have the form of acicular four-sided prisms, and polarize light very readily. This crystalline body the author proposes to call *Excretine*. It is very soluble in æther, cold or hot, but sparingly soluble in cold alcohol; its solution has a decided though weak alkaline reaction. It is insoluble in hot or cold water, and is not decomposed by dilute mineral acids. It fuses between 95° and 96° C., and at a higher temperature burns away without inorganic residue. When boiled with solution of potash it does not dissolve. As to its qualitative constitution, it is found to contain nitrogen and sulphur, though in small proportions; the products of its decomposition have not yet been investigated.

The author has in several cases observed the excretine to crystallize directly in the alcoholic solution of fæces before the addition of lime, and has scarcely any doubt that it exists for the most part in a free state in the excrements, and constitutes one of their immediate principles. As to its source, he observes that it appeared in excess when a considerable quantity of beef had been taken, and in less than the usual quantity in a case of diarrhœa attended with loss of appetite; but none could be directly obtained from beef on subjecting it to the same process of extraction as fæces. Neither could it be found in ox-bile, the urine, or the substance of the spleen. From the difficulty of obtaining the contents of the human small intestine in a healthy state, its presence or absence in that part of the alimentary canal has not yet been satisfactorily determined.

The lime precipitate, after having been thus thoroughly deprived of the excretine by æther, is next treated with hydrochloric acid, and water or alcohol, by which means margaric acid is extracted from it. The author is uncertain whether the margaric acid of the fæces is free or combined with excretine, but he is disposed to conclude that the neutral fats are decomposed in the intestinal canal and their acid set free. Not having been able to detect stearic acid in human evacuations, he supposes that what is contained in the fat of mutton or beef taken as food must be converted into margaric acid in its passage through the alimentary canal.

The lime precipitate, freed from excretine and dissolved in alcohol by means of hydrochloric acid, forms a dark port-wine-coloured solution, from which the margaric acid is deposited. On then adding water to the solution and concentrating it on the water-bath, a flaky colouring matter separates, which, being purified by solution in æther and washing with water, is obtained as a dark-brown or black amorphous substance, similar to the colouring matter of blood, and to that which Dr. Harley has lately extracted from urine.

The matters brought down with the lime having been thus extracted, the sediment which spontaneously subsides from the alcoholic solution of fæces before its treatment with the milk of lime, is next examined. This deposit appears to be complex in its nature; it has a strongly acid reaction, and presents under the microscope small oily globules, mixed sometimes with crystals of excretine and accompanied by a yellow amorphous matter. By boiling with alcohol and filtration, a residue remains which the author has not yet examined, and two substances are obtained from the filtrate. The first is deposited on cooling; when collected and dried on filtering-paper it has a granular character and is quite colourless; it is very sparingly soluble in æther, fuses by heat, and burns with a bright fuliginous flame, leaving a white residue consisting of phosphate of potash. The author has not yet been able satisfactorily to decide whether this is a pure immediate principle or not; he is inclined to consider it as a combination of phosphate of potash and a pure organic substance. The filtered fluid, after separation of this matter, still contains a substance which he has called *Excretolic acid*. It is obtained by evaporating to dryness, extracting the residue with æther, adding to the æthereal solution alcohol and lime-water, and heating. The acid is precipitated in combination with lime, from which it is separated by means of sulphuric or hydrochloric acid and solution in æther. The æthereal solution, after being well washed with water to remove mineral acid, yields the pure excretolic acid on evaporation. This body is of an olive colour; it fuses between 25° and 26° C., and at a higher temperature burns without residue. It is insoluble in water and in a boiling solution of potash; very soluble in æther, sparingly soluble in cold alcohol, readily so in hot; its solutions having a marked acid reaction. The author is disposed to believe that in excrement it is combined in form of a salt, with excretine or a basic substance closely allied to it, which is obtained in the filtrate from which the excretolic acid is precipitated in combination with lime in the process of its purification.

The author failed to obtain evidence of the presence either of butyric or of lactic acid in the clear alcoholic solution of fæces filtered from the precipitate formed by the milk of lime. From the above investigation, therefore, it appears that healthy human excrements contain:—

1. A new organic substance, possessing an alkaline reaction, which the author names *Excretine*.

2. A fatty acid, having the properties of margaric acid, but not constantly present.

3. A colouring matter, similar to that of blood and urine.

4. A light granular substance, whose properties have not yet been sufficiently examined to admit of its being considered a pure substance.

5. An acid olive-coloured substance, of a fatty nature, named *Excretolic acid*.

6. No butyric acid and no lactic acid.

The fæces of various animals were submitted to the same process of analysis, with the following results:—

1. The excrements of carnivorous mammalia, viz. the Tiger, Leopard and Dog (fed on meat), contain a substance allied in its nature to excretine, but not identical with it. They contain no excretine; they yield butyric acid, which is not present in human excrements.

2. The excrements of the Crocodile contain cholesterine and no uric acid, whilst those of the Boa yield uric acid and no cholesterine.

3. The fæces of herbivorous animals, viz. the Horse, Sheep, Dog (fed on bread), Wild Boar, Elephant, Deer and Monkey, contain no excretine, no butyric acid and no cholesterine.

"On the Vine-Disease in the Port-wine Districts of the Alto-Douro, in April 1854. With a Supplementary Note on the Proposed Remedies for its Eradication." By Jos. James Forrester, Esq., F.R.G.S.

In Portugal, where the vine-disease committed great ravages last year, no measures have as yet been adopted for ascertaining whether the disease is *radical*, or only superficial; or whether any *practical remedy* may be adopted in order to arrest the progress of the evil.

At Oporto, and in the north of Portugal, an opinion prevails—

"That the *Oïdium* is the *effect*, and not the *cause* of the epidemic.

"That the roots and the wood of the vines are diseased.

"That *sporules* of the *Oïdium* exist in the interior of the vine, and about its roots.

"That the obstruction to the ascent of the sap through the various ducts, originates in the roots.

"That black spots appear in the joints of the branches, indicating that disease exists throughout the body of the vine.

"That a new fungus has appeared on the vines, in the shape of small globules, containing carbonic acid.

And "that, although vegetation may continue for a while, the fruit will not ripen, and the vines will die in a couple of years from this date."

Considering that it would be of some importance to determine whether the disease has its origin in the roots or from external causes, and with a hope that some practical cure for the diseased vines grown in the open air may be discovered, I record the results of my own observations of the progress of the vine-malady in the Alto-Douro.

The Port-wine District extends eight leagues west and east from the Serra do Marão (an elevation of 4400 feet[*] from the level of the sea) to the Quinta do Baleira, near Sam João da Pesqueira, and four leagues north and south, between Villa Real and the city of Lamego[†].

The winter streams, tributaries to the Douro, on the right bank, are the Sermenha, Corgo, Ceira, Pinhão, and Tua; and, on the left, the Varoza, Temilobos, Tedo, Tavora, and Torto.

At Baleira, the Douro runs at an elevation of not more than 250 feet; whence some opinion may be formed of the nature and inequality of the country, and of the numerous abrupt mountain ridges, on the inclines of which the vines are grown. The Wine-Districts of the Alto-Douro form a long irregular basin, girt by the granite chains of the Tras-os-Montes and Beira; and this being for the most part of schist formation, and protected from the bleak winds, is particularly adapted for the cultivation of the vine. The strata of the margins of the Douro differ from the higher and middle grounds in character, "being composed of strong clays, more or less micaceous."

The extreme northern and southern boundaries—from the Serra do Marão to Favaios, and from the Serra do Monte Muro (near Lamego) to Sam João da Pesqueira—are undulating mountain plains of still heavier soil, and more suitable for the growth of firs than vines. In former years, this fact was clearly defined by the Royal Wine Company, who divided the districts into two, one being termed Feitoria (where the most superior wines were produced and classified for exportation), the other Ramo, where only very inferior wines, for the consumption of the country and for distillation, were produced to a small extent. Now, the two districts have become one; the plantations of pines on the heights and the corn-producing valleys having alike been converted into vineyards; the *quantity*, and not the *quality*, of the produce being the results sought by the wine-grower within this privileged demarcation.

One thousand vines generally produce a pipe of wine, and the total number of vines in the Port-wine Districts above described may be estimated at 90,000,000.

In the summer time, there is great scarcity of water throughout the district. The vineyards are for the most part situated on abrupt mountain slopes, the vines being planted on terraces, which are not appropriate for the cultivation of anything else. The vines are grown not higher than three feet from the ground, and are planted about six feet apart, supported with canes or stakes. The labour in the vineyards is performed by the natives of Gallicia, who visit the district three or four times a year in search of employment.

In July 1850, I first observed a blight on three or four vines, at a considerable distance from each other, in the Wine Districts. The general appearance of this blight to the naked eye greatly resembled that which appears on the peach-tree and the rose. The Douro farmers had often previously noticed a similar *po' branco* (white powder) on the vines.

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[*] "Considerações geraes sobre a Constituiçũo Geologicao do Alto-Douro." By Dr. J. P. Rebello. Porto, 1848.

[†] See map of the Wine-Districts of the Alto-Douro. By J. J. Forrester.

In 1851 the season was favourable, and the vines (on which we had observed the blight in the previous year) were vigorous, and produced perfect fruit. The vintage of 1851, throughout the Alto-Douro, was excellent. In 1852 there was much wet and cold; the blight again appeared, and the vines were attacked to the extent of about one in fifteen hundred. The vintage of 1852 was of inferior quality; but no one ascribed the failure to any disease in the vine. From the autumn of 1852 until midsummer 1853, continued rain, sleet, hail, and bleak winds prevailed, and in 1853 there was no spring. In March of the same year the navigation of the Douro was impeded, and the bar rendered impassable on account of the floods; and in April and May of the same year, prayers were offered up in the churches throughout the Wine Districts for fine weather.

In March 1854, only half-cargoes could be brought down the river Douro, on account of the want of water, and rain was prayed for.

Early in June 1853, the heat became suddenly intense, and the vines had already burst forth with great vigour; whilst, in the middle of the same month, the nights became as cold as in winter.

In the most exposed situations the vines received the greatest shock; the circulation of the sap was evidently deranged, and their fruit withered as soon as it appeared. In some neighbouring vineyards, less exposed, the grapes grew no larger than peas; they were then suddenly covered with the blight (now designated the *Oïdium*), and in about three days became rotten.

On the inclines of the mountains on either bank of the river Douro, the waters had run off, and but little blight appeared. In the low and heavy grounds, the most sheltered from the winds, the waters remained stagnant; yet the fruit grew to its full size, and had come to maturity, when the new wood, leaves, and fruit were all, to a greater or less degree, covered with the *Oïdium*. The blight sometimes attacked entire vineyards, and at other times only partially affected one property, and then showed itself in others at a distance—intermediate estates being for the time wholly untouched.

It was in July 1853 that the existence of the disease in the vineyards of the Douro first attracted particular attention; but many vines betrayed no unhealthy symptoms until the fruit was nearly ripe. The upper part of the branches was first attacked. In some instances the woody part of the young branches was speckled with the *Oïdium*, while the bunches of fruit were apparently altogether free from it. In other instances, the grapes became touched with the disease immediately before the vintage, but the woody part of the branches betrayed no such symptoms. In some vines, which I supposed had altogether escaped the disease (and long after the fruit was gathered and the leaves had fallen off), blotches or stains, evidently the *mycelium* of the *Oïdium*, appeared on the wood.

The usual number of seeds in a black grape is two or three; but in the year 1853, in all instances the grapes, which at first promised abundance of wine, were found each to contain from three to five seeds.

Twenty-one baskets of grapes usually produce one pipe of wine; but in the year 1853, a pipe of wine was rarely obtained even from thirty baskets of grapes. From seven to nine pipes of ordinary wine generally give a pipe of brandy, 20 per cent. above British proof; but in the year 1853, from ten to twelve pipes of ordinary wine were required to give one pipe of brandy of that strength.

Wines, when properly made, should be trodden continuously for 36 hours in the *lagar* (an open stone vat), and remain there for 36 to 48 hours more, until the tumultuous fermentation be completed, when they should be run off into larger *tonels* (wooden vats, not tightly bunged), where the second fermentation will be completed about Christmas. In 1853, in situations where the disease most prevailed, the grapes fermented before they had been trodden more than twelve hours, when the wines were drawn off and passed into tonels, where brandy, as a precautionary measure, was given to them. The fermentation of these wines ceased altogether before the 15th October. In other situations, where the disease had not made progress, the grapes were sound; and, where they were properly crushed and fermented, they produced excellent wine, without the addition of brandy.

Wines, during their second fermentation, deposit a thick coating of argol on the sides of the tonels. In 1853 there was very little argol deposited; but the gross lees of the wine were in great demand, and sold for about 15*s*. per basket,—a sum which in former years might almost have purchased double the quantity of grapes.

In the same manner as the form and colour of the wood, leaves, and fruit of vines differ, so does their pith vary in appearance, according to the age of the wood or the quality of the vine. The pith in an old vine, when the sap is rising, graduates from a deep vandyke-brown colour to a pale yellow, the shade being always darker near the joints.

In April 1854, I rooted up many vines of different qualities, and in various situations, and I was unable then to detect any remarkable appearance in the interior of the vine different from what I had seen in other years after continued wet and cold weather; but the exterior of all the last year's branches bore palpable evidence of having been violently attacked with the *Oïdium*. Some vines had suffered more than others, and many of their vessels were evidently choked; but, in most instances, in cutting the vine longitudinally, this obstruction was found to have arisen either from wounds, bad pruning, or natural decay. I found no black spots at the joints of the branches; and, with the exception of the stains left by the disease of last year, the vines looked healthy and vigorous, throwing out strong shoots and promising an abundance of fruit.

Towards the end of April 1854, much rain fell in the district; the easterly winds destroyed the young branches; and in exposed situations and heavy soils the *Oïdium* again made its appearance.

In 1853, the disease attacked the vines bodily, and almost simultaneously: whereas, in 1854, the *Oïdium* appears to be creeping out of the skin of the last year's wood, and insidiously to extend itself over the branches.

The globules (to which allusion has been made above) cover the young shoots. I have been familiar with these for twenty-three years past, and the Douro farmers call them the "perspiration" of the vine. They do not indicate disease, whereas the smallest possible quantity of the *po' branco*, or white powder, being transferred to a perfectly healthy vine, immediately infected it.

In the Alto-Douro the oranges, lemons, citrons and limes have all been blighted, and every kind of vegetable appears to be suffering from sickness.

The vines which suffered most in the Alto-Douro, in 1853, were the Muscatel, Malvazia, Alvarilhão, Ferral, Agadanho and Senzão.

Since my arrival in this country I have noticed that the vines grown on walls in the open air, vines grown in greenhouses, vines grown in hot-houses, vines forced, all show identically the same effects of the *Oïdium* of last year, as exist on the vines in the Alto-Douro.

Taking into consideration all the circumstances above narrated, I have come to the conclusion,—

That the *Oïdium* is the *cause*, and not the *effect* of the disease; that the inclemency of the season in 1853, by checking the circulation of the sap in the vines, produced a predisposition for disease; that if the *Oïdium* continues to appear on the branches of the vines, it is only too probable that it may in a very few years be destroyed; that the *globules* are a sign of health and not of disease, and have no connexion whatever with the fungus called *Oïdium*; and that if the germ of the *Oïdium*, probably still lurking on the old branches, can be destroyed in the open air as effectually as it appears to have been destroyed under glass, then I feel persuaded that all the vines in the Port-wine districts of the Alto-Douro may be saved.

"Supplementary Note on the proposed Remedies for the Eradication of the Vine-Malady."

1st. I will take the annual production of wines in the Port-wine districts of the Alto-Douro at 80,000 pipes instead of 90,000, and the number of vines to be treated as diseased at 80,000,000.

2nd. The value of freehold land in that district, for the growth of 1000 vines, or one pipe of wine, may be estimated at 50*l*., yielding an interest or rental of 3*l*. per annum.

3rd. The total freehold value of the vineyards in those districts may be estimated at 4,000,000*l*. sterling, giving an annual revenue of 240,000*l*.

4th. In the event of the disease not being checked in its progress, and the grapes being destroyed this year in the Alto-Douro, a *minimum* loss of 240,000*l*, will be sustained, and should the vines perish, the loss may be 4,000,000*l*.

5th. Portugal is said to produce annually 1,000,000 pipes of wine of all sorts and qualities, but I will estimate the total production at 800,000 pipes, and the total number of vines in the country at 800,000,000.

If *Flour of Sulphur* be used, the leaves, branches and shoots are first moistened as equally as possible with a syringe; then the whole is dusted with sulphur, which adheres to the moistened surface.

This operation would have to be repeated thrice, and would consume two ounces of sulphur for every vine, in each of the operations, making a total of 480,000,000 ounces, or about 13,392 tons for the treatment of the 80,000,000 vines in the Alto-Douro, and 133,920 tons for the vines of the whole country.

Sulphur would not cost less than 10*l.* per ton, delivered in the centre of the Alto-Douro districts, or in any other part of the interior of Portugal. The expense of sulphur required for the Douro would be 133,920*l.*, and for the whole country 1,339,200*l.*

One man could moisten one vine in one minute, and another man could dust it with sulphur in the same time, so that two men could perform the complete operation on about 700 vines daily, at a cost of 1*s.* 3*d.* each man for labour, making a total of 14,285*l.* in the Alto-Douro, and 142,850*l.* for all Portugal.

I will suppose that there are 4000 vineyards in the Alto-Douro, planted each with 20,000 vines. The first cost of syringes and fumigators would amount to not less than 10*l.* for each vineyard, or a total of 40,000*l.* for the Alto-Douro.

One quart of water would be required for every vine in each operation, making a total of about 90,000 pipes, the cartage of which, and the labour of distributing it over the mountain vineyards, in tubs, on men's heads, would cost a minimum of 10*s.* per pipe, or a total of 45,000*l.* for the Alto-Douro, and 450,000*l.* for the whole country.

Recapitulation.

	In the Douro.	In the whole country.
For sulphur, say	£135,000	£1,350,000
For labour, at £15,000 for each of the three operations	45,000	450,000
For water, at £45,000 for each of the three operations, or as much as the sulphur	135,000	1,350,000
For instruments	40,000	400,000
	£355,000	£3,550,000

This is independent of any charge for factors or superintendents, or for the extra expense in treating vines and vineyards which are so much further apart than are those in the Alto-Douro.

This expense to be incurred *in the endeavour* to save *one year's crop*, would be equal to a charge of 4*l.* 10*s.* per pipe, or to a *year and a half's rental* of the vineyards, or to more than the whole revenue of Portugal for an entire year.

If a *solution* of *lime* and *sulphur* be employed instead of *flour of sulphur*, the operation would not be less expensive.

If, in *conjunction with the sulphuring* of the branches, the roots were to be exposed, and sulphur and lime thrown upon them, I could not estimate the total expense at less than 1¼*d.* to 1½*d.* per vine, which would entail a charge equal to another year and a half's rental of the vineyards, or 18 per cent. on their freehold value for the chance of saving one year's crop.

Again, if the trunks of the vines be bored and the sulphur inserted, this most delicate operation could only be performed by the factors themselves, and if the vines were to be cut down to the ground and grafted with cuttings from sound vines, the entire operation (which could only be performed by the factors) would cost 1¼*d.* to 1½*d.* for each vine, or as much as the sulphuring process; and besides this, there would be a loss of four years' produce at 3*l.* per pipe per annum, making a total loss of 16*l.* 10*s.* in every vineyard growing vines capable of yielding one pipe of wine, or about 33⅓ per cent., or one-third of the freehold value of the estate.

Lastly, the dressing of the trunk and branches of 800,000,000 vines with mineral tar could not be carried into operation within any reasonable period, on account of the tediousness of the process and the scarcity of labourers. The expense of the tar would also be a bar to its being used.

XII. *Intelligence and Miscellaneous Articles.*

ON A PECULIAR PHÆNOMENON IN THE ELECTRO-DEPOSITION OF ANTIMONY. BY MR. G. GORE.

If a piece of metallic antimony is connected by a wire with the positive pole of a small Smee's battery of one or two pairs of plates, and immersed in a solution of hydrochlorate of terchloride of antimony, *i. e.* the ordinary chloride of antimony as prepared for pharmaceutical purposes, and a clean piece of sheet copper of similar size, or a little larger, connected by a wire with the negative pole of the battery, and immersed in the same liquid, at a distance of two or three inches from the antimony, a strong current of electricity will pass through the liquid, and metallic antimony will immediately be deposited all over the piece of copper, and will form a distinct coating in two or three minutes; if the power of the battery is too strong, the deposited metal will have a dull appearance, and less battery power should be employed, or the piece of antimony immersed to a smaller extent in the liquid; the deposit will then in a short time assume a fine bright appearance, somewhat similar to highly polished silver. If the process be allowed to continue for twenty-four hours, the coating of antimony will be at least half the thickness of a sixpence; and by continuing it for eight or nine days, I have obtained a continually bright and reguline deposit of upwards of half an inch in thickness.

If, during any part of the time the deposit is progressing, the deposited antimony be taken out and struck gently, or rubbed, with any hard substance, such as metal or glass, an explosion occurs, with a small cloud of white vapour, sometimes with a flash of light, and nearly always with considerable heat, sufficient to burn one's fingers, melt gutta percha, burn paper, and even scorch deal wood quite brown, and invariably accompanied by fracture of the deposited metal; sometimes, if the process of deposition has been interrupted, and the deposited metal is not homogeneous, only a thin scale falls off, in such case the explosion and heat are less; in other instances, where the process was regular and the metal homogeneous, the fracture extended quite through the metal to upwards of one-eighth of an inch in depth.

I have observed this phænomenon in about nine instances, in several of which the explosion took place *even in the liquid*, by striking the deposit against the glass containing vessel; and in one instance it occurred after the metal had been well washed with dilute hydrochloric acid, dried, and had remained out of the liquid several hours.

The same phænomenon occurred with deposits obtained in a solution composed of one fluid ounce of the antimony liquid, and half a fluid ounce of a saturated aqueous solution of hydrochlorate of ammonia.

Birmingham.

ON THE COMPOSITION AND PROPERTIES OF FATS. BY D. HEINTZ.

According to the investigations of Heintz upon fats, these bodies always furnish, on saponification with potash, fatty acids and glycerine, as indeed has been known ever since Chevreul's experiments. According to the author's experiments, the acids of the acetic acid series, expressible by the formula $C^n H^n O^4$, occur together with oleic acid in fats; but those acids of this series in which n is a number not divisible by 4, are absent from the products of the saponification of fats. Thus the non-existence of margaric acid ($C^{34} H^{34} O^4$) as a chemically pure substance is particularly proved.

In his most recent investigation, Heintz shows that even the saponification of spermaceti furnishes no other fatty acids than those which can be expressed by the general formula $C^{4n} H^{4n} O^4$ (n=a whole number). The cetic acid ($C^{30} H^{30} O^4$) and cocic acid ($C^{26} H^{26} O^4$), formerly supposed by him to exist in that substance, are mixtures of at least two of the fatty acids of spermaceti. Besides stearic acid ($C^{36} H^{36} O^4$) and palmitic acid ($C^{32} H^{32} O^4$), the occurrence of which in spermaceti has already been proved by the author, two other acids have also been obtained in a chemically pure state from that substance. These are myristic acid ($C^{28} H^{28} O^4$) and laurostearic acid ($C^{24} H^{24} O^4$). Their separation was effected by the method of partial precipitation combined with that of recrystallization.

Myristic acid in the pure state has hitherto been unknown, for

Playfair, who first mentioned it, gives its melting-point at 120° F., whilst it really fuses at 129° F. When cold, it is exactly like palmitic acid, appearing in scaly crystals. It is however more readily soluble in alcohol, and crystallizes from this solution in laminæ of a pearly lustre. Analyses of the acid itself, as well as of its silver, lead, copper, baryta and magnesia salts, furnish concordant results leading to the formula $C^{28}H^{27}O^{3}+RO$. Its combination with oxide of æthyle (myristic æther) melts at 83°·3 F., crystallizes very beautifully in the cold, and dissolves readily in hot alcohol. Its composition is $C^{28}H^{27}O^{3}+C^{4}H^{5}O$.

Laurostearic acid, which was prepared by Marsson from the oil of laurel berries, by Sthamer from the fat of pichurine beans, and by Görgey from cocoa-nut oil, melts with rather more difficulty than stated by these chemists. Its melting-point is 110°·5 F. It dissolves very readily in alcohol, and only crystallizes partially from this solution at a low temperature. It is transparent, but still forms scaly crystals; it consists of $C^{24}H^{23}O^{3}+HO$, as appears from analyses not only of the acid itself, but also of its silver, lead and baryta salts.

Gottlieb has already noticed, that stearic acid mixed with margaric acid in certain proportions, may acquire a lower melting-point than that of the last-mentioned acid. As this, according to the author's previous investigations, is a mixture of stearic and palmitic acids, this peculiarity must be a property of this mixture, which in fact is the case, as margaric acid itself is nothing but a mixture of palmitic acid with stearic acid, which melts more readily than the former acid. Heintz has found, however, that any two fatty acids may form a mixture possessing a lower melting-point than even the most fusible of them in a pure state. He has drawn up the following tables, showing the melting-point and mode of solidification of mixtures in simple proportions of every two of the four acids,—stearic acid, palmitic acid, myristic acid, and laurostearic acid:—

A mixture of stearic acid. parts.	palmitic acid. parts.	Melts at	Solidifies at	Form of solidification.
100	0	156·56° F.	.. ° F.	scaly crystalline.
90	10	152·96	144·5	the same.
80	20	149·48	140·54	finely acicular.
70	30	145·08	106·68	the same.
60	40	140·48	133·70	uncrystalline tubercular.
50	50	133·88	131·00	laminar crystalline.
40	60	133·40	130·1	the same.
35	65	132·08	129·74	uncrystalline, shining.
32·5	67·5	131·86	129·2	the same.
30	70	131·18	129·2	the same, lustreless.
20	80	135·5	128·84	very indistinctly acicular
10	90	140·18	130·1	beautifully acicular.
0	100	143·6	..	scaly crystalline.

A mixture of palmitic acid. parts.	myristic acid. parts.	Melts at	Solidifies at	Form of solidification.
100	0	143·6°F.	.. °F.	scaly crystalline.
95	5	141·98	136·4	the same.
90	10	140·18	132·26	the same.
80	20	136·4	128·3	fine scaly crystals.
70	30	130·82	124·34	extremely fine needles.
60	40	124·7	121·1	uncrystalline tubercular.
50	50	119·04	113·54	large laminar crystals.
40	60	116·6	110·66	indistinctly laminar.
35	65	115·7	..	uncrystalline opake.
32·5	67·5	115·16	111·2	the same.
30	70	115·16	110·66	the same.
20	80	121·1	106·34	uncrystalline.
10	90	125·54	113·54	in long needles.
0	100	128·84	..	scaly crystals.

A mixture of myristic acid. parts.	laurostearic acid. parts.	Melts at	Solidifies at	Mode of solidification.
100	0	128·84°F.	.. °F.	scaly crystals.
90	10	125·54	117·14	the same.
80	20	121·28	112·1	fine crystals, neither distinctly scaly nor acicular.
70	30	116·06	102·2	the same.
60	40	109·4	102·2	uncrystalline, with a few shining spots.
50	50	99·32	96·26	large laminar crystals.
40	60	98·06	92·3	uncrystalline, with a few shining spots.
30	70	95·18	90·14	uncrystalline.
20	80	101·3	91·4	the same.
10	90	106·34	96·8	acicular crystals.
0	100	110·48	..	scaly crystals.

A mixture of stearic acid. parts.	myristic acid. parts.	Melts at	Form of solidification.
0	100	128·84°F.	
10	90	125·06	uncrystalline opake.
20	80	119·04	indistinctly crystalline.
30	70	118·76	laminar crystals.
40	60	122·72	beautiful large laminar crystals.

A mixture of palmitic acid. parts.	A mixture of laurostearic acid. parts.	Melts at	Form of solidification.
0	100	110·48° F.	
10	90	106·7	uncrystalline.
20	80	98·78	indistinctly crystalline.
30	70	100·94	small laminar crystals.
40	60	104·18	beautiful large laminar crystals.

A mixture of stearic acid. parts.	A mixture of laurostearic acid. parts.	Melts at	Form o. solidification.
0	100	110·48° F.	
10	90	106·7	uncrystalline.
20	80	101·3	uncrystalline, warty.
30	70	110·12	the shining faces of small crystals appearing on the surface.
40	60	123·44	uncrystalline, warty.

From these tables it appears that,—

1. By the addition of any fatty acid to from 4 to 10 times its quantity of another fatty acid, the melting-point of the latter is lowered, even though the acid added be more difficult of fusion.

2. The mixture of two acids differing by $C^4 H^4$, which possesses the lowest melting-point, consists of about 3 parts of that which contains the most carbon and 7 parts of the other.

3. The mixture of two acids differing by $C^8 H^8$, which possesses the lowest melting-point, consists of about 25 parts of the richest in carbon and 75 of the other.

4. The mixture of two acids differing by $C^{12} H^{12}$, which possesses the lowest possible melting-point, consists of about 20 parts of that which contains most carbon and 80 of the other.

5. Thus the greater the difference in the amount of carbon in two acids, the smaller is the quantity of that which contains most carbon required to produce the lowest melting-point.

6. The greater the amount of carbon in two acids differing by $C^4 H^4$, the less is the difference between the melting-point of the pure acids and the lowest point of the mixed acids.

7. If to 9 parts of an acid $C^{4n} H^{4n} O^4$, we add 1 part of an acid $C^{4(n+1)} H^{4(n+1)} O^4$, and to a similar quantity of the former also 1 part of an acid $C^{4(n-1)} H^{4(n-1)} O^4$, two mixtures are obtained possessing the same melting-point. The same applies, or nearly so, to mixtures of 8 and 7 parts $C^{4n} H^{4n} O^4$, and 2 and 3 parts $C^{4(n+1)} H^{4(n+1)} O^4$, or $C^{4(n-1)} H^{4(n-1)} O^4$.

8. A mixture of a little more than 3 parts of the acid $C^{4n} H^{4n} O^4$ with a little less than 7 parts of the acid $C^{4(n+1)} H^{4(n+1)} O^4$, possesses the same melting-point as the acid $C^{4n} H^{4n} O^4$ in the pure state.

The mixture of 9 parts $C^{4n} H^{4n} O^4$ with 1 part $C^{4(n+1)} H^{4(n+1)} O^4$, solidifies in acicular crystals (like margaric acid).

10. The mixture of equal portions of fatty acids differing by $C^4 H^4$, solidifies in large laminar crystals (like anthropic acid).

11. Mixtures of 20 to 30 parts $C^{4n} H^{4n} O^4$ with 80 to 70 parts $C_{4(n+1)} H^{4(n+1)} O^4$, crystallize in extremely fine needles.

12. Mixtures of 60 parts $C^{4n} H^{4n} O^4$ with 40 parts $C^{4(n+2)} H^{4(n+2)} O^4$, solidify in large laminar crystals (like anthropic acid).

Heintz has also found that when, to a mixture of two acids differing by $C^4 H^4$, a small quantity of one containing a larger amount of carbon, and consequently more difficult of fusion, is added, the melting-point becomes still lower by several degrees. For instance, if about 3 to 4 parts of stearic acid, which melts at 124°·26 F., be added to the mixture, fusing at 115°·16 F., of palmitic acid (which fuses at 143°·6 F.) and myristic acid (which melts at 128°·84 F.), the mixture obtained fuses at 110°·84 F. Such mixtures of these fatty acids consequently behave very like the readily fusible metallic mixtures, which also consist of three metals (lead, tin and bismuth).—*Bericht der Akad. der Wiss. zu Berlin*, 1854, p. 207.

ON PHILLYRINE. BY C. BERTAGNINI.

In the *Annalen der Chemie* for October, M. Bertagnini gives a preliminary account of some experiments with *Phillyrine*, by which he finds it to be a body analogous with salicine.

The phillyrine is obtained by treating a decoction of the bark of the *Phillyria* with oxide of lead or lime. After filtration and appropriate evaporation, the phillyrine separates out in crystalline form.

It is almost tasteless, and little soluble in cold water, soluble in hot water and alcohol, insoluble in æther; not precipitated from its solutions by salts of the metals.

On boiling it with dilute hydrochloric acid, a resinous matter separated. The liquid was then neutralized with carbonate of lead, filtered and evaporated: the mass was treated with alcohol, and the alcoholic solution evaporated in the water-bath, when a sweet syrup was obtained, which after some weeks changed into a papillary crystalline mass, with all the properties of grape-sugar.

Phillyrine was not acted upon by synaptase; but when submitted to the lactic fermentation, it gave, on the one hand, the usual products of the lactic fermentation of sugar, and on the other, the resinous substance obtained by the action of hydrochloric acid, but in this case pure. This is *Phillygenine.* It crystallizes easily, and gives pure white pearly masses. It is not soluble in cold, and only slightly so in warm water, but easily soluble in alcohol and æther. By concentrated sulphuric acid it is coloured red.

The analyses of phillyrine lead to the formula $C^{54} H^{34} O^{22} + 3HO$. At 100° C. it loses all its water of crystallization.

Phillygenine, on analysis, gave numbers corresponding with the formula $C^{42} H^{24} O^{12}$. Since phillyrine gives on decomposition only grape-sugar and phillygenine, its constitution must entirely correspond with that of salicine, and it must be formed by the union of two groups of atoms, with the separation of 2 equivs. of water:—

$$C^{42}\,H^{24}\,O^{12} + C^{12}\,H^{12}\,O^{12} = C^{54}\,H^{34}\,O^{22} + 2HO.$$

Phillygenine. Grape-sugar. Anhydrous phillyrine.

This equation is quite analogous to that which expresses the constitution of salicine:—

$$C^{14}\,H^{8}\,O^{4} + C^{12}\,H^{12}\,O^{12} = C^{26}\,H^{18}\,O^{14} + 2HO.$$

Saligenine. Grape-sugar. Salicine.

It is remarkable that phillygenine is polymeric with saligenine, for the formula of saligenine tripled gives exactly that of phillygenine:—

$$3C^{14}\,H^{8}\,O^{4} = C^{42}\,H^{24}\,O^{12}.$$

Saligenine. Phillygenine.

The action of chlorine and bromine on phillyrine is entirely analogous with their action on salicine.

The author is engaged in the study of these bodies.—*Ann. der Chem. und Pharm.*, vol. xcii. p. 109.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1854.

Chiswick.—November 1. Dense fog: very fine. 2. Foggy: hazy: very fine. 3. Fine: clear. 4. Overcast: fine. 5. Cloudy. 6. Clear and very fine. 7. Foggy: cloudy. 8. Overcast: fine. 9. Clear and cold: sharp frost: rain. 10. Fine, but cold: rain. 11. Slight rain: cloudy: fine. 12. Fine: frosty at night. 13. Foggy: fine. 14. Densely clouded: heavy rain. 15. Heavy rain: fine: rain. 16. Rain: foggy at night. 17. Foggy: rain: overcast. 18. Densely overcast and windy. 19. Overcast: clear and cold. 20. Cloudy: clear and cold: fine. 21. Overcast. 22. Clear: densely clouded: clear. 23. Fine: cloudy: sharp frost. 24, 25. Cloudy and cold. 26. Clear: overcast. 27. Foggy. 28. Overcast: rain. 29. Cloudy and fine. 30. Clear: rain at night.

Mean temperature of the month .... 39°·35
Mean temperature of Nov. 1853 .... 40 ·14
Mean temperature of Nov. for the last twenty-eight years ... 43 ·07
Average amount of rain in Nov. .... 2·38 inches.

Boston.—Nov. 1. Fine. 2. Cloudy. 3. Fine. 4. Fine: rain P.M. 5. Cloudy. 6, 7. Fine. 8. Cloudy. 9. Fine: snow and rain A.M. and P.M. 10. Fine: rain P.M. 11. Fine. 12. Fine: rain P.M. 13. Cloudy. 14. Cloudy: rain A.M. 15, 16. Rain A.M. and P.M. 17. Rain A.M. 18. Cloudy: rain P.M. 19. Fine: rain A.M. and P.M. 20. Cloudy. 21. Cloudy: rain A.M. and P.M. 22, 23. Cloudy. 24, 25. Cloudy: rain A.M. and P.M. 26, 27. Fine. 28. Cloudy: rain P.M. 29. Fine rain A.M. 30. Cloudy.

Sandwick Manse, Orkney.—Nov. 1. Showers A.M.: cloudy P.M. 2. Rain A.M: clear P.M. 3. Bright A.M.: showers P.M. 4. Cloudy A.M.: rain P.M. 5. Cloudy A.M. and P.M. 6. Cloudy A.M.: showers P.M. 7. Showers A.M. and P.M. 8. Showers A.M.: snow-showers P.M. 9. Snow-showers A.M.: sleet-showers P.M. 10, 11. Cloudy A.M.: clear P.M. 12, 13. Cloudy A.M. and P.M. 14. Cloudy A.M.: cloudy, drops P.M. 15. Showers A.M. and P.M. 16. Showers A.M: rain P.M. 17. Drizzle A.M.: damp P.M. 18. Fine, cloudy A.M.: showers P.M. 19. Fine, cloudy A.M.: clear P.M. 20. Damp A.M.: showers P.M. 21. Showers A.M. and P.M. 22. Rain A.M.: showers, aurora P.M. 23. Clear, frost A.M.: snow-showers frost P.M. 24. Snow A.M. and P.M. 25. Snow, thaw A.M.: thaw, rain P.M. 26. Fog A.M.: cloudy P.M. 27. Cloudy A.M.: showers, sleet P.M. 28. Showers A.M. and P.M. 29. Showers A.M.: sleet-showers P.M. 30. Clear A.M.: sleet-showers P.M.

Mean temperature of Nov. for twenty-seven previous years . 42°·67
Mean temperature of this month .... 42 ·05
Mean temperature of Nov. 1853 .... 44 ·87
Average quantity of rain in Nov. for thirteen previous years 4·25 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1854. Nov.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·361	30·352	29·90	29·74	29·90	61	32	40	53½	52½	sw.	s.	s.	....	·12	·03
2.	30·424	30·161	29·90	29·90	29·86	59	39	49	47½	45	se.	sw.	wnw.	....	....	·30
3.	30·265	30·215	29·77	30·04	30·08	52	25	40	42½	40	n.	nw.	nw.	....	....	·18
○ 4.	30·197	29·992	29·75	29·94	29·78	53	49	41	46½	49½	w.	nw.	wnw.	....	....	·05
5.	30·144	29·982	29·50	29·82	30·02	57	29	53	48	48	nw.	wnw.	wnw.	....	·07	·17
6.	30·359	30·301	29·92	30·16	30·19	53	25	36	48½	50	n.	nw.	nw.	....	....	....
7.	30·501	30·421	30·07	30·13	29·91	51	26	33	51	51	w.	nw.	w.	....	....	·04
8.	30·278	30·164	29·80	29·94	30·16	51	31	42	41	34	w.	wsw.	w.	·01	....	·63
9.	30·246	30·156	29·86	30·23	30·02	44	18	33·5	38	41½	n.	nw.	wnw.	·04	....	·19
10.	30·209	30·002	29·78	29·98	29·92	50	41	34	40½	37	w.	nw.	n.	·01	·09	·05
11.	30·126	29·994	29·66	30·10	30·20	49	28	41	37½	38	ne.	nw.	nnw.	·03	·18	....
☾ 12.	30·228	30·219	29·88	30·14	29·96	47	21	39	41½	44½	w.	nw.	ssw.	....	....	....
13.	30·088	29·828	29·70	29·71	29·58	51	36	35	44½	46	s.	s.	sse.	....	·02	....
14.	29·598	29·471	29·30	29·46	29·44	45	29	39	47	45	se.	s.	s.	·09	....	....
15.	29·116	29·056	28·87	29·38	29·42	53	30	40	43	41	sw.	se.	sse.	·34	·08	·16
16.	29·207	28·965	28·72	29·52	29·56	49	29	45	41½	42½	sw.	e.	e.	·02	·29	·13
17.	29·407	29·331	29·07	29·73	29·96	49	39	45	44½	42	ne.	ese.	se.	·08	·19	·38
18.	29·828	29·621	29·40	30·07	30·18	44	36	43	39	41	ne.	ne.	ne.	....	·04	·02
19.	30·084	29·993	29·74	30·26	30·31	44	32	39	41½	40	nw.	ne.	n.	....	·03	·02
● 20.	30·122	30·018	29·84	30·12	29·70	42	32	40	44	45	ne.	n.	nw.	....	·09	·02
21.	29·876	29·175	29·38	29·34	29·22	44	26	38	43	36½	sw.	ssw.	n.	·17	....	·14
22.	28·975	28·924	28·65	29·08	29·42	43	26	34	40	37½	w.	nw.	e.	....	·26	·41
23.	29·061	29·057	28·88	29·46	29·52	42	23	36	34	33	ne.	n.	ne.	....	....	·20
24.	29·266	29·186	28·98	29·60	29·72	42	30	36	33	33	ne.	n.	e.	....	·10	·05
25.	29·487	29·130	29·20	29·76	29·73	40	26	35	37½	39½	n.	nnw.	w.	....	·16	·17
26.	29·942	29·302	29·57	29·77	29·71	39	28	30	37½	44	ne.	w.	w.	....	·07	·31
☽ 27.	29·932	29·797	29·60	29·38	29·12	34	29	27·5	44	42	sw.	n.	w.	....	....	·02
28.	29·702	29·371	29·33	29·23	28·66	49	42	39	40½	46	sw.	w.	w.	·24	....	·41
29.	29·199	28·990	28·63	28·52	28·96	50	29	43	39	34	w.	w.	wnw.	·06	·05	·64
30.	29·516	29·123	29·14	29·01	28·86	51	37	37	39½	35	w.	nw.	wnw.	·22	·07	·10
Mean.	29·858	29·676	29·46	29·717	29·702	47·93	30·77	38·8	42·30	41·80				1·31	1·91	4·82

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[FOURTH SERIES.]

FEBRUARY 1855.

XIII. *On some Points of Magnetic Philosophy.*

By Professor FARADAY, *D.C.L., F.R.S. &c.*[*]

3300. WITHIN the last three years I have been bold enough, though only as an experimentalist, to put forth new views of magnetic action in papers having for titles, "On Lines of Magnetic Force[†]," and "On Physical Lines of Magnetic Force[‡]." The first paper was simply an attempt to give, for the use of experimentalists and others, a correct expression of the dual nature, amount, and direction of the magnetic power both within and outside of magnets, apart from any assumption regarding the character of the source of the power; that the mind, in reasoning forward towards new developments and discoveries, might be free from the bondage and deleterious influence of assumptions of such a nature (3075. 3243.). The second paper was a speculation respecting the possible physical nature of the force, as existing outside of the magnet as well as within it, and within what are called magnetic bodies, and was expressly described as being entirely hypothetical in its character (3243.).

3301. There are at present two, or rather three general hypotheses of the physical nature of magnetic action. First, that of æthers, carrying with it the idea of fluxes or currents, and this Euler has set forth in a simple manner to the unmathematical philosopher in his Letters[§];—in that hypothesis the magnetic fluid or æther is supposed to move in streams through magnets, and also the space and substances around them. Then there is the hypothesis of two magnetic fluids, which being present in all magnetic bodies, and accumulated at the poles of a magnet, exert attractions and repulsions upon portions of both fluids at a distance, and so cause the attractions and repulsions of the distant bodies containing them. Lastly, there is the hypothesis of Ampère, which assumes the existence of electrical currents round the particles of magnets, which currents, acting at a distance upon other particles having like currents, arranges them in the masses to which they belong, and so renders such masses subject to the magnetic action. Each of these ideas is varied more or less by different philosophers, but the three distinct expressions of them which I have just given will suffice for my present purpose. My physico-hypothetical notion does not go so far in assumption as the second and third of these ideas, for it does not profess to say how the magnetic force is originated or sustained in a magnet; it falls in rather with the first view, yet does not assume so much. Accepting the magnet as a centre of power surrounded by lines of force, which, as representants of the power, are now justified by mathematical analysis (3302.), it views these lines as *physical* lines of power, essential both to the existence of the force within the magnet, and to its conveyance to, and exertion upon, magnetic bodies at a distance. Those who entertain in any degree the æther notion might consider these lines as currents, or progressive vibrations, or as stationary undulations, or as a state of tension. For many reasons they should be contemplated round a wire carrying an electric current, as well as when issuing from a magnetic pole.

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[*] Communicated by the Author.

[†] Phil. Trans. 1852, p. 25.

[‡] Phil. Mag. 1852, June, p. 401.

[§] Euler's Letters, translated, 1802, vol. i. p. 214; vol. ii. pp. 240. 242. 244.

3302. The attention of two very able men and eminent mathematicians has fallen upon my proposition to represent the magnetic power by lines of magnetic force; and it is to me a source of great gratification and much encouragement to find that they affirm the truthfulness and generality of the method of representation. Professor W. Thomson, in referring to a like view of lines of force applied to static electricity (1295. 1304.), and to Fourier's law of motion for heat, says that the lines of force give the same mathematical results as Coulomb's theory, and by more simple processes of analysis (if possible) than the latter*; and afterwards refers to the "strict foundation for an analogy on which the *conducting power of a magnetic medium for lines of force* may be spoken of†." Van Rees has published a mathematical paper on my lines of force in Dutch‡, which has been transferred into Poggendorff's *Annalen**, and of which I have only a very imperfect knowledge by translated abstracts. He objects, as I understand, to what I may call the physical part of my view as assigning no origin for the lines, and as not presenting the higher principle conveyed by the idea of magnetic fluids or of electric currents: he says it does not displace the old theories, or render them superfluous; but I think I am right in believing, that, as far as the lines are taken to be representations of the power, he accepts them as correct representations, even to the full extent of the hypotheses, either of magnetic fluids or electric currents. It was always my intention to *avoid* substituting anything in place of these fluids or currents, that the mind might be delivered from the bondage of preconceived notions; but for those who desire an idea to rest upon, there is the old principle of the æthers.

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* Phil. Mag. 1854, vol. viii. p. 53.

† Ibid. p. 56.

‡ Trans. Royal Acad. Sciences of Amsterdam, 1854, p. 17.

3303. The encouragement I derive from this appreciation by mathematicians of the mode of figuring to one's self the magnetic forces by lines, emboldens me to dwell a little more upon the further point of the true but unknown natural magnetic action. Indeed, what we really want, is not a variety of different methods of representing the forces, but the one true physical signification of that which is rendered apparent to us by the phænomena, and the laws governing them. Of the two assumptions most usually entertained at present, magnetic fluids and electric currents, *one* must be wrong, perhaps *both* are; and I do not perceive that the mathematician, even though he may think that each contains a higher principle than any I have advanced, can tell the true from the false, or say that either is true. Neither of these views could have led the mind to the phænomena of diamagnetism, and I think not to the magnetic rotation of light; and I suppose that if the question of the possibility of diamagnetic phænomena could have been asked beforehand, a mathematician, guided by either hypothesis, must have denied that possibility. The notion that I have introduced complicates the matter still further, for it is inconsistent with either of the former views, so long as they depend exclusively upon action at a distance without intermediation; and yet in the form of lines of force it represents magnetic actions truly in all that is not hypothetical. So that there are now three fundamental notions, and *two* of them at least must be impossible, *i. e.* untrue.

3304. It is evident, therefore, that our physical views are very doubtful; and I think good would result from an endeavour to shake ourselves loose from such preconceptions as are contained in them, that we may contemplate for a time the force as much as possible in its purity. At present we cannot think of polarity without feeling ourselves drawn into one or the other of the two hypotheses of the origin of polar powers; and as mathematical considerations cannot give a decision, we feel as if the subject were in that same doubtful condition which hung over the conflicting theories of light prior to the researches of modern time; but as there the use of Wheatstone's reflector, combined with Arago's suggestion of a decisive experiment, and its realization by Leon Foucault, appear to have settled that question, so we may hope by a due exertion of judgement, united with experiment, to obtain a resolution of the magnetic difficulty also.

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* Poggendorff's *Annalen*, 1853, vol. xc. p. 415.

3305. If we could tell the *disposition* of the force of a magnet, first at the place of its origin, and next in the space around, we should then have attained to a very important position in the pursuit of our subject; and if we could do that, assuming little or nothing, then we should be in the very best condition for carrying the pursuit further. Supposing that we imagine the magnet a sort of sun (as there is every reason to believe that the sun is a magnet) polarized, with antithetical powers, ever filling all space around it with its curved beams, as either the sun or a candle fills space with luminous rays; and supposing that such a view takes equal position with either of the two former views in representing truly the disposition of the forces, and that mathematical considerations cannot at present decide which of the three views is either above or inferior to its co-rivals; it surely becomes necessary that physical reasoning should be brought to bear upon the subject as largely as possible. For if there be such physical lines of magnetic force as correspond (in having a real existence) to the rays of light, it does not seem so very impossible for experiment to touch *them*; and it must be very important to obtain an answer to the inquiry respecting their existence, especially as the answer is likely enough to be in the affirmative. I therefore purpose, without asserting anything regarding the physical hypothesis of the magnet more strongly than before (3299.), to call the attention of experimenters, in a somewhat desultory manner, to the subject again, both as respects the deficiency of the present physical views and the possible existence of lines of physical force, concentrating the observations I may have to make about a few points—as *polarity*, *duality*, &c., as occasion may best serve; and I am encouraged to make this endeavour by the following considerations. 1. The confirmation by mathematicians of the truthfulness of the abstract lines of force in representing the direction and amount of the magnetic power;—2. My own personal advantageous use of the lines on numerous occasions (3174.);—3. The close analogy of the magnetic force and the other dual powers, either in the static or dynamic state, and especially of the magnet with the voltaic battery or any other sustaining source of an electric current;—4. Euler's idea of magnetic æthers or circulating fluids; —5. The strong conviction expressed by Sir Isaac Newton, that even gravity cannot be carried on to produce a distant effect except by some interposed agent* fulfilling the conditions of a physical line of force;—6. The example of the conflict and final experimental settlement of the two theories of light.

3306. I believe that the use by me of the phrase "places of force" has been considered by some as objectionable, inasmuch as it would seem to anticipate the decision that there are physical lines of force. I will endeavour so to use it, if necessary, as not to imply the assertion. Nevertheless I may observe, that we use such a phrase in relation to a ray of light, even in those parts of the ray where it is not extinguished, and where therefore we have no better knowledge of it or its existence than in similar magnetic cases; and we also use the phrase when speaking of gravity in respect of places where no second body to gravitate upon is present, and where, when existing, it cannot, according to our present views, cause the gravitating force of the primary body, or even the determination of it, upon that particular place.

Magnetic polarity.

3307. The meaning of this phrase is rapidly becoming more and more uncertain. In the ordinary view, polarity does not necessarily touch much upon the idea of lines of physical force; yet in the one natural truth it must either be essential to, and identified with it, or else absolutely incompatible with, and opposed to it. Coulomb's view makes polarity to depend upon the resultant in direction of the action of two separated and distant portions of two magnetic fluids upon other like separated portions, which are either originally separate, as in a magnet, or are induced to separate, as in soft iron, by the action of the dominant magnet;—it is essential to this hypothesis that the polarity force of one name should repel polarity force of the same name and attract that of the other name. Ampère's view of polarity is, that there are no magnetic fluids, but that closed currents of electricity can exist round particles of matter (or round masses), and that the known experimental difference on the opposite sides of these currents, shown by attraction and repulsion of other currents, constitutes polarity. Ampère's view is modified (chiefly by addition) in various ways by Weber, De la Rive, Matteucci, and others. My view of polarity is founded upon the character in direction of the force itself, whatever the cause of that force may be, and asserts that when an electro-conducting body moving in a constant direction near or between bodies acting magnetically on themselves or each other, has a current in a constant direction produced in it, the magnetic polarity is the same; if the motion or the current be reversed, the contrary polarity is indicated. The indication is true either for the exterior or the interior of magnetic bodies whenever the electric current is produced, and depends upon the unknown but essential dual or antithetical nature of the force which we call magnetism (3154.).

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* Newton says, "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers." See the third letter to Bentley.

3308. The numerous meanings of the term polarity, and various interpretations of polarity indications at present current, show the increasing uncertainty of the idea and the word itself. Some consider that the mere set or attraction, or even repulsion, shown by a body when subject to a dominant magnet is sufficient to mark polarity, and I think it is as good a test as any more refined arrangement (2693.) when the old notion of polarity only is under consideration. Others require that two bodies under the power of a dominant magnet should by their actions show a mutual relation to each other before they can be considered as polar. Tyndall, without meaning to include any idea of the nature of the magnetic force, takes his type from soft iron, and considers that any body presenting the like or the antithetical phænomena which such iron would present under magnetic action, is in a like or antithetical state of polarity*. Thomson does not view two bodies which present these antithetical positions or phænomena as being necessarily the reverse of each other in what may be called their polar states†, but, I think, looks more to differential action, and in that approaches towards the views held generally by E. Becquerel and myself. Matteucci considers that the whole mass of the polar body ought to be in dependence by its particles as a mass of iron is, and that a solution of iron and certain salts of iron have not poles, properly speaking, but that at the nearest points to the dominant pole there is the contrary magnetism to that of the pole, surrounded by the same magnetism as of the pole in the further part, the two ends of a bar of such matter between two dominant poles having no relation to each other*. Becquerel considers that polarity may in certain cases occur transverse to the length, and so produce results which others explain by reverse polarity. The views of very many parties always include the idea of the source of the polar action, whether that be supposed to depend on the accumulation of magnetic fluids at the chief poles of the dominant magnet, or the action of electric currents in a determinate position around its molecules; and such views are adhered to even when the polarity induced is of the reverse kind, as in bismuth, &c., to that of the inducing magnet. Others, like Weber, add to Ampère's hypothesis an idea of electricity, loose as regards the particles, though inseparably associated with the mass of the body under induction. Some, I think, make the polarity not altogether dependent upon the dominant magnet, but upon the neighbouring or surrounding substances; and I propose, if the physical lines of force should hereafter be justified, to make that which is commonly called polarity, in distinction from the true polarity (3307.), dependent upon the better or worse magneto-conduction power of the substances presenting the usual polar phænomena (2818.).

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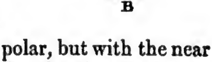
* Athenæum, No. 1406, p. 1203.

† Ibid. column 3 at bottom.

3309. The views of polar action and of magnetism itself, as formerly entertained, have been powerfully agitated by the discovery of diamagnetism. I was soon driven from my first supposition, that the N pole of a magnet induced like or N polarity in the near part of a piece of bismuth or phosphorus; but as that view has been sustained by very eminent men, who tie up with it the existence of magnetic fluids or closed electric currents as the source of magnetic power, it claims continued examination, for it will most likely be a touchstone and developer of real scientific truth, whichever way the arguments may prevail. To me the idea appears to involve, if not magnetic impossibilities, at least great contradiction and much confusion, some of which I proceed to state, but only with the desire of elucidating the general subject.

3310. If an ordinary magnet M, fig. 1, acting upon a piece of iron or other paramagnetic matter I, renders it polar by throwing its near end into the contrary or S state in the manner usually understood, and, acting upon a like piece of diamagnetic matter as bismuth B, renders it also polar, but with the near end in the same state; then B and I are for the time two magnets, and must act back upon the magnet M; or if they could be made able to retain their states after M is removed (and that is the case with I), would act as magnets upon a third piece of

Fig. 1.



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* *Cours spécial sur l'induction, &c.*, p. 201.

magnetic matter as C. When M acts upon I, it exerts its influence, according to the received theories, upon all the particles of the latter, bringing them into like polar position with itself, and these, consistently with the simple assumption, act also upon each other as particle magnets, and exalt the polarity of the whole mass in its two extremities. In like manner M should act upon B, polarizing the mass and all its particles; for the particles of the diamagnetic body B, even to the smallest, must be operated upon; and we know experimentally, that a tube filled with powdered bismuth acts as a bar of the metal does. But then what is the mutual action of these bismuth particles on each other? for though all may be supposed to have a reverse polarity to that of M, they cannot in that case be reverse in respect of each other. All must have like polarity, and the N of one particle must be opposed to the S of the next particle in the polarity direction. That these particles act on each other, must be true, and Tyndall's results on the effect of compression have proved that by the right means, namely, experiment. If they were supposed to have no such action on each other, it would be in contradiction to the essential nature of magnetic action, and there would remain no reason to think that the magnet itself could act on the particles, or the particles react on it. If they acted on each other as the magnet is supposed to act on them, *i. e.* to induce contrary poles, then the power of the magnet would be nullified, and the more effectually the nearer the particles were together; whereas Tyndall has shown that the bismuth magnetic condition is exalted by such vicinity of the particles, and hence we have a further right to conclude that they do act on, or influence each other, to the exaltation of the state of the mass. But if the N-ness of one particle corresponds to, and aids in sustaining and exalting, the S-ness of the next particle, the whole mass must have the same kind of force; so that, as a magnet, its polarity must have the same kind of polarity as that of the particles themselves. For whether a particle of bismuth be considered as acting upon a neighbouring particle or upon a distant particle of bismuth, or whether a mass of particles be considered as acting on the distant particle, the action in both cases must be precisely of the same kind.

3311. But why should a polarized particle of bismuth acting upon another particle of bismuth produce in it like polarity, and with a particle of iron produce a contrary polarity? or why should masses of bismuth and iron, when they act as magnets (3310.), produce such different effects? If such were the case, then the N pole of a paramagnetic body would induce an S pole on the near end of an iron rod, whilst the N pole of a diamagnetic body would produce a pole contrary to the former, *i. e.* an

N pole at the same end of the iron rod in the same position and place. This would be to assume two kinds of magnetism, *i. e.* two north fluids (or electric currents) and two south; and the northness of bismuth would differ from the northness of iron as much as pole from pole. Still more, the northness of bismuth and the southness of iron would be found to have exactly like qualities in all points, and to differ in nothing but name; and the southness of bismuth and northness of iron would also prove to be absolutely alike. What is this, in fact, but to say they *are* the same? and why should we not accept the confirmation and unfailing proof that it is so, which is given to us experimentally by the moving wire? (3307. 3356.)

3312. If we employ a magnet as the originally inducing body (3310.), and entertain the idea of magnetic fluids accumulated at the poles, which act by their power of attracting each other, but repelling their like, then the inconsistency of supposing that the north fluid of a given pole can attract the north fluid of one body and the south fluid of another, or that the north and south fluids of the dominant magnet can attract one and the same fluid in bismuth and in iron, &c., is very manifest. Or if we act by a solenoid or a helix of copper wire carrying an electric current instead of a magnet, and find that analogous effects are produced, are we to admit at once that the electric currents in it, acting upon the assumed electric circuits round the particles of matter, sometimes attract them on the one side and sometimes on the other? or if such bodies as bismuth and platinum are put into such a helix, are we to allow that currents in opposite directions are induced in them by one and the same inducing condition? and that, too, when all the other phænomena, and there are many, point to a uniformity of action as to direction with a variation only in power.

Media.

3313. Let us now consider for a time the action of different *media*, and the evidence they give in respect of polarity. If a weak solution of protosulphate of iron*, *m*, be put into a selected thin glass tube about an inch long, and one-third or one-fourth of an inch in diameter, and sealed up hermetically (2279.), and be then suspended horizontally between the magnetic poles in the air, it will point axially, and behave in other respects as iron; if, instead of air between the poles, a solution of the same kind as *m*, but a little stronger, *n*, be substituted, the solution in the tube will point equatorially, or as bismuth. A like solution somewhat weaker than *m*, to be called *l*, enclosed in a similar tube, will behave like bismuth in air but like iron in water. Now these are precisely the actions which have been attributed to polarity, and by which the assumed reverse polarities of paramagnetic and diamagnetic bodies have been considered as established; but when examined, how will ideas of polarity apply to these cases, or they to it? The solution *l* points and acts like bismuth in air and like iron in water; are we then to conclude that it has reverse polarity in these cases? and if so, what are the reasons and causes for such a singular contrast in that which must be considered as dependent upon its internal or molecular state?

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* Let *l* contain 4 grains, *m* 8 grains, *n* 16 grains, and *o* 32 grains, of crystallized protosulphate of iron in each cubic inch of water.

3314. In the first place, no want of magnetic continuity of parts can have anything to do with the inversion of the phænomena; for it has been shown sufficiently by former experiments*, that such solutions are as magnetically continuous in character as iron itself.

3315. In the next place, I think it is impossible to say that the medium interposed between the magnet and the suspended cylinder of fluid can cut off, or in any way affect the direct force of the former on the latter, so as to change the direction of its internal polarity. Let the tube be filled with the solution *m*, then if it be surrounded by the solution *l*, it will point as iron; if the stronger solution *n* surround it, it will point as bismuth; and with sufficient care a succession of these fluids may be arranged as indicated in figs. 2, 3, where the outlines between the poles represent the forms of thin glass troughs, and the letters the solutions in them. In fig. 2 we see that the action on *m* is the same as that on *m'*, and the pointing of the two portions is the same, *i. e.* equatorial; neither has the action on *m* been altered by the power of the poles having to traverse *n*, *m'* and *n'*; and in fig. 3 we see, that, under like circumstances of the power, *m'* points as bismuth and *m* as iron, though they are the same solution with each other and with the former *m m'* solutions. No cutting off of power by the media could cause these changes;—repetitions of position in the first case, and inversions in the second. All that could be expected from any such interceptions would be perhaps diminutions of action, but not inversions of polarity; and every consideration indicates that all the portions of these solutions in the field at once have *like polarity*, *i.e.* like direction of force through them, and like internal condition;

Fig. 2.

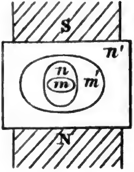
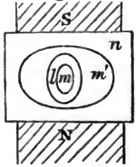


Fig. 3.



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* Phil. Mag. 1846, vol. xxix. p. 254.

each solution in its complex arrangement being affected exactly in the same way and degree as if it filled the whole of the magnetic field, although in these particular arrangements it sometimes points like iron, and at other times like bismuth (2362. 2414.).

3316. These motions and pointings of the same or of different solutions, contain every action and indication which is supposed to distinguish the contrary polarities of paramagnetic and diamagnetic bodies from each other, and the solutions *l* and *m* in air repeat exactly the phænomena presented in air by phosphorus and platinum, which are respectively diamagnetic and paramagnetic substances. But we know that these actions are due to the differential result of the masses of the moving or setting solution and of that (or the air) surrounding it. No structural or internal polarity, having opposite directions, is necessary to account for them (2361. 2757.). If, therefore, it is still said that the solution *m* has one polarity in *l* and the reverse polarity in *n*, that would be to make the polarity depend upon the *mass* of *m* independently of its particles; for it can hardly be supposed that the particles of *m* are more affected by the influence upon them of the surrounding medium (itself under like inductive action only, and almost insensible as a magnet), than they are by the dominant magnet*. It would be also to make the polarity of *m* as much, or more, dependent upon the surrounding medium than upon the magnet itself;—and it would be, to make the masses of *m* and *l* and even their *form* the determining cause of the polarity; which would remove polarity altogether from dependence upon internal molecular condition, and, I think, destroy the last remains of the usual idea. For my own part, I cannot conceive that when a little sphere of *m* in the solution *l* is attracted upon the approach of a given magnetic pole, and repelled under the action of the same pole when it is in the solution *n*, its particles are in the two cases polar in two opposite directions; or that if for a north magnetic pole it is the *near* side of the particles of *m* when in *l* that assume the south state, it is the *further* side which acquires the same state when the solution *l* is changed for *n*. Nor can I think that when the particles of *m* have the same polar state in both solutions, the whole, as a mass, can have the opposite states.

3317. These differential results run on in one uninterrupted course from the extreme of paramagnetic bodies to the extreme of diamagnetic bodies; and there is no substance within the series which, in association with those on each side of it, may not be made to present in itself the appearances and action which are considered as indicating the opposite polarities of iron and bismuth. How then is their case, in the one or the other condition, to be distinguished from the assumed polarity conditions of bismuth or of iron?—only, I think, by assuming other points which beg the whole question. In the first place, it must be, or is assumed, that no magnetic force exists in the space around a magnet when it is in a vacuum, it being denied that the power either crosses or reaches a locality in that space until some material substance, as the bismuth or iron, is there. It is assumed that the space is in a state of magnetic darkness (3305.), an assumption so large, considering the knowledge we have of natural powers, and especially of dual forces, that there is none larger in any part of magnetic or electric science, and is the very point which of all others should be held in doubt and pursued by experimental investigation. It is as if one should say, there is no light or form of light in the space between the sun and the earth because that space is invisible to the eye. Newton himself durst not make a like assumption even in the case of gravitation (3305.), but most carefully guards himself and warns others against it, and Euler* seems to follow him in this matter. Such an assumption, however, enables the parties who make it to dismiss the consideration of differential effects when bodies are placed in a vacuum, and to divide the bodies into the well known double series of paramagnetic and diamagnetic substances. But in the second place, even then, those who assume the reverse polarity of diamagnetic bodies, must assume also that the state set up in them by conduction is less favourable to either the exercise or the transmission of the magnetic force than the original unpolarized state of the bismuth; an assumption which is, I think, contrary to the natural action and final stable condition into which the physical forces tend to bring all bodies subject to them. That a magnet acting on a piece of iron should so determine and dispose of the forces as to make the magnet and iron mutually accordant in their action, I can conceive; but that it should throw the bismuth into a state which would make it repel the magnet, whereas if unaffected it should be so far favourable as to be at least indifferent, is what I cannot imagine to myself. In the third place, those who rest their ideas on *magnetic fluids*, must assume that in all diamagnetic cases, and in them only, the fundamental idea of their mutual action must not only be set aside but inverted, so that the hypothesis would be at war with itself; and those who assume that *electric currents* are the cause of magnetic effects, would have to give up the law of their inducing action (as far as we know it) in all cases of diamagnetism, at the very same moment when, if they approached the diamagnetic bismuth in the form of a spiral to the pole, they would have a current produced in it *according* to that law.

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* If the polarity of the inner mass of solution is dependent upon that of the outer, and cannot be affected but through it, then why is not air and space admitted as being in effective magnetic relation to the bodies surrounded by them? How else could a distant body be acted upon by a magnet, if the inner solution of sulphate of iron is so acted on? Are we to assume one mode of action by contiguous masses or particles in one case, and another through distance in another case?

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* Letters, &c. translated. Letter LXVIII., or pp. 260–262.

Time.

3318. I will venture another thought or two regarding the condition into which diamagnetic bodies are brought by the act of magnetic induction, in connexion with the point of *time*. It appears, as far as I remember, that all natural forces tend to produce a state of rest, except in cases where vital or organic powers are concerned; and that as in *life* the actions are for ever progressive, and have respect to a future rather than a present state (Paget), so all *inorganic* exertions of force tend to bring about a stable and permanent condition, having as the result a state of rest, *i. e.* a static condition of the powers.

3319. Applying this consideration to the case of bismuth in the magnetic field, it seems to me more like the truth of nature that the state assumed by the bismuth should be one more favourable to the final and static exercise of the power of the dominant magnet upon it, than that state belonging to the bismuth before it had suffered or undergone the induction; exactly as in soft iron we know that before it has acquired the state which a dominant magnet can induce upon it, it is not so favourable to the final static condition of the powers as it is afterwards. Now it is very manifest, by numerous forms of experiment, that *time* enters as an element into ordinary magnetic and magneto-electric actions, and there is every reason to expect into diamagnetic actions also; and it is also well known that we can take advantage of this time, and test the state of a piece of iron in the magnetic field before it has attained its finally induced state, and afterwards;—as, for instance, by placing it with a helix round it in the magnetic field and quickly connecting the helix *afterwards* with a galvanometer, when a current of electricity in such direction as to prove the truth of the statement will be obtained. In other forms of experiment, and with large pieces of iron, the time which can be so separated or snatched up during the act of progressive induction will amount to a minute or more. Supposing this could be done in any sensible degree with diamagnetic bodies, then the following considerations present themselves. A globe or bar of bismuth in the magnetic field may have its states, before and after induction, considered as separated by a moment of time; if the induction raises up a state of polarity the reverse of that of the magnet, then the bismuth ought to be more favourable to the determination of magnetic force upon it *before* the induction than *after*; whereas if, according to my view, the polarity is not reversed, but is the same as that of the magnet, the metal ought to be more favourable to the determination of magnetic force upon or through it *after* induction than *before*. Believing this to be an experiment which would settle the question of reverse polarity, and perhaps the existence or non-existence of physical lines of magnetic force, I have made many attempts in various ways, and especially by alternating motions of cylinders and balls of bismuth between soft iron magnetic poles furnished with helices, to obtain some results due to the *time* of induction, but have been as yet unable to succeed. I cannot doubt that time is concerned; but it seems to be so brief in period as to be inappreciable by the means I have employed.

3320. Professor Thomson has put this matter of time and polarity in another form. If a globe of bismuth be placed without friction in the middle of the magnetic field, it will not point or move because of its shape; but if it have reverse polarity, it will be in a state of unstable equilibrium; and if *time* be an element, then the ball, being once moved on its axis ever so little, would then have its polarity inclined to the magnetic axis, and would go on revolving for ever, producing a perpetual motion. I do not see how this consequence can be avoided, and therefore cannot admit the principles on which it rests. The idea of a perpetual motion produced by static forces is philosophically illogical and impossible, and so I think is the polarly opposed or adverse static condition to which I have already referred.

3321. It is not necessary here that I should refer to the manner in which my view of the lines of magnetic force meet these cases, for it has been done in former papers (2797, &c.); but I will call the attention of those who like to pursue the subject, to a true case of reverse polarity in the magnetic field (Experimental Researches, 3238, fig. 15), and there they will easily see and comprehend the beginning of the rotation of Professor Thomson's bismuth globe, and its continuance, if, as supposed, the polar state represented in the figure could be continually renewed.

3322. When the north pole of a magnet repels a piece of bismuth in a vacuum, or makes a bar of it set equatorially, and is found to produce like actions with many paramagnetic bodies when surrounded by media a little more paramagnetic than themselves, and with as many diamagnetic bodies when surrounded by media a little less diamagnetic, it would seem more cautious in the first instance to inquire how these latter motions take place, and how it is that parts, which with the paramagnetics have certainly been brought into a south condition by the north end of the pole, recede from it; and to apply these results in the first instance to those obtained with bismuth in a vacuum, before we assume a total change in principle, and yet an exceptional change as to substances, in the general law of magnetic polarity, without any cause assigned than, or any supporting facts beyond, the effect in question.

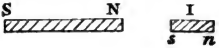
Curved lines of magnetic force—dependence of the dualities.

3323. The representative idea of lines of magnetic force which I entertain, includes in it the thought of the curvature of these lines, not as a merely convenient notion making the idea of the lines more manageable, but as one flowing from and suggested, if not proved, by the phænomena themselves. It is in this point of view that I proceed to consider it; and as the proof of the curvature is, in respect of *principle*, in the essential and necessary dependence of the two qualities or parts of a dual force upon each other (3324, &c.); and in respect of *experiment*, by the numerous results supplied during the mutual actions of magnets and magnetic bodies and the phænomena of moving conductors (3337, &c.), I will consider each in turn.

3324. There is no known case of one form or part of a dual power existing otherwise than with, and in dependence on, the other, which then exists simultaneously to an equivalent, *i. e.* an equal, degree. In static electricity, where supposed electric fluids are considered as being separated from each other, they are in equal amount (1177.), are ever related to each other (1681.), often by curved lines of force (1215.), and the existence of the one electricity without the other, or in the smallest degree of excess or deficiency, is absolutely impossible (1174.). In the voltaic battery, or in the electric current produced in any other way, as by thermo arrangements or inductions, the current in one part of the circuit is absolutely the same in amount and in dual character as in another; and in the insulated, unconnected voltaic battery, where the sustaining power is internal, not the slightest development of the forces, or of either of them, can occur until circuit is completed, or induction allowed at the extremities; for if, when there is no circuit, the induction be prevented, not merely no current, but no stock of electricity at the battery poles ready to produce a current can be evolved in the slightest degree. In like manner I am fully persuaded that the northness and southness of magnetism (in whatever they may be supposed to consist) cannot exist alone;—nor without exact proportion to each other;—nor without mutual dependence upon each other;—but that they are subject to the mutual relation and dependence of all dual force.

3325. Let us consider a hard invariable magnet in space, fig. 4. If a piece of soft iron, I, be brought towards it, the N end of the magnet will cause southness in the near end of the iron and northness in the further end, and this will continue until the iron is removed, the southness and northness at the two ends or halves of the magnet having remained all the time unchanged in their equality and amount (3223. 3221.). Now to say that the force emanating from N could act on the iron, producing like and the contrary force, and then, by removal of the iron, cease to act there *or elsewhere*; and then again act on the iron if approached, or anything else, and then cease to act, and so on; would in my mind be to deny the *conservation of force*:—and we know that there is no equivalent action within the magnet, to explain by any alternate excitement and suppression of the dual parts, any supposed appearance and disappearance of the powers at the different times; for a helix closely applied round the middle part of the magnet during the experiment gives no current, and by that shows that there is no equivalent *internal* derangement of the power, when the outer exercise of it may be supposed to change between active and inert.

Fig. 4.

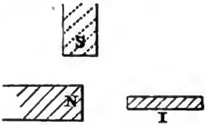


3326. Suppose the power of such a magnet to be due to magnetic N and S fluids; can it be thought that the N particles can be sometimes exerting their attraction for S particles, and sometimes not? Would not that be equivalent to the assumption of a suppression, *i. e.* a destruction of force?—which surely cannot be. Such an assumption could be surpassed only by that which supposes that the N fluid might sometimes attract S and repel N, and at other times repel S and attract N fluids (3311. 3312. 3317.).

3327. As to the soft iron under induction (3325.), its dual magnetic forces do re-enter into their former mutually dependent and mutually satisfying state: but suppose it to be replaced by steel, and that the magnetisms produced in it do not recombine or disappear on the removal of the dominant magnet, then on what is their power ultimately turned, *if not on each other*? (3257. 3324.) Where is the S power of the steel disposed of when it is separated from its relation with the N power of the magnet that evolved it? The case cannot be met except by affirming the independent existence of the two powers (3329.); or, admitting the suppression of force, and of either of these forces the one without the other (3330.); or allowing the mutual dependence of the two polarities of the magnet (3331.).

3328. When the N pole of a magnet (fig. 5) is acting in

Fig. 5.



free space, its force is sensible around to a certain amount (114.); when a piece of soft iron, I, is brought near it, much of its force gathers up upon that iron, but the whole amount of force from and about the N pole is the same; when an S pole is brought up, either of another magnet or of itself (for the effect is precisely the same), much of the force exerted upon the iron is removed from it, and falls upon the S pole, but the amount of force about the pole N remains the same; all of which can be proved experimentally by a helix on the soft iron and loops carried over the N pole (3218. 3223.). Indeed the way in which the power of one pole over either iron or bismuth is affected and diminished by the approximation on the same side of a contrary pole, is perfectly well known, and there are hundreds of cases in which the disposition in direction of the magnetic power can be varied in a great variety of ways, without the slightest change in the *sum* of its amount at the source, each of which gives evidence of the antithetical and inseparable condition of the two forms of force.

3329. As to independent existence of the two powers (3327.), how is it then that they cannot be shown separately?—not even up to the degree which is exhibited, so to say, by static electricity. There is nothing like a charge of northness or a charge of southness in any one of the innumerable phænomena presented by magnetism (3341.). The two are just as closely connected as the two electricities of a voltaic battery; whether we consider it as giving the current when properly connected, or exhibiting induction at its extremities when unconnected. The difficulty, indeed, is to find a fact which gives one the least hold for consideration of the thought that the two magnetic forces can be separated, or considered apart from each other.

3330. As to the suppression of force (3327.), I conceive that the creation, annihilation, or suppression of force, and still more emphatically of one form only of a dual force, is as impossible as the like of matter. All that is permitted under the general laws of nature is to displace, remove, and otherwise employ it; and these conditions are as true of the smallest suppression of a force, or part of a force, as of the suppression of the whole. I may further ask, whether, as it is *physically* impossible to annihilate or suppress force, it is not also *mathematically* impossible to do so, consistently with the law of the conservation of force?

3331. If we say that the forces in the cases of removal (3327.) are disposed of, sometimes in one direction and sometimes in another, but with the preservation of their full and equivalent amount, then how are we to consider them disposed of in the case of a cylinder or globular magnet, placed in air or *vacuo*, so as to be entirely self-dependent?—or in the case of a magnetic sphere placed in an inverted position in the magnetic field, so as to be entirely surrounded and enclosed by magnetic forces having a contrary direction to its own (3321. 3238.)?

3332. If we say that the dualities of such a magnet are dependent on each other (which is the third case (3327.), then we have to consider how this can be, consistently with the distant mutual action, either of magnetic fluids or electric currents, acting in right lines only. Such action must then be through the body of the magnet (3260.). If we confine our attention to magnetic fluids, then the direction of their forces towards each other through the magnet when it is alone, must be of the like nature as their direction to approached iron, in which they are supposed to induce collections of the contrary fluids, or towards the fluids at the contrary poles of approached equal or superior magnets; *i. e.* the two poles of the magnet must be conceived of as centres of force, sometimes exerting their power towards each other in a given direction through the body of the magnet, and at other times exerting them outwardly to external poles in a direction exactly the contrary. But the currents which are evolved by the rotation of the magnet, or of discs of metal combined with it (3119. 3163.), show that the direction of the force (which is its polarity) is not thus reverse in the two halves of the case, but is the same within the magnet as in the prolongation of direction through and beyond the pole; and also, that whether the magnet be alone, and therefore supposed to have the polar forces exerted on each other through it, or be in relation to outer magnets, so as to have this exertion of force entirely removed from its interior, still it is always the same; having in both cases the same condition, direction, and amount of power within it (3116.).

3333. If the charged and polar state of the magnet be supposed to depend upon molecular electric currents, held by some internal condition in a position of parallelism, it is impossible that these can act backwards upon each other through the magnet in straight lines, so as to put the northness and southness of the pole in mutual dependence, as they are supposed to be in relation to external poles, without the currents themselves being *displaced and turned*, until the whole magnet is neutralized; falling back into the undeveloped state, just as a piece of soft iron falls back. When this return of state happens in soft iron or steel in any degree, a helix round these show the induced currents consequent on such a change; and a loop (3133. 3217.)

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shows the difference when the iron or magnet is polar outwards and when its state has fallen. No such effects happen with a hard magnet, when it is alternately left to itself or put in relation to external poles of other magnets. The body of the magnet, and the forces passing through it, remain unchanged, whether examined by the loop (3223.) or by its own motion, and that of discs or wires associated with it (3116. &c.). Its force ever remains the same in quantity and general direction.

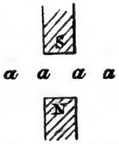
3334. The case of the steel ring magnet (3283.) is well known, and the manner in which such a magnet, showing no external relation, developes strong poles when it is broken. The phænomena assure us, I think, that when broken the northness and southness then appearing, cannot, when the pieces are by themselves, be determined upon each other backward through the magnet; there is no sufficient reason to suppose such a thing. And, again, the mutual destruction of highly-charged linear magnets, such as steel needles, when many of them are made into a thick, short bundle, shows the same thing; for if when alone the polar powers are not external, but are determined upon each other through each individual magnet, they are as free for a like disposal when the elementary magnets are associated as when they are separated :—and then there remains no sufficient reason to expect a dominant action over each other superior to that which each has over itself.

3335. It is not to be supposed that the change of force which occurs when the magnet first acting externally is then made to act internally or through itself, would be small and unnoticeable. It should be as great as the whole amount of power which the magnet can show under the most favourable circumstance; and the means are abundantly sufficient, by moving wires and discs, to make that evident in any case which might imply its passing through, or being removed out of, the magnet:—so that no difficulty can occur in that respect, and there remains, therefore, in my mind, but two suppositions; either the N polar force of a magnet when taken off from external compensating S polar force, is not exerted elsewhere as magnetic force at all; or else it is externally thrown upon and associated with the S polar force of the same magnet, and so sustained and disposed of, for the time, in its natural equivalent and essential state. If converted into any new form of power, what is that form? where is it disposed of? by what effects is it recognized? what are the proofs of its existence? To these inquiries there are no answers. But if it be directed externally upon the opposite S pole of the magnet, then all the consequences and foundations of my hypotheses of magnetic force and its polarity come forth; and, as I incline to believe, a consistent and satisfactory account of all magnetic phænomena, short of the idea of the nature of the magnetic force itself, is supplied.

3336. For if the dual forces of the poles of a magnet in free space are related to, and dependent upon, each other, and yet not through the magnet (3331.), then it must be through the space around. Then space must have a real magnetic relation to the force passing across it, just as it has to the ray of light passing from an illuminating to an illuminated body. Then the directions in which the two forces are exerted upon each other cannot be in right lines, which must, if they existed, pass of necessity through the magnet; but in curved lines, seeing that it is impossible that any but curved lines can hold the poles in relation to each other through the surrounding space (3297.):—and if they be curved lines, then I cannot imagine them to be anything else than physical lines of force; lines fitted to transfer the power onwards in consistency with its inevitable dual relation, and in conformity with that direction which ought, as I think, to be properly called *polarity*. And it further appears to me, that if we once admit the magnetic relation of a vacuum, then all the phænomena of paramagnetic and diamagnetic bodies; of differential polarity and individual polarity; of solutions, needles, crystals and moving conductors, are presented in a simple mutual relation, without any contradiction of fact or hypothesis, and in perfect harmony with each other.

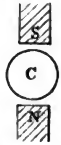
3337. I wish to avoid prolonging this paper by a repetition of the considerations and reasons already advanced on former occasions, and therefore will very briefly call to mind the idea I have put forth, that there are such lines of force in the space around a magnet; that the mutual dependence of the dualities, which is essential in the isolated magnet, is thus sustained; and that bodies in this space produce paramagnetic or diamagnetic phænomena, according as they favour or oppose the degree of sustaining power which mere space possesses. That these bodies, or *media* as they may be called, have a magnetic relation like that of space, is easily shown by numerous experimental results; but as they have a further relation amongst themselves, dependent upon their relative electro-conducting power, I think a little time may be usefully employed in considering how far the consequent results illustrate the probable condition of space where they are not present. Consider a magnet pole N, fig. 6, placed in relation to an equal magnetic pole S, so that their powers are mutually related and sustained, and the space between them, *a, a, a,* occupied by a vacuum, nitrogen, or some other gas at magnetic zero (2770. &c.):—the force exerted by N on S, or

Fig. 6.



reciprocally, is easily taken cognizance of by spirals, &c. as regards any change in direction or degree. Then consider the medium *a, a, a* to be all copper or all mercury, still the forces are undisturbed: or consider it part mercury or copper, and part vacuum or glass, divided either by a line running from S to N, or along *a a a*, or any other way, still the forces are undisturbed; any of these media act exactly like space, or so like it, we can scarcely trace a difference. Then consider the metal moving, either as a finely divided stream at *a, a, a*, or as a solid globe (of copper) C, fig. 7, revolving rapidly round the line from N to S; still it is exactly like the vacuum or indifferent gas or glass, and there is no effect as yet by which we might distinguish the material medium from the mere space. But let the stream of metallic particles be converted into a continuous plate, and then we know it becomes filled with abundant currents of electricity; or if we apply the wires of a galvanometer to the revolving copper globe C, at the axial and equatorial parts, we can then cause it to develope (by permission of currents) a new effect, and the currents are sent out most abundantly by the conductors applied. If the copper globe C be rapidly revolved upon an axis perpendicular to the line S N, so strong and influential a medium is it, magnetically considered, that the two poles, N and S, if free to move, do move in the same direction as the near parts of the globe; and are absolutely carried away from each other, in opposition to their mutually attractive force, which tends strongly all the while to draw them together. Now, how is it possible to conceive that the copper or mercury could have this power in the moving state, if it had no relation at all to the magnetic force in the fixed state? or, that it should have like power in the compact state, and yet have no relation to the magnetic force in the divided and moving state? The mere addition of motion could do nothing, unless there were a prior static dependence of the magnet and the metal upon each other. We know very well that the actions in the moving cases involve the evolution, or a tendency to the evolution, of electric currents; but that knowledge is further proof that the metals are in prior relation to the magnetic forces; and as bodies, even down to aqueous solution, have these electric currents set up in them under like circumstances, we have full reason to believe that all bodies when in the magnetic field are in like static relation as the copper when not moving:—and that when motion is superadded, they would all evolve electric currents, were it not for their bad electro-conducting powers.

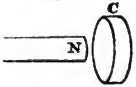
Fig. 7.



3338. These effects of motion are known to be identical with those of the moving wire (36. 55.), or those of voltaic induction (6. &c.); and their intensity and power is very well shown in the force of Elkington's magneto-electric apparatus and Ruhmkorff's induction coil. *Time* is concerned in their production, and Professor Henry has shown us, in some degree, that when the currents are moving in helices, the magnetic action across them is for a time cut off or deflected (1730.). These actions are, in every case, simple; *i. e.* a line of force in a given polar direction produces, or tends to produce, in a body moving across it, whether paramagnetic, neutral, or diamagnetic (3146. 3162.), a current in the *like* direction; which current must, as I conceive, be dependent upon a previous like *static* state. Nothing in the slightest degree analogous to the supposed oppositely polar states of paramagnetic and diamagnetic bodies has ever been discovered amongst them;—and it has never been said, or supposed, as far as I know, that the two actions, *i. e.* the magnetic and the magneto-electric, are separate in their essential nature, or that they are not the consistent and accordant, and I must add reciprocal, actions of one force.

3339. That the copper, &c. are effectual as magnetic media when in the field, may be stated also thus:—Let N, fig. 8, be a magnetic pole, and C a thick disc or short cylinder of copper. If the copper revolve ever so rapidly on its axis, there will be no production of currents in it; and the magnetic action of N on other magnets will be the same, as if the metal were quiescent or even away. If N recede from C there are then currents in C, though it be not moving; and though the effect of N upon other magnets, as far as we know them, are unchanged; yet there is then a slight attraction between C and the N pole. If N be made to approach C, the reverse currents and actions occur. As N approaches or recedes more quickly or slowly, the currents produced, and consequent temporary magnetic state, are higher. A cylinder electro-magnet will show these effects very well. The copper has all the time been still, no motion has been purposely given to it; it has been affected by the approximation and recession of the pole, has passed from one state to another, which states remain stationary as long as the poles are quiescent, and it shows every character of a medium affected by the magnetic force. By expedients the currents in the copper may be allowed or prevented; but whether they be allowed or not, the state the copper medium arrives at is the same. If disallowed as the magnet approaches, but allowed as it recedes, then the current due to the last change occurs, an effect easily shown with a magnet and helix; and this seems to prove very distinctly that

Fig. 8.



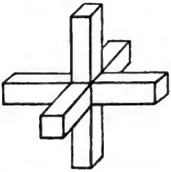
the copper within the constant influence of the magnet has a permanent, static, magnetic condition; and is therefore a magnetic medium, having lines of force passing through it. If C be of bismuth instead of copper, the same currents in the same direction occur, though in a far smaller degree; and, as it is believed, only because of deficiency in its conducting power.

3340. There can be no doubt that very much is involved in these phænomena, of the nature of which we have little or no knowledge; and the results obtained by Matteucci will probably lead to developments and discoveries of great importance. He states* that copper, when finely divided, presents very persisting phænomena, proving its right to be considered as a diamagnetic body; but that when aggregated, all, or nearly all, its diamagnetic character disappears. Nothing is known as yet of the manner in which the mere difference of cohesion or division can so affect the diamagnetic character. He finds, too, that in other respects, as in Arago's rotation, particles of matter act in a manner not to be anticipated from what is at present known of them as masses; and it is to be hoped and expected that when these results are enlarged and developed, we shall be able to form a better judgement of the true physical action of magnetism than at present.

Places of no magnetic action.

3341. The essential relation and dependence of the two magnetic dualities is manifested, I think, in a very striking manner, by the results which occur when we attempt to isolate northness or southness, by concentrating either of them on one space or piece of matter, and looking for their presence by effects, either of tension or any other kind, whether connected with polarity or not. A soft iron bar, an inch square, 3 or 4 inches long and rounded at the edges, had thirty-two convolutions of covered copper wire 0·05 of an inch in diameter put round it, so that covering the middle part of the bar, chiefly, it could be shifted if needful a little nearer to one end than the other; such a bar could be rendered magnetic by an electric current passed through the wire, and a degree of adjustment, in the strength of the N and S extremities, could be effected by this motion of the iron in its helix. Having six of these, it was easy to arrange them with their like poles together, so as to include a cubical space or chamber, fig. 9; and in this space I worked by every means at my disposal. Access to it was easily obtained by a previous removal of a portion of the solid angles

Fig. 9.



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* *Cours spécial sur l'induction, &c.*, 1854. pp. 165, 269.

of the ends which were to be brought together, or by withdrawing the electro-magnets a little the one from the others, and then a ray of light could be passed into or across it; magnetic needles or crystals of bismuth could be suspended in it;—a ring helix could be introduced and rotated there; and the motions of anything within could be observed by the eye outside.

3342. A small magnetic needle hung in the middle of this space, gave no indication of any magnetic power; near the open edges and angles vibrations occurred, but they were as nothing compared to the powerful indications given outside the chamber; even when the needle was many inches away. A crystal of bismuth was entirely indifferent. A piece of soft iron hung on a jointed copper wire within the chamber showed no trace of magnetic power, whether examined by the little needle or in any other manner. Iron filings on a card across the chamber were not affected in the middle part, but only near the partly open angles. A ring helix of many convolutions, having its terminations passing out at opposite corners, was connected with a very sensitive galvanometer and rotated; it showed no trace of inductive action. Numerous other experiments were made, but with results altogether negative. Attempts (though desperate) were made to ascertain if any electro-chemical conditions were induced there, but in vain. Every kind of trial that I could think of, not merely by tests of a polar character, but of all sorts, were instituted, but with the same negative result.

3343. It was of course not to be expected that any polar, *i. e.* any dually related polar, action could be exerted in this place; but if the polarities can exist without mutual relation, we might surely expect some condition, some tonic or static state, in a chamber thus prepared and surrounded with a high intensity of magnetic power, acting in great concentration on one particular spot or substance. But it is not so; and the chamber offers a space destitute of magnetic action, and free, under the circumstances, from magnetic influence. It is the complete analogue of the space presented within a deep metallic vessel or globe*, when charged with electricity (1174.). There is then no electricity within, because that necessary connexion and dependence of the electric duals, which is essential to their nature, cannot be. In like manner, there is no appearance of magnetic force in the cubical chamber, because the duals are not both there at once, and one cannot be present without the other.

3344. There are many ways of examining in a more or less perfect manner these neutral and highly instructive magnetic places. A cavity in the end of an electro-magnetic core or a permanent magnet will present similar phænomena, and in some respects even more perfectly; for though a trace of power will perhaps appear at the bottom of the cavity, the sum or amount, as compared to the sum of power at the end of the magnet, will show how complete the analogy between this space and the interior of a metallic vessel charged with positive or negative electricity is. A cylinder of soft iron, 9 inches in length and 1·6 in diameter, had a chamber 0·9 in diameter and 1 inch in depth formed in one extremity concentric with the cylinder; and being placed in a powerful helix of thick copper wire, and associated with a Grove's battery of ten pair of plates, was ready for experiment:—a like chambered magnet can be prepared by putting a proper iron ring against the end of any electro- or ordinary magnet, and will show the phænomena I am about to describe. A piece of soft iron, not more than 0·3 of an inch in length or thickness, held at the end of a copper wire and brought near the outer edge of the excited magnet pole, will be very strongly attracted; but if it be applied to the bottom of the chamber it will present no such effect, but be quite indifferent. If applied about the sides of the chamber, it will indicate no effect until it approaches the mouth. If the magnet be placed horizontally, and a piece of card-board be cut, so that it can enter the chamber and represent a horizontal section of its cavity; and, being sprinkled over with clean iron filings, is then put into its position and the magnet excited for a moment that it may develope its power over the chamber and filings and give them their indicative position; it will be found that only those near the mouth have been driven into a new position (about the outside angles of the pole), and that four-fifths of those upon the surface of the card within the chamber have been left unaffected, unmoved. If the chamber be filled with iron filings, closed with a card, placed in a vertical position with the aperture downward, and the magnet be then excited and the card removed, the filings will fall out; as they come out they will be caught away, and form a fine fringe round the external angles of the pole, but not one will remain at the bottom of the chamber, or even anywhere within the chamber, except near to its external edge. Yet, if a piece of iron long enough to reach out of the chamber, as a nail 2, 3, or 4 inches long, touch the *bottom* of the chamber, it is strongly attracted and held there, and will support a weight of several ounces, though prevented from touching the chamber anywhere else by a card with a hole in it placed over the mouth.

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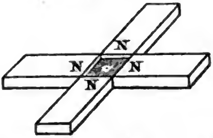
* Phil. Mag., Oct. 1846, vol. xxix. p. 257, note.

3345. If a small magnetic needle, about 0·1 of an inch long, be brought towards this excited magnet, it is almost unmanageable by reason of the force exerted upon it; but, as soon as it has entered the chamber, the power rapidly diminishes, and at the bottom the needle is scarcely, if at all, affected.

3346. If, instead of the core and chamber described, an iron tube of sufficient thickness of metal (as part of a gun-barrel) be employed, then like effects occur. If the magnetic needle be introduced, it ceases to be acted upon when about 1·5 inches within the tube. If the tube be more or less filled with iron filings, and then be excited and held vertical, they will all pour out and fall away, except those which are retained at the external edges. Yet, if a long nail be introduced, so as to be partly out of the cylinder, then it will be strongly attracted at the internal point, where it touches the iron of the tube core.

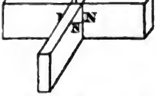
3347. The realization of like effects by grouping together the poles of ordinary magnets gives most interesting results. I have four very hard steel magnets, each 6 inches in length, 1 inch in breadth, and 0·4 nearly in thickness. When the four like poles are put together, fig. 10, they form a flat square chamber in the same plane as that of the magnets. If a piece of stiff paper, the size of this chamber, be raised on a block 0·2 of an inch high, then sprinkled over with iron filings, and the magnets afterwards approached regularly until the square chamber is formed, a little tapping on the card will then arrange the filings in lines from the sides of the square chamber to the centre. The filings show at once the direction of the lines of force in this medium plane, and their greater abundance at the middle of each pole than at the re-entering angles; and if the filings be then removed and the indication of the course of the lines be followed out by a small magnetic needle, it will be found that the lines rise upwards from this plane above, and descend from it below, and then turn back upon their course in the free space over and beneath the arrangement towards the S poles of the different magnets. The condition will be understood in a moment, by considering the sphondyloids of power belonging to each magnet (3271.), and the manner in which they are associated when the four like poles come together.

Fig. 10.



3348. When the magnets are turned edges upwards, they form a vertical chamber 1 inch high and only 0·4 of an inch in width, and now phænomena like those just described occur, but only near the entrances to the chamber; as the little needle proceeds into the enclosed space, the power of the magnets becomes less and less, and at the middle of the chamber scarcely a trace remains; that place being, like the closed chamber, formed with six poles (3341.),

Fig. 11.

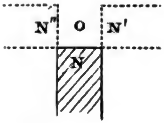


or like the bottom of a chamber formed in the end of a magnetic pole, a neutral place, or place of no magnetic action.

3349. The transition by degrees, from a pointed conical pole to an inclosed chamber, is, from the results described, very evident; and so also is their connexion with those belonging to the numerous neutral places produced under ordinary circumstances (3234. figs. 6, 10, 11, 15). Not the slightest difficulty or hesitation occurs, when these results are read or considered by the principle of representative lines of force; all the variations in the strength of the magnetic force and in the direction appear at once. But the great point is to observe how they all concur in showing the necessity of the complete and equivalent dual relation of the magnetic forces. When that is diminished or interfered with in any degree, in the same proportion does the power as a whole become diminished; until, at last, it absolutely disappears from a given place, though energies of the strongest kind are directing the force on to that spot, supposing that one of the dual elements could exist in any degree without, or independent of, the other.

3350. When formerly working with bismuth and magnets, I described several results (2298. 2487. 2491.) due to the principle of neutral magnetic places, more or less developed. If a sphere or cube of bismuth be delicately suspended by a vertical suspension or on a torsion balance, and an N pole be brought towards it, fig. 12, the bismuth will be repelled and the suspension deflected:—if a second N′ pole be brought up, as in the figure, the bismuth will be less repelled by N than before, will return towards it, and N′ will also seem to attract it, for on approaching the bismuth will tend to go into the angle formed by N and N′. If a third pole, N″, be brought up on the opposite side, the bismuth will then seem to be attracted by it, and by the first pole, and will, in fact, return very nearly into the position it would have if all the magnets were away. I thought at one time that magnetic structure, given by the second north pole N′ to the bismuth, might produce the approximation of it to N, and if so that this would be neutralized by the action of a like pole N″ on the opposite side, and so the approximation of the bismuth (if due to such a cause) be prevented. On the contrary, however, such a pole *increased* it; and a moment's consideration, by showing that the three poles form a chamber of diminished or no action (3341. 3347.), shows also that such ought to be the case. All the movements of the bismuth are the result of the tendency which it has to pass from stronger to weaker places of magnetic action (2418.); and

Fig. 12.



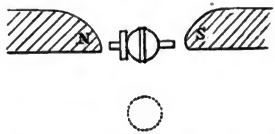
in the present case they show *that* weakened place, which in a higher degree would be a place of no magnetic force.

The moving conductor.

3351. I wish to make a few further remarks (3336. 3337.) upon the value of the moving conductor, as a means of investigation in magnetical science. It will be sufficient to refer to former papers for a statement of the principles, the power, and the certainty of its indications (3156. 3172. 3176. 3270.). At present, I desire to apply it in a direct form of experiment, to the supposed reverse polarity of iron and bismuth (3309.).

3352. Four metallic spheres of copper, bismuth, soft iron, and hard steel, 0·8 of an inch in diameter, have been prepared; each has a copper axis carrying a small wooden pulley, so that when in its supporting frame, rotation, more or less rapid, can be given to it by the band of a multiplying wheel; each also has a thin copper ring driven tightly on to it at the equator, which, being grooved, serves to retain a galvanometer wire pressed against that part during the revolution of the globe; the other wire meanwhile being held against the copper axis. These globes, in their frame, could be placed one by one in the magnetic field of a powerful permanent Logeman magnet, so as to be subject to the magnetic force, fig. 13; and then rotated, and the currents of electricity induced in them carried to galvanometers. Two such instruments were employed, one, a Ruhmkorff's, with fine wire (2651.), the other with a thick wire of only four revolutions (3178.). The latter was the best, but both gave good indications. The position of all things concerned was preserved undisturbed during the experiments, so that it will not be necessary to do more than to describe a standard effect, and afterwards refer other effects to it. This standard may be taken from the current indicated when the *copper* globe was in the magnetic field; and it was such, when the upper part of the globe moved westward, as to send the south ends of the galvanometer needles to the west also: eight or ten revolutions of the globe would cause the needles to pass through 80° or 90°.

Fig. 13.



3353. The *soft iron* sphere was placed in the magnetic field; it was so good in character as to retain very slight traces of magnetism when taken out again. Being revolved, it gave a current of electricity, the same in direction as that of the standard or copper ball. It is easy to understand, that if the globe be moved parallel to itself, but away from the magnet, in a line perpendicular to the magnetic axis (as into the dotted position, 3352. fig. 13), it will pass through places of weaker magnetic action. Under such changes of place, the induced current was weaker or stronger, according to the distance, but always in the same direction. Assuming that the rotating metal does give a true indication of the polarity or direction of the magnetic force (3077.), the results show that the polarity of the force which induces these currents, and which is the magnetic force of the dominant magnet, is the same both in the copper and in the iron. Other cases of the current from revolving iron may be referred to in the Exp. Res. (3162.).

3354. The *bismuth* globe was placed in the magnetic field. If made to revolve much, with the galvanometer wire pressing against the copper equator (3352.), the latter became warm by friction, and a permanent thermo-current was produced: this has been considered on a former occasion (3168.). Its effect is easily eliminated by revolving the globe a given number of times in opposite directions, observing the two deflections, adding them together, and taking the half of the sum for the amount of induced current in either one direction or the other; for as the thermo-current is added on the one side and subtracted on the other, such a process gives the real amount of the induced current. When, however, the bismuth sphere is revolved only five or ten times, the thermo-effect is so small as to make the galvanometer deflection very little more in one direction than in the other. When due attention was given, the rotation of the bismuth sphere produced an induced current in precisely the same direction as those obtained with the copper and iron; and so far, therefore, it indicated precisely the same direction of polarity for the magnetic force then acting *upon* and *in* it.

3355. The *hard steel* sphere, having been previously examined by a small needle and found to be unmagnetized, was placed in the magnetic field. It was then revolved, and gave an induced magneto-electric current in the *same* direction as the former currents. Being removed and again examined by the magnetic needle, it was found not to have received any sensible charge of magnetism.

3356. So these four metal globes indicate like polarity of the magnetic force, acting upon and within them, when examined thus by the magneto-electric current due to movement across the lines of force. By researches described elsewhere, it is known that all metals, and all bodies which are sufficiently electro-conductors, down even to aqueous fluids, give the same direction of the magneto-electric current:—it is never reversed without reversion of the polarity, and reversion of the polarity *always* reverses the induced current.

3357. The hard steel sphere was now made *a magnet,* and though not of good shape to retain magnetism, being very short, yet it was able to sustain being placed in the magnetic field, in a position the reverse of the polarity of that field, and yet retain its polarity; for when taken out and examined by a magnetic needle, the polarity was found to be the same as before. Such being the case, it seemed to me that this magnet might be employed to represent, according to the view of those who conceive that iron and bismuth are polarized in opposite directions in the magnetic field, both *iron* and *bismuth*; inasmuch as it could be placed in the field in that condition of polarity, which these are then supposed respectively to acquire. The *globe magnet* was therefore placed in the magnetic field in a position conformable to that of the dominant magnet, *i. e.* with its N pole towards the S pole of the magnet, &c.; and being rotated, it gave an induced magneto-electric current like that of the standard and of iron (3352. 3353.). The dominant magnet was then withdrawn to a distance (3353.) and the globe rotated by itself; it gave, as it ought to do, the same current as before; for it, by its coercitive force, retains permanently that state of polarity, which the iron could receive only temporarily whilst in the magnetic field. Being now turned 180° in a horizontal direction, the globe magnet was reversed as regards the dominant magnet (the latter being, however, still at a distance), and now the globe magnet gave a current the *reverse* of the former, or of the standard current; and yet a very consistent current in relation to its own polarity.

3358. The dominant magnet was now gradually brought up, and its effect on the reversed globe magnet observed. The current from the latter became less and less, and at last was inverted, becoming like that of the standard current; nor can that be wondered at, when it is considered that the dominant magnet was the largest supplied by Logeman to the Great Exhibition, and able to sustain a weight of 430 pounds, and the sphere magnet only 0·8 of an inch in diameter, and very imperfectly hardened in the interior. But when the dominant magnet was withdrawn a little, a place was soon found for the globe magnet, where its rotation in either direction produced no current at all. Outside of this place, the rotated sphere gave a current, the reverse of that of the standard; whilst the iron and bismuth spheres in the same place, gave currents alike in kind and the same as that of the standard. In this region, therefore (and it is like the whole of the magnetic field of many inferior yet very powerful magnets), if we represent bismuth by a magnet, reversely polar, as bismuth is supposed to be, we obtain induced magneto-electric currents, not like those of bismuth, but the contrary; and if we turn the representative magnet round, so as to give it the position in which it yields currents like those of the bismuth, then its polarity contradicts, or is the reverse of the assumed polarity of the bismuth.

3359. Now until the polarity or direction of the magnetic force which determines the course of the induced magneto-electric currents produced in every moving conductor, is distinguished and separated from the polarity or direction which causes movement amongst bodies subjected to the same force, how can these phænomena be accounted for by the supposition that the bismuth sphere is in the same polar condition as the reversed globe magnet? The reversed magnet is, in fact, the *contrary* to bismuth and to iron;—then bismuth and iron must be the same. The direct magnet is the *same* as the bismuth, in that polarity which induces a current;—then the magnet and the bismuth are the same. How easily all these effects present themselves in a consistent form, if read by the principle of representative lines of force. The reversed globe magnet at a distance from the dominant, shows, in revolving, the effect of the lines of force within it (3116.); as the magnet is approached, its external sphondyloid of power is compressed inwards (3238. fig. 15), and at last the magnet is self-contained; then showing the equalization of its own powers, and as yet the absence from within it of any of the powers of the chief magnet; so that it gives no induced currents, though in a place where bismuth and iron would give them freely. Within that distance the effect of the superior and overpowering force of the great magnet appears (3358.), which, though it can take partial possession of the little magnet, still, when removed, suffers the force of the latter to develope itself again, and present the same series of phæmonena as before.

3360. Van Rees admits, I believe, that the moving wire shows truly the presence, direction, and nature of the magnetic force or forces; and it is very important to know that the setting of a magnetic needle, or crystal of bismuth, and the production of a current of electricity in a moving conductor, are like correlative and consequent effects of the magnetic force; the power of producing one or the other effect being rigidly the same. Philosophers should either agree or differ distinctly on this point; so that if they differ, they may point out clearly the physical separation of the phænomena; which, if established, must lead to new and important discoveries. The polarity direction which the moving conductor makes manifest, whether that conductor be one of the paramagnetic or diamagnetic bodies themselves, or whether it be a conductor moving amongst them, either by itself or with them, is *always the same.* The electric current produced never indicates a change in the direction of the polarity, from that belonging to the *first* source or seat of the power; whether it be a magnet, a solenoid, or of any other nature; the only difference being in the strength of the electric current produced, which difference is directly referable to the electro-conducting power (3143. 3152. 3163.). If such be the natural truth, how can the two modes of indication ever give opposite results? If opposite results seem to appear, and only occasionally, is it that mode of induction which gives one consistent result that we should doubt, or that which seems to be inconsistent with itself? especially when similar contrary phænomena in abundance are known to be produced by bodies having *like* polarity (3316.), and when excellent physical reasons, founded on differential action, offer themselves for their explication. There is sufficient reason to admit, that the magnetic needle cannot be always a true direct indicator of the amount or the direction of magnetic action (2868. 2870. 3156. 3293.). Should we not therefore, in respect of the above phænomena, rather conclude, for the time, that the simple and uniform results of the one mode of action, are the true indication; and that where, in the other mode, the phænomena are reversed or doubled, a part of them are *compound* in their nature? I may, in conclusion, remark, that the effects of motion and those produced in the action of magnetism on light, are never reversed in any case, whatever the medium in which they are observed; both point to one direction of polarity only, namely, that of the dominant source of magnetism.

3361. I will bring these imperfect observations to an end, by a very brief statement of what I suppose to be the condition of a magnet; and by a disclaimer, as to anything like conviction on all points of that which I set forth as a supposition tending to lead to inquiry. Contemplating a bar magnet by itself, I see in it a source of dual power. I believe its dualities are essentially related to each other, and cannot exist but by that relation. I think that though related through the magnet by sustaining power, they are not so related by discharging or inducing power, a power equal in amount to the coercitive or sustaining power. The relation externally appears to me to be through the space around the magnet; in which space a sphondyloid of power is present consisting of closed curves of magnetic force. That the space is not magnetically dark (3305.) appears to me by this; that when bodies occupy that space, having like relation by known phænomena to the power as the space has, as copper, mercury, &c., they produce magneto-electric currents when moved. When bodies (media) occupy the space around the magnet, they modify its capability of transmitting and relating the dual forces of the magnet, and as they increase or diminish that capability, are paramagnetic or diamagnetic in their nature; giving rise to the phænomena which come under the term of magnetic conduction (2797.). The same magnet can hold different charges, as the medium connecting its poles varies; and so one, fully charged with a good medium, as iron between its poles, falls in power when the iron is replaced by air, or space, or bismuth. Corresponding effects occur with longer or shorter magnets (3290.), or with magnets made thick by adding many sideways together (3287.). The medium about a magnet may be mixed in its nature, and then more dual power is disposed of through the better conductor than the worse, but the whole amount of power remains unchanged. The powers and utility of the media, and of space itself, fail, if the dual force or polar action be interrupted. The magnet could not exist without a surrounding medium or space, and would be extinguished if deprived of it, and is extinguished, if the space be occupied adversely by the dual power of a dominant magnet of sufficient force. The polarity of each line of force is in the same direction throughout the whole of its closed course. Pointing in one direction or another, is a differential action due to the convergence or divergence of the lines of force upon the substance acted on, according as it is a better or a worse conductor of the magnetic force.

3362. But though such is my view, I put it forth with all the reservation made on former occasions (3244. 3299.). I do not pretend to explain all points of difficulty. I have no clear idea of the physical condition constituting the charged magnetic state; *i. e.* the state of the source of magnetic power:—or of the coercitivity by which that state is either resisted in its attainment, or sustained in its permanent condition; for the hypotheses as yet put forth give no satisfaction to my mind. I profess rather to point out the difficulties in the way of the views, which are at present somewhat too easily accepted, and to shake mens' minds from their habitual trust in them; for, next to developing and expounding, that appears to me the most useful and effectual way of really advancing the subject:—it is better to be aware, or even to suspect, we are wrong, than to be unconsciously or easily led to accept an error as right.

Royal Institution, 20 Dec. 1854.

XIV. *On the Action of the Violet and Ultra-violet Invisible Light.*

By W. EISENLOHR*.

THE phænomenon described by Stokes under the name *fluorescence,* led me to the supposition that this was caused by the interference of the shorter system of waves, blue-violet and ultra-violet (for the sake of shortness, the chemically-acting invisible rays of the spectrum may be so designated). I think, with many others, that the eye has the greatest sensibility for a certain duration of vibration (the yellow light), and that it is the more sensitive for longer or shorter waves, the more these differ from the medium light in their depth or height.

Light itself consists of the visible systems of waves, and besides these, of such as are longer than red and shorter than violet. As the combination of two tones is always deeper than each single one, out of which the compound tone arises, so from the interference of yellow and blue there can result only light of greater length of undulation, and not violet light. Now since red has the longest undulations of the visible light, the combination of red and yellow waves of light can only give a deeper tint than red, and consequently no visible light. A fluorescence in the dark space of the spectrum near the red is not therefore to be expected. It is quite otherwise at the other end of the spectrum. The ultra-violet is the light acting in the dark space of the spectrum near the violet; its existence could only be shown by its chemical action, before the wonderful discovery of Stokes. It consists of countless systems of undulations, the lengths of which, differing among themselves, have all a shorter duration than the violet light. Through their interference, waves of greater length than their own result; and by their great variety, tints of combination no less numerous; hence in many cases all kinds of visible light, or white.

In other cases a certain colour prevails in the mixture of the tints of combination, which will partly arise from the length of the original waves, and partly from the distance of the reflecting layers of atoms of the fluorescent body†.

Starting from this view, I have made experiments to find sources of light in which high tints prevail, in order to test this idea. Violet and blue glasses, through which the sunlight was admitted into the room by means of a heliostat, separating single parts of the entire spectrum from the rest, and causing the light thus obtained to penetrate into the fluorescent bodies, proved, at least, not the contrary of my supposition. I ascribe the cause of the partially slight success to the circumstance, that I possessed no blue and violet glasses of sufficient purity, which on that account allowed fewer of the more intensely acting rays to pass through. At last the violet light occurred to me, which results in the so-called electric egg when it is exhausted of air. I tried its action on fluorescent bodies, and was delighted to see that it produced some of the appearances described by Stokes, with a splendour which I have never seen with the most beautiful experiment by means of the spectrum. Paper on which a design had been made with a solution of sulphate of quinine, showed at a distance of ten to twelve feet from the oval receiver in the dark chamber, all the details of the design in the most beautiful white on a *deep* violet ground. Ruhmkorff's induction apparatus is extremely convenient for the production of the electric light in the receiver, when the latter is almost exhausted of air. The appearance is so striking, as to lead to the belief that the writing or design on the paper is itself shining and sparkling.

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* Poggendorff's *Annalen*, vol. xciii. p. 623.

† Of course the comparison between the tone of combination, and the light produced by various kinds of ultra-violet or other waves, must not be taken literally, for otherwise the number of vibrations of the resulting colour must be equal to the difference in the number of vibrations of the original rays.

Hence, in my opinion, it follows,—1. That the violet light produced *in vacuo* is mixed with a large quantity of invisible ultra-violet rays. 2. That out of the ultra-violet rays of the so-called northern light invisible to the naked eye, there results by interference in fluorescent bodies a quantity of visible light, and that therefore this light, reflected from the surface of the paper which has been marked with quinine solution, appears brighter than direct light; that therefore, out of the ultra-violet light invisible to the naked eye, there is produced by mechanical means actual light. 3. That the so-called northern light has the strongest chemical action.

A further conclusion is, that the light *in vacuo* of Ruhmkorff's apparatus, or even that of the electric machine, is a much more powerful agent for testing the fluorescence of bodies than any hitherto employed.

I may here mention that I am still engaged on this subject, and reserve to myself further communications. The short time at my disposal compels me to limit myself to the statement, that a thick white glass in the dark chamber appears of a clear and splendid gray.

I scarcely doubt that the white *colour* of the electric light in air has also its explanation in the combination of the higher systems of waves, which are formed in consequence of the manifold reflexion on the atoms of air, and the consequent interference. Even the sun's rays are, according to Sondhaus, violet, and we see the sun, as it appears to us, only through a mixture of tints, whose production can be explained by the combination of the shorter systems of rays of the violet light.

XV. *On the Law of Absorption of Gases.* *By* R. Bunsen*.

[With a Plate.]

GASEOUS bodies are absorbed by liquids on which they exert no chemical action, in quantities depending upon—

1st. The essential nature of the gas and of the absorbing liquid;

2nd. The temperature;

3rd. The pressure to which the gas is subjected.

The volume of gas, reduced to 0° and $0^{m}\cdot76$ pressure of mercury, which is absorbed by the unit volume of a liquid, under the pressure of $0^{m}\cdot76$ is called the *absorption-coefficient*, or *coefficient of absorption*. The value of this absorption-coefficient decreases in general with increase of temperature, in a ratio dependent upon the chemical nature of the absorbed gas and absorbing liquid. The values of the absorption-coefficients for varying temperatures can only be determined empirically. An exact relation exists between the volumes of absorbed gas† and the pressures under which the absorption takes place, the amounts of gas absorbed varying directly as the pressure.

The coefficient of absorption of any gas is therefore known when the following quantities are given:—1, the volume V, before the absorption, reduced to 0° and P pressure; 2nd, the volume V_1 remaining after the absorption reduced to 0° and P_1 pressure; and 3rd, the volume of the absorbing liquid. The amount of gas absorbed by the volume h_1 of liquid under the pressure P_1, is equal to the difference between the volume of gas originally taken, and that remaining unabsorbed,

$$=\frac{VP}{0\cdot76}-\frac{V_1P_1}{0\cdot76}.$$

If the pressure during the absorption had not been P_1 but 0·76, the amount of gas absorbed would have been, according to the above law,

$$\frac{VP}{P_1}-V_1.$$

Hence it follows that the coefficient of absorption, *i. e.* the amount of gas absorbed in the unit volume of liquid under the pressure 0·76, is

$$\alpha=\frac{1}{h_1}\left(\frac{VP}{P_1}-V_1\right). \quad . \quad . \quad . \quad . \quad . \quad (1)$$

When the coefficient α is known, the amount of gas g absorbed

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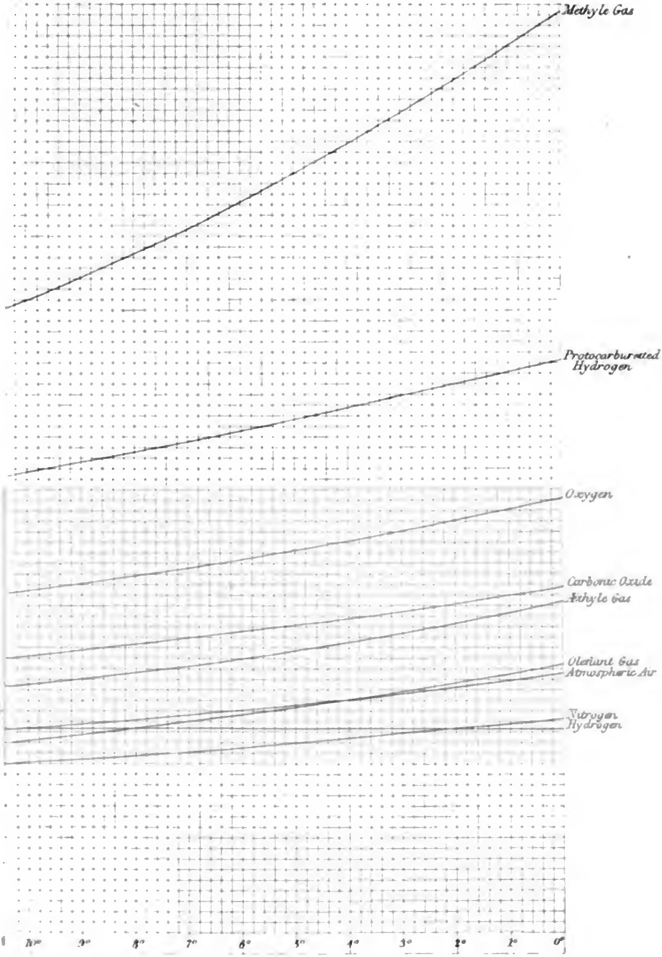
* Communicated by the Author. The Editors are indebted to Mr. Roscoe of Heidelberg for the translation of this memoir.

† The expressions "amount of gas" or "reduced volume," are henceforward to be understood to signify the volume of gas reduced to 0° and $0^{m}\cdot76$ pressure of mercury.

Gases for Water between 0°C. & 20°C.



J. Basire, sc.

in h volumes of liquid under the pressure P is given by the equation

$$g=\frac{\alpha h\mathrm{P}}{0{\cdot}76}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If two or more gases are mixed together, the absorption of the constituent parts is proportional to the pressures to which these parts are severally subjected. Let the volumes v_1, v_2, v_n, fig. 4, of different gases, each under the pressure P, remain unmixed one above the other, separated by the diaphragms $i\,i$, $i_1\,i_1$; each of the gases exerts a pressure P against the inclosing diaphragm; withdraw the diaphragm and remove the resistance opposing the action of the pressure, and the particles of v_1 will, in virtue of the pressure P, penetrate into the gases $v_2\,v_n$, which offer no resistance. The motion of the particles of the gas v_1 ends with a state of equilibrium, which ensues when the pressure exerted by v_1 has become equally great at every point of the space $v_1+v_2+v_n$; that is, when the gas v_1 exerts a uniform tension throughout the total volume. The pressure on v_1 is therefore, according to the law of Mariotte,

$$\frac{v_1}{v_1+v_2+v_n}\,\mathrm{P}.$$

In like manner it is found that the gases v_2, v_n, when equilibrium has ensued, are subject to a pressure of

$$\frac{v_2}{v_1+v_2+v_n}\,\mathrm{P} \text{ and } \frac{v_n}{v_1+v_2+v_n}\,\mathrm{P}.$$

From these pressures of the constituent parts the total pressure of the mixture is found to be

$$\mathrm{P}=\frac{v_1}{v_1+v_2+v_n}\,\mathrm{P}+\frac{v_2}{v_1+v_2+v_n}\,\mathrm{P}+\frac{v_n}{v_1+v_2+v_n}\,\mathrm{P}.$$

The amount of each constituent gas absorbed is proportional to the pressure on that constituent part $\frac{v_1}{v_1+v_2+v_n}\mathrm{P}$; and these pressures may be distinguished as "partial pressures," in contradistinction to the "total pressures" on the whole mixture.

If a mixture of gas, unalterable by absorption, consists of two or more volumes of chemically different gases v_1, v_2, v_n, the amount of each gas dissolved in h volumes of liquid under the pressure P, when α_1, α_2, α_n are the respective absorption-coefficients of the different gases at the observed temperature, is of the first gas,

$$\frac{\alpha_1 h\mathrm{P}v_1}{0{\cdot}76(v_1+v_2+v_n)}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

of the second gas,

$$\frac{\alpha_2 i P v_2}{0{\cdot}76(v_1+v_2+v_n)}; \quad \cdots\cdots \quad (4)$$

of the nth gas,

$$\frac{\alpha_n i P v_n}{0{\cdot}76(v_1+v_2+v_n)}. \quad \cdots\cdots \quad (5)$$

The unit volume of the absorbed gaseous mixture contains therefore, of the first gas,

$$u_1=\frac{\alpha_1 v_1}{\alpha_1 v_1+\alpha_2 v_2+\alpha_n v_n}; \quad \cdots\cdots \quad (6)$$

of the second,

$$u_2=\frac{\alpha_2 v_2}{\alpha_1 v_1+\alpha_2 v_2+\alpha_n v_n}; \quad \cdots\cdots \quad (7)$$

of the nth,

$$u_n=\frac{\alpha_n v_n}{\alpha_1 v_1+\alpha_2 v_2+\alpha_n v_n}. \quad \cdots\cdots \quad (8)$$

If, on the contrary, the amounts of the separate gases u_1, u_2, u_n contained in the unit of absorbed gas are known, the composition of the gas before absorption, or of that remaining unabsorbed, is found from the following equations.

The unit of free gas contains of the first gas,

$$v_1=\frac{\frac{u_1}{\alpha_1}}{\frac{u_1}{\alpha_1}+\frac{u_2}{\alpha_2}+\ldots+\frac{u_n}{\alpha_n}}; \quad \cdots\cdots \quad (9)$$

of the second,

$$v_2=\frac{\frac{u_2}{\alpha_2}}{\frac{u_1}{\alpha_1}+\frac{u_2}{\alpha_2}+\ldots+\frac{u_n}{\alpha_n}}; \quad \cdots\cdots \quad (10)$$

of the third,

$$v_n=\frac{\frac{u_n}{\alpha_n}}{\frac{u_1}{\alpha_1}+\frac{u_2}{\alpha_2}+\ldots+\frac{u_n}{\alpha_n}}. \quad \cdots\cdots \quad (11)$$

These formulæ are only strictly true on the supposition that the relation originally existing between the volumes of the constituent gases is not appreciably disturbed in consequence of the absorption; they are therefore, accurately speaking, only applicable in the case in which a gaseous mixture of constant composition, either infinitely large or continually renewed, acts upon a finite volume of liquid. If the volume of liquid employed is appreciable compared with the volume of gas, the alteration which the absorption causes in the composition of the unabsorbed gas must be brought into the calculation.

Let us next consider the alterations which a mixture of two gases undergoes by absorption, supposing that all the volumes of gas employed are reduced to 0°. Let the total volume of gas under the pressure P be V; in the unit volume of this gas let there be v volumes of the first gas, and v_1 of the second. Let the absorption-coefficient of the first gas at the observed temperature be α, and that of the second β, and the volume of absorbing liquid h. Further, let the total volume of the gas remaining after the absorption be V_1 under the pressure P_1; and lastly, let the unit volume of this residual gas contain u volumes of the first, and u_1 volumes of the second gas.

The volume V contains vV volumes of the first gas at the pressure P, or $\frac{vVP}{0{\cdot}76}$ volumes at $0^{m}{\cdot}76$. This volume is separated by absorption into two parts: the first part, x, remains behind after the absorption as free gas; the second, x_1, is that absorbed by the water. The amount of this latter is determined by the law of absorption; the unit of liquid absorbs the volume α under the pressure $0^{m}{\cdot}76$; hence under the pressure P_1, h volumes of water will absorb

$$\frac{\alpha h P_1}{0{\cdot}76}.$$

As, however, the first gas is expanded by mixture with the second from x to $\frac{V_1 P_1}{0{\cdot}76}$, the amount of gas absorbed by h is, in consequence of the partial pressure,

$$\frac{\alpha h x}{V_1} = x_1.$$

Hence

$$x + \frac{\alpha h x}{V_1} = \frac{vVP}{0{\cdot}76},$$

or

$$x = \frac{vVP}{0{\cdot}76\left(1 + \frac{\alpha h}{V_1}\right)};$$

and by similar reasoning, the volume of the second gas is

$$y = \frac{v_1 VP}{0{\cdot}76\left(1 + \frac{\beta h}{V_1}\right)}.$$

Hence when

$$vVP=A,\quad v_1VP=A_1,\quad \left(1+\frac{\alpha h}{V_1}\right)=B,\ \text{and}\ \left(1+\frac{\beta h}{V_1}\right)=B_1,$$

we obtain

$$\frac{AB_1}{(AB_1+A_1B)}=\frac{x}{x+y}=u, \quad . \quad . \quad . \quad . \quad (12)$$

$$\frac{A_1B}{(AB_1+A_1B)}=\frac{y}{x+y}=u_1. \quad . \quad . \quad . \quad (13)$$

It is clear that, *vice versâ*, the composition of an unknown gaseous mixture may be found from the change of volume ensuing from absorption by a liquid. In this way it is possible to analyse mixtures of gases by a purely physical experiment unassisted by chemical decomposition. Such absorptiometric determinations, as I term them, are, under certain conditions, scarcely less correct than a chemical analysis, often much more simple and convenient. Often, indeed, this mode of analysis is of immense importance, as solving questions which by other methods are not determinable.

Let us next consider the case in which two gases are given whose relation to each other is to be determined by an absorptiometric experiment.

Let x be the original volume of the first gas reduced to the pressure 1;

Let x' be the volume of the same gas unabsorbed, also reduced to the pressure 1;

Let v' be the volume of unabsorbed gaseous mixture at the pressure P′;

The pressure on the unabsorbed gas 1 is then $\frac{x'}{v'}$. If the absorbed amount of the gas 1 be reduced to this pressure, the volume is αh; reduced to the pressure 1, it is therefore

$$\frac{x'}{v'}\alpha h,$$

and hence

$$x=x'+\frac{x'}{v'}\alpha h=x'\left(1+\frac{\alpha h}{v'}\right),$$

or

$$x'=\frac{x}{1+\frac{\alpha h}{v'}}.$$

Hence the pressure of the unabsorbed gas 1 is

$$\frac{x}{v'+\alpha h}.$$

If y and y' represent the same values for the gas 2 which x and x' did for the gas 1, the pressure of the unabsorbed gas 2 is

$$\frac{y}{v'+\beta h}.$$

As P′ is the pressure of the mixture, we get

$$P'=\frac{x}{v'+\alpha h}+\frac{y}{v'+\beta h}.$$

If P is the pressure under which the mixture originally occupied the volume V, we have

$$P=\frac{x}{V}+\frac{y}{V}$$

(also obtained when $h=0$). We have then

$$1=\frac{x}{(v'+\alpha h)P'}+\frac{y}{(v'+\beta h)P'}$$

$$1=\frac{x}{VP}+\frac{y}{VP}.$$

If we place

$$VP=W$$
$$(V'+\alpha h)P'=A$$
$$(V'+\beta h)P'=B,$$

we obtain

$$\frac{x}{y}=\frac{W-B}{A-W}\cdot\frac{A}{B},$$

or the volumes of the first and second gases in the unit volume of the mixture are

$$\frac{x}{x+y}=\frac{W-B}{A-B}\cdot\frac{A}{W} \quad \ldots\ldots \quad (14)$$

$$\frac{y}{x+y}=\frac{A-W}{A-B}\cdot\frac{B}{W}. \quad \ldots\ldots \quad (15)$$

For the case in which n gases are to be determined, n equations are required, easily obtained by observing for particular temperatures $t, t_1, t_2 \ldots t_{n-1}$, the corresponding gaseous volumes $V, V_1, V_2 \ldots V_{n-1}$, at various pressures $P, P_1, P_2 \ldots P_{n-1}$, or for different volumes of liquid $h, h_1, h_2 \ldots h_{n-1}$. Thus for a mixture of three gases whose volume is $x+y+z$, the following equations are obtained:—

$$1=\frac{x}{VP}+\frac{y}{VP}+\frac{z}{VP},$$

$$1=\frac{x}{(V_1+\alpha_1 h_1)P_1}+\frac{y}{(V_1+\beta_1 h_1)P_1}+\frac{z}{(V_1+\gamma_1 h_1)P_1},$$

$$1=\frac{x}{(V_2+\alpha_2 h_2)P_2}+\frac{y}{(V_2+\beta_2 h_2)P_2}+\frac{z}{(V_2+\gamma_2 h_2)P_2}.$$

If we substitute a, b, c for the coefficients $\frac{1}{VP}$ in the first equation, $a_1\, b_1\, c_1$ for $\frac{1}{(V_1+\alpha_1 h_1)P_1}$ in the second, and $a_2\, b_2\, c_2$ for $\frac{1}{(V_2+\alpha_2 h_2)P_2}$ in the third, we obtain

$$\frac{x}{y}=\frac{A}{B}=\frac{b_1c_2-b_2c_1+b_2c-bc_2+bc_1-b_1c}{ac_2-ac_1+a_1c-a_1c_2+a_2c_1-a_2c},$$

$$\frac{z}{y}=\frac{C}{B}=\frac{ab_1-ab_2+a_1b_2-a_1b+a_2b-a_2b_1}{ac_2-ac_1+a_1c-a_1c_2+a_2c_1-a_2c},$$

or

$$\frac{y}{x+y+z}=\frac{B}{A+B+C}. \quad \ldots \quad (16)$$

$$\frac{x}{x+y+z}=\frac{A}{A+B+C}. \quad \ldots \quad (17)$$

$$\frac{z}{x+y+z}=\frac{C}{A+B+C}. \quad \ldots \quad (18)$$

The foregoing formulæ are founded upon the well-known hypothesis of Dalton and Henry, which, however, is not confirmed by the extremely inaccurate experiments hitherto made upon the subject. In order to verify them, it is necessary in the first place to obtain an exact determination of the coefficients of absorption; for this purpose I use an absorptiometer of the following construction:—

The absorption-tube, *e e*, Plate I. fig. 1, divided into millimetres and calibrated, has a small iron band *b*, furnished with a screw luted on to its lower and open end; this fits into another screw attached to the small iron stand *a a*, fig. 2. By this arrangement the open end of the tube can be screwed down against a plate of caoutchouc covering the lower surface of the stand, and the tube thus hermetically closed. On each side of the stand are fixed two steel springs *c c*, which fit into two upright grooves inside the wooden foot of the apparatus *f*, fig. 1, so that the stand can be either raised or depressed, but not turned on its axis. It is thus easy, when the tube and stand are in their places, to open or close the absorption-tube by giving it a very slight motion on its axis to the right or left. The outer cylinder *g g*, fig. 1, surrounding the absorption-tube is not luted either into the wooden foot *f*, or into the iron rim *h*, but the screws *i i* press the ground-glass edges of the cylinder against the caoutchouc rings. The tubes *r r* serve to pour in mercury, so that any wished-for pressure is obtained in the absorption-tube by raising or depressing the column of mercury in the inner glass cylinder. The temperature of the surrounding water is determined by the small thermometer *c*. The upper end of the outer cylinder is closed by an iron lid having a hinge at one side, and fastened down by means of a nut and screw attached to the iron rim *h*, fitting into a small slit in the side of the lid. In the middle of the cover inside there is a raised rim of iron, over which a thick sheet of caoutchouc is extended and fastened by a screwed ring *s*. This distended caoutchouc serves as a spring against which the top of the tube can be pressed, keeping it in a fixed position during the violent agitation necessary in the process of absorption.

The experiment itself is conducted in the following manner:—A volume of the gas to be examined is first collected in the tube over mercury, and the usual precautions taken in reading off, &c., as in the reduction and measurement of gases. A measured volume of water freed from air is next admitted under the mercury into the tube, which is then screwed tightly against the caoutchouc plate, and the tube thus closed placed in the cylinder containing some mercury, and over that a quantity of water. As soon as the pressure within and without has been equalised by slightly turning the tube, it is again closed, and the whole apparatus rapidly agitated for about a minute. This agitation, with opening and closing of the tube, is continued many times, until no further change of volume is perceptible. The observations necessary for the measurement and reduction of the residual gas are then made. Besides the temperature *t* and the barometric pressure *p*, four readings from the divided tube are required:—

1. The lower surface of mercury in the outer cylinder at *a*.
2. The upper surface of mercury in the absorption-tube *b*.
3. The upper surface of water in the absorption-tube at *c*.
4. The upper surface of water in the outer cylinder at *d*.

The method of calculation will be best explained by an example. I select for this purpose an experiment on the determination of the absorption-coefficient of nitrogen and water at 19° C.

Elements of the Calculation.

1. *Observations before the Absorption.*

Lower surface of mercury in outer cylinder	$a=423{\cdot}6$ millims.
Upper surface of mercury in tube* . .	$b=124{\cdot}1$ ...
Barometric pressure	$p=746{\cdot}9$ ...
Temperature of the absorptiometer . .	$t=19^{\circ}{\cdot}2$ C.
Temperature of the barometer	$\tau=19^{\circ}{\cdot}0$ ...

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* In this experiment the absorptiometer contained only mercury, and no water.

2. *Observations after the Absorption.*

Lower surface of mercury in outer cylinder .	$a_1=352{\cdot}2$ mm.
Upper surface of mercury in tube of absorption	$b_1=350{\cdot}7$ ...
Upper surface of water in tube	$c_1=\ 65{\cdot}5$ ...
Upper surface of water in outer cylinder .	$d_1=\ \ 8{\cdot}0$ ...
Barometric pressure	$p_1=746{\cdot}3$ ...
Temperature of the absorptiometer . . .	$t_1=19^{\circ}{\cdot}0$ C.
Temperature of the barometer	$\tau_1=18^{\circ}{\cdot}9$ C.

Reduction of these Elements.

1. *Before the Absorption.*

	mm.
Surface of mercury at	$a=423{\cdot}6$
Surface of mercury at	$b=124{\cdot}1$
Column of mercury in the absorption-tube .	$a-b=299{\cdot}5$
Ditto reduced to 0°	$\pi=298{\cdot}5$
The barometric pressure ($p=0{\cdot}7469$) reduced to 0°	$\pi_1=744{\cdot}4$
The pressure of the gas reduced to 0° .	$\pi_1-\pi=445{\cdot}9$
Tension of vapour of water at 19°·2 to be subtracted	$=\ 16{\cdot}6$
Pressure of the dry nitrogen	$P=429{\cdot}3$
The volume of gas corrected from the calibration read off at $b=124{\cdot}1$ at 19°·2 C . . .	$=\ 34{\cdot}90$
Ditto reduced to 0°	$V=\ 32{\cdot}608$

2. *After the Absorption.*

	mm.
Barometric pressure $p_1=746{\cdot}3$ reduced to 0° .	$\delta=743{\cdot}8$
Surface of mercury at	$a_1=352{\cdot}2$
Surface of mercury at	$b_1=350{\cdot}7$
Column of mercury in tube at 19°·0 C .	$a_1-b_1=\ \ 1{\cdot}5$
Ditto at 0° C.	$\delta_1=\ \ 1{\cdot}5$
Lower surface of water in tube at	$b_1=350{\cdot}7$
Upper surface of water in tube at	$c_1=\ 65{\cdot}5$
Column of water in the tube . .	$=(b_1-c_1)w=285{\cdot}2$
Lower surface of water in outer cylinder . .	$a_1=352{\cdot}2$
Upper surface of water in outer cylinder . .	$d_1=\ \ 8{\cdot}0$
Column of water in the outer cylinder (a_1-d_1)	$=w_1=344{\cdot}2$
The column of water (w_1-w) acting opposite to the barometer	$=\ 59{\cdot}0$
Ditto reduced to pressure of mercury . . .	$q=\ \ 4{\cdot}4$
The pressure reduced to 0°·C . . .	$(\delta-\delta_1-q)=746{\cdot}8$
Tension of watery vapour at 19°·0 to be subtracted	$=\ 16{\cdot}3$
Pressure of the unabsorbed nitrogen . . .	$P_1=730{\cdot}5$
The residual volume of gas corrected from calibration read off at division $c_1=65{\cdot}5$ . . .	$=\ 17{\cdot}67$
Ditto reduced to 0°	$V_1=\ 16{\cdot}522$
The corresponding volume to division $b_1=350{\cdot}7$	$=200{\cdot}04$
Volume of absorbing liquid	$h_1=182{\cdot}37$

The value of the absorption-coefficient, as obtained from these elements by equation No. 1, is

$$a=\frac{1}{h_1}\left(\frac{\mathrm{VP}}{\mathrm{P}_1}-\mathrm{V}_1\right)=0{\cdot}01448.$$

The accuracy of these determinations depends in a great measure upon the water employed being perfectly free from air. This is best accomplished by boiling the water briskly for several hours, and then causing it to pass while still boiling into a bottle, the neck of which has been drawn out to a fine point before the blowpipe. The water is then again boiled for half an hour in the bottle, and the end of the tube hermetically sealed, access of air being prevented during the closing by pressing a caoutchouc tube attached to the extremity. When the water thus freed from air, which should strike against the glass like a water-hammer, is required for experiment, the end of the drawn-out tube moistened with solution of corrosive sublimate is broken under mercury, and the water admitted directly into the absorption-tube. Before admitting the water, care must be taken that no bubble of air appears in the bottle, which would show imperfect boiling. The following coefficients of absorption have been thus determined from experiments made in my laboratory, chiefly by Dr. Pauli*.

1. *Nitrogen in Water.*

The gas was prepared by passing dry ammonia and air, freed from carbonic acid, over glowing copper turnings.

No.	° C.	Coefficient found.	Coefficient from formula (19).	Difference.
1	at 4·0	0·01843	0·01837	−0·00006
2	6·2	0·01751	0·01737	−0·00014
3	12·6	0·01520	0·01533	+0·00013
4	17·7	0·01436	0·01430	−0·00006
5	23·7	0·01392	0·01384	−0·00008

By combination of the experiments 1, 2, 3; 2, 3, 4; and 3, 4, 5, we obtain the interpolation formula,

$$c=0{\cdot}20346-0{\cdot}00053887t+0{\cdot}000011156t^2. \quad . \quad (19)$$

By the help of this formula the following Table is calculated :—

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* This talented young chemist, who most zealously assisted me in the experimental part of this research, has since, to my deep regret, been removed by death from science and from his friends.

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·02035		7	0·01713	0·00039	14	0·01500	0·00023
1	0·01981	0·00054	8	0·01675	0·00038	15	0·01478	0·00022
2	0·01932	0·00049	9	0·01640	0·00035	16	0·01458	0·00020
3	0·01884	0·00048	10	0·01607	0·00033	17	0·01441	0·00017
4	0·01838	0·00046	11	0·01577	0·00030	18	0·01426	0·00015
5	0·01794	0·00044	12	0·01549	0·00028	19	0·01413	0·00013
6	0·01752	0·00042	13	0·01523	0·00026	20	0·01403	0·00010

2. *Hydrogen in Water.*

The gas was prepared from dilute sulphuric acid and pure zinc.

No.	° C.	Coefficient.	Difference from the mean.
1	at 4·0	0·0185	−0·0008
2	7·0	0·0205	+0·0012
3	9·6	0·0196	+0·0003
4	12·8	0·0186	−0·0007
5	15·5	0·0197	+0·0003
6	18·8	0·0188	−0·0005
7	23·6	0·0194	+0·0001

From these experiments it is seen that the mean coefficient of absorption 0·0193 of hydrogen is constant for temperatures from 0° to 20°.

3. *Æthyle Gas in Water.*

For these experiments a quantity of the same æthyle gas was employed, which Professor Frankland prepared in my laboratory some years ago, and of which he gave the analysis in his research on the æther radicals.

No.	° C.	Coefficient found.	Coefficient calculated from (20).	Difference.
1	at 5·8	0·02637	0·02626	+0·0011
2	8·7	0·02393	0·02428	−0·0035
3	14·0	0·02199	0·02175	+0·0024
4	17·2	0·02103	0·02092	+0·0011
5	21·8	0·02026	0·02061	−0·0035

By combination of the experiments 1, 2, 3; 2, 3, 4; and 3, 4, 5, the following interpolation formula is found,

$$c=0{\cdot}031474-0{\cdot}0010449t+0{\cdot}000025066t^2, \quad . \quad (20)$$

by means of which the following Table is calculated:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·03147	0·00102	7	0·02541	0·00072	14	0·02179	0·00037
1	0·03045	0·00098	8	0·02474	0·00067	15	0·02147	0·00032
2	0·02947	0·00091	9	0·02412	0·00062	16	0·02121	0·00026
3	0·02856	0·00086	10	0·02355	0·00057	17	0·02100	0·00021
4	0·02770	0·00081	11	0·02303	0·00052	18	0·02084	0·00016
5	0·02689	0·00076	12	0·02257	0·00046	19	0·02073	0·00011
6	0·02613		13	0·02216	0·00041	20	0·02065	0·00008

4. *Carbonic Oxide in Water.*

The gas was prepared by heating sulphuric acid with pure formiate of magnesia; treatment with a ball of potash showed it to be perfectly pure.

No.	° C.	Coefficient found.	Coefficient from formula (21).	Difference.
1	at 5·8	0·028636	0·028691	−0·000055
2	8·6	0·027125	0·027069	+0·000056
3	9·0	0·026855	0·026857	−0·000002
4	17·4	0·023854	0·023642	+0·000212
5	18·4	0·023147	0·023414	−0·000267
6	22·0	0·022907	0·022863	+0·000044

If the mean value from 1, 2, 3, from 2, 3, 4, 5, and from 4, 5, 6, be taken for the calculation of the constants, we get the following formula:—

$$c=0·032874-0·00081632\,t+0·000016421\,t^2 \ldots \quad (21)$$

This gives for the temperature from 0° to 20°,—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·032874	0·000800	7	0·027964	0·000603	14	0·024664	0·000373
1	0·032074	0·000767	8	0·027394	0·009570	15	0·024324	0·000340
2	0·031307	0·000734	9	0·026857	0·000537	16	0·024017	0·000307
3	0·030573	0·000701	10	0·026353	0·000504	17	0·023743	0·000274
4	0·029872	0·000669	11	0·025882	0·000471	18	0·023501	0·000242
5	0·029203	0·000636	12	0·025443	0·000439	19	0·023292	0·000209
6	0·028567		13	0·025037	0·000406	20	0·023116	0·000176

5. *Protocarburetted Hydrogen in Water.*

I have used for this determination a gas, preserved in hermetically-closed tubes, which is found in the mud-volcanoes of Bulganack in the Crimea, where it occurs under similar circumstances as at Baku on the Caspian Sea. This gas was employed because it appeared from my researches that it is the purest which occurs in nature. By treatment with a potash ball, it was freed from a trace of carbonic acid, and it contained, as the following analysis shows, neither nitrogen, oxygen nor olefiant gas:—

	Vol.	° C.	Pressure.	Vol. at 0° and 1 m.
Original volume of gas	127·6	4·8	0·1596	20·01
After admission of air	499·0	4·8	0·5151	252·60
After admission of oxygen	537·4	4·8	0·5500	290·47
After the explosion	495·4	4·5	0·5115	249·29
After absorption of carbonic acid...	466·2	4·6	0·4994	228·97
After addition of hydrogen	609·3	4·3	0·6284	376·95
After the explosion	478·8	4·3	0·5105	240·64

	Found.	Calculated.
Gas employed . . .	20·01	20·45
Carbonic acid formed	20·32	20·45
Contraction	41·18	40·90
Oxygen consumed . .	41·18	40·90

This gas gave the following values:—

No.	° C.	Coefficient found.	Coefficient from formula (22).	Difference.
1	6·2	0·04742	0·04757	−0·00015
2	9·4	0·04451	0·04430	+0·00021
3	12·5	0·04126	0·04134	−0·00008
4	18·7	0·03586	0·03600	−0·00014
5	25·6	0·03121	0·03100	+0·00021

The mean from 1, 2, 3, from 2, 3, 4, and from 3, 4, 5, gives the interpolation formula,

$$c=0·05449-0·0011807\,t+0·000010278\,t^2, \ldots \quad (22)$$

from which the following Table is calculated:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·05449		7	0·04674	0·00104	14	0·03997	0·00091
1	0·05332	0·00117	8	0·04571	0·00103	15	0·03909	0·00088
2	0·05217	0·00115	9	0·04470	0·00101	16	0·03823	0·00086
3	0·05104	0·00113	10	0·04372	0·00098	17	0·03739	0·00084
4	0·04993	0·00111	11	0·04275	0·00097	18	0·03657	0·00082
5	0·04885	0·00108	12	0·04180	0·00095	19	0·03577	0·00080
6	0·04778	0·00107	13	0·04088	0·00092	20	0·03499	0·00078

6. *Methyle Gas in Water.*

For this experiment, a specimen of methyle gas, sealed up in a glass tube, was used, which Professor Frankland prepared from iodide of methyle, and analysed some years ago at Marburg:—

No.	° C.	Coefficient found.	Coefficient from formula (23).	Difference.
1	4·6	0·072884	0·073084	−0·000200
2	7·8	0·064732	0·064839	−0·000107
3	12·1	0·055788	0·055703	+0·000085
4	15·2	0·050722	0·050500	+0·000222
5	19·8	0·045715	0·044915	+0·000800
6	24·2	0·040817	0·041960	−0·001143

If the arithmetical mean from 1, 2, 3, 4, from 3, 4, 5, and 4, 5, 6, are used for the conditional equations for the interpolation formula, we obtain,

$$c=0·0871-0·0033242\,t+0·0000603\,t^2, \ldots\ldots (23)$$

and from this the coefficients between the temperatures of 0° and 20° are,—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·0871	0·0033	7	0·0668	0·0025	14	0·0524	0·0017
1	0·0838	0·0031	8	0·0644	0·0024	15	0·0508	0·0016
2	0·0807	0·0030	9	0 0621	0·0023	16	0·0493	0·0015
3	0·0777	0·0029	10	0·0599	0·0022	17	0·0480	0·0013
4	0·0748	0·0028	11	0·0578	0·0021	18	0·0468	0·0012
5	0·0720	0·0027	12	0·0559	0·0019	19	0·0457	0·0011
6	0·0693		13	0·0541	0·0018	20	0·0447	0·0010

7. *Olefiant Gas in Water.*

The gas was prepared with the well-known precautions from alcohol and sulphuric acid. To free it from the vapours of æther and alcohol, and from traces of other hydrocarbons polymeric with elayle, a ball of coke, saturated with concentrated but not fuming sulphuric acid, was placed in the gas until nearly the half of it was absorbed; a ball of potash was afterwards introduced, and left in contact with the gas for some time. Eudiometrical analysis proved the purity of the gas:—

No.	° C.	Coefficient found.	Coefficient from formula (24).	Difference.
1	4·6	0·21870	0·21824	+0·00046
2	9·6	0·18398	0·18592	−0·00194
3	14·0	0·16673	0·16525	+0·00148
4	18·0	0·15324	0·15278	+0·00046
5	20·6	0·14597	0·14791	−0·00194

The interpolation formula is obtained from combination of 1, 2, 3 and 2, 3, 4 and 3, 4, 5,

$$c=0·25629-0·00913631\,t+0·000188108\,t^2, \ldots (24)$$

and hence the coefficients between 0° and 20°:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·2563	0·0090	7	0·2018	0·0064	14	0·1652	0·0041
1	0·2473	0·0085	8	0·1952	0·0066	15	0·1615	0·0037
2	0·2388	0·0082	9	0·1893	0·0059	16	0·1583	0·0032
3	0·2306	0·0079	10	0·1837	0·0056	17	0·1553	0·0030
4	0·2227	0·0074	11	0·1786	0·0051	18	0·1528	0·0025
5	0·2153	0·0071	12	0·1737	0·0049	19	0·1506	0·0022
6	0·2082		13	0·1693	0·0044	20	0·1488	0 0018

8. *Carbonic Acid in Water.*

This gas was prepared by the action of strong sulphuric acid upon chalk, a few drops of water being added to the mixture. The gas, thus steadily evolving, was washed by being passed through boiled water. The experiment gave,—

No.	° C.	Coefficient found.	Coefficient from formula (25).	Difference.
1	4·4	1·4698	1·4584	+0·0114
2	8·4	1·2426	1·2607	−0·0181
3	13·8	1·0654	1·0385	+0·0269
4	16·6	0·9692	0·9610	+0·0082
5	19·1	0·8963	0·9134	−0·0171
6	22·4	0·8642	0·8825	−0·0183

The three conditional equations from the mean 1, 2, 3, 4 and 2, 3, 4 and 3, 4, 5, 6, give the interpolation formula,

$$c=1{\cdot}7967-0{\cdot}07761\,t+0{\cdot}0016424\,t^2, \quad \ldots\ldots \quad (25)$$

and from this the following coefficients are found:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	1·7967	0·0760	7	1·3339	0·0562	14	1·0321	0·0332
1	1·7207	0·0726	8	1·2809	0·0530	15	1·0020	0·0301
2	1·6481	0·0694	9	1·2311	0·0498	16	0·9753	0·0267
3	1·5787	0·0661	10	1·1847	0·0464	17	0·9519	0·0234
4	1·5126	0·0629	11	1·1416	0·0431	18	0·9318	0·0201
5	1·4497	0·0596	12	1·1018	0·0398	19	0·9150	0·0168
6	1·3901		13	1·0653	0·0365	20	0·9014	0·0136

[To be continued.]

XVI. *On the Eccentricity of the Moon's Orbit.*
By Professor CHALLIS*.

IT is a satisfactory result of the discussion relating to the moon's orbit which has recently appeared in this Magazine, that it has drawn attention to the general question of apsidal motion, which is one that is by no means exhausted. In the Number for last January, Mr. Thacker has solved a problem of this class by a method which shows the advantage of employing for the purpose the first integrals of the differential equations of the motion. I have used an analogous method to determine the motion of the apses of the moon's orbit. In the problem above mentioned, the force is supposed to be wholly central, and to be expressed by the function $\frac{\mu}{r^2}+\mu' r$, and the solution leads to the remarkable result, that the motion of the apses is uniform *whatever be the eccentricity of the orbit.* I have no grounds for concluding that the author is of opinion that any argument can be deduced from this result against the theorem I have advanced respecting the moon's orbit, viz. that the uniformity of the progression of the lunar apses depends on a certain value of the eccentricity; but as such an argument appears *primâ facie* to be deducible, I have undertaken to show that this is not really the case. For this purpose I propose to solve the problem by a method somewhat different from that employed by Mr. Thacker.

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* Communicated by the Author.

The force is wholly central, and equal to $\frac{\mu}{r^2}-\mu' r$, the second term is supposed to be always small compared to the other, and only the first power of μ' is retained. From the usual differential equations of the motion, viz.

$$\frac{d^2x}{dt^2}+\frac{\mu x}{r^3}-\mu' x=0, \quad \frac{d^2y}{dt^2}+\frac{\mu y}{r^3}-\mu' y=0,$$

may be derived, after substituting $r\cos\theta$ for x, and $r\sin\theta$ for y, the following:

$$\frac{d^2r}{dt^2}-r\frac{d\theta^2}{dt^2}+\frac{\mu}{r^2}-\mu' r=0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{d\theta}{dt}=\frac{h}{r^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\frac{d^2r}{dt^2}-\frac{h^2}{r^3}+\frac{\mu}{r^2}-\mu' r=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Multiplying (3) by $2dr$ and integrating,

$$\frac{dr^2}{dt^2}+\frac{h^2}{r^2}-\frac{2\mu}{r}-\mu' r^2+\mathrm{C}=0. \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{C})$$

Hence, eliminating dt by (2), we have

$$d\theta=\frac{hdr}{r\sqrt{-\mathrm{C}r^2+2\mu r-h^2+\mu' r^4}}.$$

As the integral of this equation can only be obtained approximately, and the approximation is to proceed according to the powers of μ', we get by expanding to the first power of this quantity,

$$d\theta=\frac{hdr}{r(-\mathrm{C}r^2+2\mu r-h^2)^{\frac{1}{2}}}-\frac{\mu' h}{2}\cdot\frac{r^3dr}{(-\mathrm{C}r^2+2\mu r-h^2)^{\frac{3}{2}}}.$$

This equation being integrated by the usual rules, gives

$$\theta+\gamma=\cos^{-1}\frac{h^2-\mu r}{r\sqrt{\mu^2-h^2\mathrm{C}}}+\frac{3\mu\mu' h}{2\mathrm{C}^{\frac{5}{2}}}\cos^{-1}\frac{\mathrm{C}r}{\sqrt{\mu^2-h^2\mathrm{C}}}$$
$$-\frac{\mu' h}{2\sqrt{\mathrm{R}}}\cdot\left\{-\frac{r^2}{\mathrm{C}}+\frac{2(h^2\mathrm{C}-3\mu^2)(h^2-\mu r)}{\mathrm{C}^2(\mu^2-h^2\mathrm{C})}+\frac{3h^2\mu(\mu-\mathrm{C}r)}{\mathrm{C}^2(\mu^2-h^2\mathrm{C})}\right\},$$

where R is put for $-\mathrm{C}r^2+2\mu r-h^2$. Let $a(1+e)$ and $a(1-e)$ be apsidal distances which satisfy the equation

$$-\mathrm{C}r^2+2\mu r-h^2+\mu' r^4=0. \quad . \quad . \quad . \quad . \quad (\mathrm{A})$$

Since the square of μ' is not retained, it will suffice to employ in those terms of the foregoing equation which involve μ', the values of a and e given by the solution of the approximate equation, $-\mathrm{C}r^2+2\mu r-h^2=0$, that is, *to suppose the approximation to commence from a fixed ellipse.* Consequently,

$$a=\frac{\mu}{\mathrm{C}}, \quad ae=\frac{1}{\mathrm{C}}\sqrt{\mu^2-h^2\mathrm{C}},$$

and

$$\cos^{-1}\frac{\mu-\mathrm{C}r}{\sqrt{\mu^2-h^2\mathrm{C}}}=\cos^{-1}\frac{a-r}{ae}.$$

Let this arc $=\phi$. Then the above equation between r and θ gives to the same approximation,

$$\cos\left(\theta+\gamma-\frac{3\mu\mu' h}{2\mathrm{C}^{\frac{5}{2}}}\phi\right)=\frac{h^2-\mu r}{r\sqrt{\mu^2-h^2\mathrm{C}}}$$

$$+\frac{h^2\mu'}{2r\sqrt{\mu^2-h^2\mathrm{C}}}\cdot\left\{-\frac{r^2}{\mathrm{C}}+\frac{2(h^2\mathrm{C}-3\mu^2)(h^2-\mu r)}{\mathrm{C}^2(\mu^2-h^2\mathrm{C})}+\frac{3h^2\mu(\mu-\mathrm{C}r)}{\mathrm{C}^2(\mu^2-h^2\mathrm{C})}\right\}.$$

Now since $r=\frac{1}{\mathrm{C}}(\mu-\sqrt{\mu^2-h^2\mathrm{C}}\cos\phi)$ nearly, it follows that

$$r^2=2\mu r-h^2-\frac{\mu^2-h^2\mathrm{C}}{\mathrm{C}^2}\sin^2\phi.$$

After substituting this value of r^2, the equation may be arranged as follows:

$$\cos\left(\theta+\gamma-\frac{3\mu\mu' h}{2\mathrm{C}^{\frac{5}{2}}}\phi\right)=\frac{h^2}{\sqrt{\mu^2-h^2\mathrm{C}}}\cdot\left\{1+\frac{h^2\mu'}{2\mathrm{C}^2}\cdot\frac{h^2\mathrm{C}-2\mu^2}{\mu^2-h^2\mathrm{C}}\right\}\cdot\frac{1}{r}$$

$$-\frac{\mu}{\sqrt{\mu^2-h^2\mathrm{C}}}\cdot\left\{1+\frac{h^2\mu'}{2\mathrm{C}^2}\cdot\frac{3h^2\mathrm{C}-4\mu^2}{\mu^2-h^2\mathrm{C}}\right\}$$

$$+\frac{h^2\mu'}{2\mathrm{C}^3}\cdot\sqrt{\mu^2-h^2\mathrm{C}}\cdot\frac{\sin^2\phi}{r}.$$

The values of the arbitrary constants h^2 and C, derived in terms of the constants a and e from the apsidal equation (A), are

$$\mathrm{C}=\frac{\mu}{a}+2\mu' a^2(1+e^2), \quad h^2=\mu a(1-e^2)-\mu' a^4(1-e^2)^2.$$

If these be substituted in the foregoing equation, the result to the same approximation is,

$$\cos\left(\theta+\gamma-\frac{3\mu' a^3\sqrt{1-e^2}}{2\mu}\phi\right)=\frac{a(1-e^2)}{er}-\frac{1}{e}+\frac{\mu' a^4 e(1-e^2)}{2\mu}\cdot\frac{\sin^2\phi}{r}.$$

Let

$$\cos\psi = \frac{a(1-e^2)}{er} - \frac{1}{e},$$

so that

$$\frac{\sin\phi}{r} = \frac{\sin\psi}{a\sqrt{1-e^2}}.$$

Hence

$$\cos\left(\theta+\gamma-\frac{3\mu' a^3\sqrt{1-e^2}}{2\mu}\phi\right) = \cos\psi + \frac{\mu' a^3 e\sqrt{1-e^2}}{2\mu}\sin\phi\sin\psi$$

$$= \cos\left(\psi - \frac{\mu' a^3 e\sqrt{1-e^2}}{2\mu}\sin\phi\right) \text{ nearly;}$$

$$\therefore\ \theta = \psi + \frac{\mu' a^3\sqrt{1-e^2}}{2\mu}(3\phi - e\sin\phi),$$

θ being supposed to commence with ψ and ϕ.

It is not necessary for my purpose to pursue the reasoning further. The equation last obtained, which is identical with the principal result of Mr. Thacker's solution, sufficiently proves that the two methods are the same in principle, and lead to the same inferences respecting the motion of the apses. It is, however, important to remark, that my method shows clearly that the analytical reasoning may legitimately commence from *a fixed ellipse*. Let us now consider the bearing this inference has on the lunar theory.

It is well known that in the lunary theory the analysis fails to give the true development of the moon's radius-vector and true longitude, if the approximation commences from a fixed ellipse. What is the reason that the process fails in this instance, while it succeeds in that just considered? I reply, that the different conditions of the lunar problem entirely account for this difference. In the first place, the force in the lunar theory is not wholly central, and the equation (C) is consequently only approximate. Next, there is introduced into the lunar theory a limitation, according to which the moon's true longitude and radius-vector can never differ much from mean values*. In consequence of this limitation, which is essential to the subsequent treatment of the problem, the eccentricity of the moon's orbit is always small. Lastly, in the lunar theory it is necessary to conduct the approximation, not only according to the disturbing force, but according to the eccentricity also. The failure just spoken of proves that, under *these* circumstances, it is not legitimate to commence from a fixed ellipse.

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* In Mr. Airy's Lunar Theory (Mathematical Tracts, 3rd edition) this limitation is introduced where it is said, in page 29, that "for θ' we shall put the value which it would have if the motions of the sun and moon were both uniform."

But from independent analytical principles (as illustrated in the case already considered), it is certain that, under *any* circumstances, it would be legitimate to commence from a fixed ellipse, if the aggregate of the terms $h^2-2\mu r+Cr^2$ be of a higher order of magnitude, estimated according to the disturbing force, than the term $-\mu' r^4$. It is clear that they cannot be of a lower order.

It follows, therefore, inevitably, that the aggregate of the terms $h^2-2\mu r+Cr^2$ is of the *same* order of magnitude with respect to the disturbing force as the term $-\mu' r^4$; in other words, *that they contain the disturbing force as a factor.* It is not difficult to point out the origin of this analytical circumstance. The integration by which the constant C is introduced (see Phil. Mag. for April 1854, p. 279), may be effected by assuming only that the increment in a given time of the sun's true longitude is small compared to that of the moon's. The remarkable integral I have obtained in the Supplement to the Philosophical Magazine for December 1854 (p. 521), shows that even that assumption is not necessary for effecting an integration. The limitation that the moon's true longitude differs little from a mean value, is made *subsequently* to that integration. Hence the constant C possesses greater generality than comports with the conditions of the problem, and must itself be subject to some limitation. Clearly, therefore, the reasoning will be complete if, by means of the constant C, the condition that $h^2-2\mu r+Cr^2$ contains the disturbing force as a factor can be fulfilled. But this may be readily done as follows.

Let $h^2C=\mu^2$, and $r=\frac{\mu}{C}+fv$. Then

$$h^2-2\mu r+Cr^2-\mu' r^4=C\left(r-\frac{\mu}{C}\right)^2-\mu' r^4$$

$$=Cf^2v^2-\mu'\left(\frac{\mu}{C}+fv\right)^4.$$

Hence the required condition is satisfied if $h^2C=\mu^2$, the unknown quantity f being determined by the equation $f^2=\mu'$.

This point being settled, it remains to indicate the process of approximation that must be adopted. Resuming the equation

$$\frac{r^2dr^2}{dt^2}=-h^2+2\mu r-Cr^2+\mu' r^4, \quad . \quad . \quad . \quad \text{(C)}$$

it is evident, since $-h^2+2\mu r-Cr^2$ contains μ' as a factor, that the approximation must proceed according to the eccentricity, and not according to the disturbing force. For the same reason, the equation which gives the apsidal distances must be solved as a biquadratic, the term $\mu' r^4$ being of equal significance with the others. Since, therefore, from the foregoing reasoning, it appears that r is nearly equal to $\frac{\mu}{C}$, and the apsidal distances by hypothesis differ little from each other, it follows from the theory of equations, that to obtain their approximate values, $\frac{\mu}{C}+\left(r-\frac{\mu}{C}\right)$ is to be substituted for r in the last term of the equation

$$-h^2+2\mu r-Cr^2+\mu' r^4=0,$$

and then, after expanding to the *second* power of $r-\frac{\mu}{C}$, the equation is to be solved as a quadratic. The same process of substitution must for the same reason be gone through to prepare the equation (C) for approximate integration. I need not pursue the investigation further, as the results of the subsequent steps are given in the Philosophical Magazine for April 1854, p. 279, and in the Supplement to the Philosophical Magazine for December 1854, p. 526. By far the most important result is, that the eccentricity of the moon's orbit has a special value depending on the disturbing force.

The following considerations appear to me proper for proving that, whatever be the law of the disturbing force, and whether it be central or not, the motion of the apse is uniform if the eccentricity be a function of the disturbing force. Let the undisturbed orbit be an ellipse described about the focus, and let the three bodies be always in a given plane, the central body having a fixed position. A straight line being drawn from the centre of the fixed body in any direction in the plane of motion, the radius-vector of the disturbed body at the instants it passes this line has different values in successive revolutions. The change of value in each complete revolution depends on the disturbing force, in such a manner, that the function by which it is expressed would vanish if the disturbing force vanished. The total fluctuations, in the case of a uniform apsidal motion, are the same in all directions; and the difference between the extreme values of the radius-vector in any given direction depends both on the partial fluctuations, and on the rate of the angular motion of the apses. Hence as the partial fluctuations, as well as the motion of the apses, would vanish with the disturbing force, it follows that the difference between the extreme values, and consequently the eccentricity of an orbit nearly circular, is a function of the disturbing force.

I adduced an argument similar to the above in the Philosophical Magazine for April 1854 (p. 282), which I subsequently rejected (Phil. Mag. for August 1854, p. 108), because it appeared to lead to the inference that the eccentricity is simply proportional to the disturbing force. But by taking account, as is done above, of the rate of angular motion of the apse, this inference by no means follows, and the eccentricity is still a function of the disturbing force, although the form of it may be discoverable only by analytical investigation.

It is particularly to be remarked, that the above general reasoning only proves that, whatever be the law of the disturbing force, and whether it be central or not, for a certain value of the eccentricity the motion of the apse will be uniform. But it does not prove, that, under particular conditions, for instance, when the disturbing force is wholly central, and the law of it is given, uniform motion of the apse may not be consistent with any value of the eccentricity. In a similar manner, the general theorem, that every mass, whatever be its form, has three axes of permanent rotation, is not invalidated by finding that masses of particular forms have an unlimited number of axes of permanent rotation. Hence the fact, that when the central force is $\frac{\mu}{r^2}+\frac{\mu'}{r^3}$, the motion of the apse is uniform whatever be the eccentricity, is no argument against the general proposition, unless it should appear that in this instance the eccentricity can under no circumstances be a function of the disturbing force. This, however, is not the case; for the equation which gives the apsidal distances for that law of force is

$$h^2-\mu'-2\mu r+\mathrm{C}r^2=0,$$

from which it follows that

$$e^2=1-\frac{h^2\mathrm{C}}{\mu^2}+\frac{\mu'\mathrm{C}}{\mu^2},$$

and consequently that e is a function of μ' if $h^2\mathrm{C}=\mu^2$.

I think that I have now established the truth, in all essential particulars, of the new views respecting the moon's orbit, which were first published in the Philosophical Magazine for April 1854; and that I have pointed out a step towards a great simplification of the lunar theory. I have not yet had time to carry the method through the third approximation.

Cambridge Observatory,
January 11, 1855.

P.S. In the reasoning of the foregoing article it has been assumed, in accordance with the view generally taken by mathematicians, that analysis fails to give the true developments of the moon's radius-vector and longitude when the approximation commences from a fixed ellipse. Since the article was written, I have ascertained that no failure occurs *on that account* in the approximations to the first and second orders of small quantities, the results being so far the same, whether the approximation commences from a fixed ellipse, or is conducted by the process which I have shown to be necessary if $\frac{dr^2}{dt^2}$ contain the disturbing force as a factor. This statement may be verified as follows.

Let the force be wholly central, and equal to $\mu u^2 - \frac{\mu'}{u}$, u being the reciprocal of the radius-vector. Also let μ' be a small quantity whose powers above the first are neglected, and the orbit be nearly circular. By a known equation, we have

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} + \frac{\mu'}{h^2u^3} = 0.$$

Omitting the small term, let the result of the integration be

$$a(1-e^2)u = 1 + e\cos\theta.$$

Substituting this value of u in the small term, expanding, and omitting powers of e above the first,

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} + \frac{\mu' a^3}{h^2} = \frac{3\mu' a^3 e}{h^2}\cos\theta;$$

or, putting for shortness' sake w for $u - \frac{\mu}{h^2} + \frac{\mu' a^3}{h^2}$,

$$\frac{d^2w}{d\theta^2} + w = \frac{3\mu' a^3 e}{h^2}\cos\theta.$$

Now if this equation be integrated by the ordinary method applicable to exact equations, the integral will contain a term which may increase indefinitely with the time, just as in the lunar theory. But no such result follows if the two integrations be performed in succession, and regard be had to the rules of approximation. After multiplying the equation by $2dw$, and putting for dw, on the right-hand side, its approximate value $-\frac{e}{a}\sin\theta\, d\theta$, we get by integration,

$$\frac{dw^2}{d\theta^2} + w^2 = \mathrm{C} - \frac{3\mu' a^2 e^2}{h^2}\sin^2\theta.$$

Hence, since approximately,

$$d\theta\sqrt{\mathrm{C} - w^2} = -dw \text{ and } \mathrm{C}\sin^2\theta = \mathrm{C} - w^2,$$

it follows that

$$d\theta = -\frac{dw}{\sqrt{\mathrm{C} - w^2}} - \frac{3\mu' a^2 e^2}{2h^2\mathrm{C}}\,d\theta.$$

But $h^2=\mu a$ and $C=\frac{e^2}{a^2}$ nearly. Therefore, by integration,

$$w=\sqrt{C}\cos\left\{\theta\left(1-\frac{3\mu' a^3}{2\mu}\right)\right\},$$

which is the true approximation. As both approximations commenced from a fixed ellipse, it thus appears that the failure of the first is *wholly due to the mode of integration.* The occurrence of a like failure in the lunar theory is to be explained in the same manner.

This reasoning shows that the truth of my theorem respecting the eccentricity of the moon's orbit will be tested only by proceeding to the third approximation, which takes account of the effect of the tangential force on the motion of the apses.

Cambridge Observatory,
January 22, 1855.

XVII. *On the Adjustment of Crystals for Measurement with the Reflective Goniometer.* By W. H. M.

THE Philosophical Magazine for December last contains a description of a contrivance for adjusting a crystal for measurement on Wollaston's goniometer. Any observer who has taken the trouble to read the very clear directions for using the reflective goniometer, given more than thirty years ago by the late W. Phillips in his 'Mineralogy,' and by Mr. Brooke in his treatise on Crystallography, will find in this instrument, as usually constructed, ample provision for making the intersection of any two faces parallel to the axis of the graduated circle. The descriptions of the use of this instrument, to which reference has been made, are accompanied by figures. In these figures it will be seen that the pin which carries the plate to which the crystal is cemented, is represented in a position nearly parallel to the plane of the graduated circle. In this position, the rotation of the pin round its own axis, and the angular motion of that part of the branch into which the pin is inserted, are sufficient and necessary for the adjustment of the crystal. But when the branch is turned so that the axis of the pin coincides with that of the graduated circle, as represented in the figure in the Philosophical Magazine, that part of the power of adjusting the crystal which depends upon the rotation of the pin round its own axis is entirely lost. In addition to this inconvenience, the branch intercepts the vision of one of the signals, and thus renders an observation impossible, through no less than about 75° out of 360°. When the instrument is properly handled, the vision of the signals is uninterrupted through an entire revolution of the circle.

XVIII. *On Galvanic Circuits in which Perchloride of Iron enters as an Element.* *By* Professor Buff*.

SOLUTION of perchloride of iron has, as far as I know, not hitherto been used as a liquid conductor in the formation of galvanic circuits. The facility with which this body furthers the oxidation of hydrogen and the electro-positive metals, combined with the possibility of producing it at a cheap rate, led to the supposition that it might be very advantageously used as an element in a constant electric circuit. Occasional observations which Liebig had made appeared to confirm this supposition, and gave rise to the following investigation of the subject.

The first experiments, made with a very pure, almost neutral perchloride of iron, did not answer the expectations formed. An ordinary charcoal zinc pair was used, in which nitric acid was replaced by solution of perchloride of iron of syrupy consistence. To measure the current, a tangent galvanometer was employed, the simple ring of which was 200 millims. in diameter. A stream regulator was inserted in order to obtain currents of different intensities. It was found that even the weakest, which only just produced an action on the needle, showed no constancy. At the same time the charcoal was covered with metallic iron mixed with oxide of iron.

Metallic iron dipped in solution of perchloride changes it, as is well known, into protochloride, being at the same time dissolved. This occurred with the iron deposited on the charcoal when allowed to stand some time in the solution. During the progress of the electric decomposition the time was too short for this; the charcoal polarized itself with the iron, and no constant current could result.

When the neutral, saturated solution of perchloride was decomposed between platinum poles, chlorine was received at the positive pole. At the negative pole iron was deposited in minute, hard, crystalline laminæ, accompanied by a very scanty evolution of hydrogen. With platinum poles of greater dimensions, the hydrogen disappeared. A small addition of hydrochloric acid to the solution prevented the deposition of the iron perfectly, even with a current of considerable strength. Hydrogen was only perceptible when the negative pole had a comparatively small surface. On the other hand, the evolution of hydrogen increased as soon as the solution was much diluted with water.

The perchloride, in a concentrated solution containing hydrochloric acid, does not appear to be electrolyzed. Its presence seems to serve only to convey fresh chlorine to the hydrogen separated from the hydrochloric acid. As long as this occurs with sufficient rapidity, no hydrogen is visible, and the negative polar surface is not polarized.

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* From the *Annalen der Chemie und Pharmacie*, vol. xcii. p. 117.

Supported by these experiments, from four to five per cent. of hydrochloric acid was added to the perchloride of iron, and the circuit established as before. A small quantity only of HCl is required, as it is always renewed during the progress of the experiment.

The circuit thus composed showed a satisfactory constancy as long as the deviation did not exceed 14°. Currents of 18° showed, a short time after the circuit had been closed, a slight decrease. The inconstancy of the current increased slowly but gradually with a deviation of 30° or 40°. The time was manifestly too short to allow the hydrogen separated on the charcoal to be changed again into hydrochloric acid; for when the circuit was opened a short time, the original strength of the current was regularly established. The deviation of 14° remained unchanged even when the circuit was kept closed longer than two hours. A current which deviated the needle 45° would, according to experiment, have separated in 31 seconds 1 millegramme of hydrogen. By a deviation of 14° the same quantity requires accordingly 124 seconds. The constancy of this electromotive couple extends therefore only to those currents whose chemically decomposing power does not exceed 1 millegramme hydrogen in 124 seconds.

Manifestly this only holds good for an element of the dimensions employed. They were the usual ones; a charcoal cylinder of 5·7 centimetres interior width by 13·5 centimetres high, of which, however, only 8·5 centimetres were in contact with the solution; the zinc cylinder, 9 centimetres high and 4 centimetres wide, dipped in sulphuric acid of 1·25 spec. grav., contained in a porous cell which almost filled the cylindrical excavation of the charcoal. Of course with a greater extent of the electromotor, particularly when a greater surface of the charcoal was placed in contact with the liquid, constant streams of greater strength would be obtained.

A still more favourable result presented itself when the sulphuric acid in the porous cell was changed for a concentrated solution of common salt.

The constancy extended now to a deviation of 22° (one millegramme of hydrogen in 77 seconds), and the retrograde motion of the needle by higher deviations was smaller than in the preceding case. There was, further, neither deposition of iron, nor evolution of gas at the charcoal cylinder; not even when the circuit was directly closed by a short, thick, copper wire. A fine platinum wire was kept in a state of incandescence.

Compared with other galvanic elements, the perchloride of iron element, as far as power and constancy is concerned, is inferior to Bunsen's, but superior to Daniell's. Without replacing the former, it may be preferred in many cases to the latter. A Daniell's battery of equal surface did not show an equal constancy above a deviation of 12°.

In the latter, a gradual intermixture of the liquids by endosmose occurred more rapidly, and was attended with greater disadvantages than with the perchloride of iron circuit. For in the sulphate of copper circuit, the zinc covers itself by degrees with copper, and loses thereby a part of its exciting force; while the perchloride of iron which gradually penetrates into the porous cell, although it assists to dissolve some of the zinc, produces no precipitate of iron on the surface of the zinc cylinder, and consequently leaves its activity unchanged.

In order to measure the force of the chloride of iron circuit, I employed a method which is of easy execution, without being on that account less accurate. It is founded on the proposition which directly follows from Ohm's law, that "when, in electric circuits which are subject to one and the same very great resistance, alterations in this resistance occur, of such smallness that they exert no perceptible influence on the strength of the current, then the strengths of these various currents is directly proportional to the electromotive force which produces them."

In the *Annalen der Chemie und Pharmacie*, vol. lxxxvi. p. 1, I described a tangent galvanometer with a long multiplication coil, the fine copper wire of which offers a resistance to conduction, which is measured by a German silver wire, 28,000 metres long by 1·5 millim. thick. With this resistance, of itself very great, others were combined, by rolling on long and thin wires, which amounted to 2·9 to 3·1 times as much. Against this amount of resistance, that of a constant element of ordinary size manifestly vanishes, for this is seldom greater than one or two metres German silver wire. If, then, various constant elements, all of inconsiderable resistance, are closed with that long wire, and the multiplication coil united with it, we may assume that their electromotive forces are as the strengths of the currents which set them in motion, and by which the needle of the tangent galvanometer is deviated.

It is stated in the paper above referred to, under what conditions and within what limits the tangents may be taken as a satisfactory expression of the strength of the current. The numbers are, however, only then sufficiently accurate to be compared, when they relate to observations made simultaneously, or, at least, almost so. For the intensity of the force by which the deviated galvanometer needle is recalled to its position of rest is subject to certain fluctuations, whose disturbing influence often exceeds the limits of the accuracy to be obtained by this kind of measurement. These fluctuations appear only in a smaller degree to be dependent on the changes of the earth's magnetism, or on the changeable elasticity of the thread by which the needle hangs; in a greater measure, on the contrary, on such magnetic actions as are neither to be avoided, nor entirely governed, in a building not expressly erected for magnetic observations. I have avoided, I think, all such disturbances, by referring all measurements of electromotive force to the force of a constant element of particular construction, which, as I have shown (Liebig's *Annalen*, vol. lxxxv. p. 1), maintains, for months, an absolute constancy. In order to show the use of this proceeding, I add some experiments which were made on four consecutive days in the month of February 1853, with the view of comparing the force of Bunsen's charcoal zinc element with those of the before-mentioned constant element.

The actions of a zinc-charcoal element on the needle are marked with β, those of the constant element, whose force served as unity of measurement, with α. Each value of α and β is the mean of two deviations caused by the same current; the one east, and the other west of zero. The numbers standing in the same horizontal line relate to observations which were made in immediately successive times.

	β.	α.	β.	α.	$F=\frac{\log\beta}{\log\alpha}$.
1.	64°·45	49°·05	64°·45	49°·03	1·8159
2.	64·60	49·175	64·55	49·175	1·8175
3.	64·20	48·75	64·25	...	1·8160
4.	64·85	49·50	64·85	49·55	1·8175
					1·8167

The charcoal cylinder in these four experiments was always the same, but the apparatus was put together each time afresh. The force remained, as is seen, unaltered. From the direct results of the deviations, small fluctuations might have been calculated upon. These fluctuations affected, however, the two constant pairs equally, and depended consequently on external influences.

In the same manner we obtained for a Daniell's pair,—

α.	β.	α.	β.	$F=\frac{\log\beta}{\log\alpha}$;
49°·0	49°·7	49°·15	49°·65	1·0201

and for another element, when the amount of resistance in the long wire was changed,—

50°·1 50°·975 50°·1 1·0316.

The investigation of the perchloride pair (slight addition of HCl, with sulphuric acid in the porcelain cell) led to these results:—

	α.	β.	α.	F.
1.	50°·1	58°·3	50°·1	1·3538
2.	30·3	58·5	50·35	1·3536

When more hydrochloric acid was added to the solution of perchloride of iron, the electromotive force of the element sunk to

$$F=1·3250.$$

Finally, with perchloride of iron and a little hydrochloric acid, and solution of salt in the porous cell, we obtained,—

$$F=1·3908.$$

This constant galvanic apparatus stands thus almost exactly in the middle between the constant charcoal element and the constant copper element. Before the latter, it has the preference of greater force and constancy; while in common with it, it has the advantage that it may be used in confined apartments without evolving injurious vapours.

XIX. *On the new Hygrometer or Dew-point Instrument. By* A. Connell, *F.R.S.E., Professor of Chemistry in the University of St. Andrews.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, St. Andrews, Nov. 29, 1854.

I BEG to request that you will do me the favour of giving insertion in your Journal to the following observations of Dr. Buist of Bombay, contained in a letter which he was so good as to address to me, after making use, on his voyage to India, of the hygrometer described by me in your Number for last August. Dr. Buist is well known as a distinguished meteorologist, both theoretically and practically, and presided over the Meteorological Observatory at Bombay for a considerable time, and in that capacity published many highly valuable reports on this science. I may therefore well consider any opinion of his in regard to a meteorological instrument as possessing peculiar value. Unluckily his hand-writing is occasionally so indistinct and so full of contractions, that the task of reading his letters, however interesting they may be in their details, is frequently a very difficult one; and on the present occasion I have found it to be impossible to make out many portions of his letter, and an individual here accustomed to his hand was equally unsuccessful. What, however, I have quoted below has been distinctly made out; and it is quite sufficient to establish two points; *first*, that Dr. Buist found this hygrometer fully to answer its purpose as a dew-point instrument; and *secondly*, that he prefers it to the elegant hygrometer of the late Professor Daniell, partly on the ground of its less liability to accident. The other grounds I have not been able to make out. The passages distinctly made out are as follows:—

"Malta, July 19, 1854.

" . . . . . I have been using it (the hygrometer) on board and found it to answer beautifully, till I broke the thermometer the day before yesterday by the slightest possible slip of the finger. . . . . The superiority of your instrument over Daniell's is, that you can make the . . . .

	Therm.	Wet-bulb.	Dew-point.	Barom.
12th July, Gibraltar, Noon .	70	65·9	60	30·164
3 P.M.	72	68		
13	77	72	68	
14	76	72	69	

I have no hesitation in pronouncing your instrument as superior to that of Daniell, inasmuch as it is stronger and less liable to accident, while it admits of a thermometer of . . . . If yours can be constructed as cheap as Daniell's, there can be no doubt of its preferability.

"Ever yours, most truly,
"GEO. BUIST."

In one part of the letter which I could not fully decipher, Dr. Buist appears to make some suggestions for strengthening the thermometer against the risk of accident in travelling, as he mentions that it had been broken in a short journey in the neighbourhood of London, although subsequently repaired. It appears to me, however, that there must have been either some defect in the packing of the thermometer in the case which Dr. Buist had obtained, or that it had been accidentally exposed to some unusual violence; the result of my own experience being, that it is in all respects quite strong enough to stand all the ordinary concussion of travelling. I carried several of the instruments, with their thermometers, during last summer from this place to London, and afterwards to Hastings and back again here, a distance in all of more than 1000 miles, without any thermometer or any part of the instruments being broken; and during the whole journey they were simply placed in their proper cases in my portmanteau, and underwent the usual not very gentle handling of the railway officials. I have also made repeated innocuous journeys between this and Edinburgh with the instrument.

It may, however, be a very proper precaution for anyone going to a distance from a town where good instrument makers are to be found, to carry with him one or more spare thermometers duly packed in their proper cases. One or two spare ivory collars might also be useful in case of accidental fracture of that part of the instrument.

If an accident happens to Daniell's instrument, the injury is irreparable; but if a thermometer or collar happens to give way in the hygrometer under consideration, the damage is easily set right at a trifling expense. The same brass stopper will always answer again.

I ought to have mentioned formerly, that a little key of brass, to be fitted, when required, into the ivory collar at O, is employed in screwing and unscrewing the ivory collar; and that the other extremity of this key may be inserted with advantage into the neck of the little brass bottle, to aid in the same operations regarding the bottle.

In cold and dry weather in winter it is very essential that the æther should be the very best commercial æther, and probably fresh æther used for each observation. In summer, the superior excellence of the æther is by no means so important; and the used æther of winter might be kept in a properly stopped bottle for summer use, with a slight addition of fresh æther, as formerly explained. Spare leather washers ought to accompany every instrument. These often require change, to adjust the relative positions of the brass bottle and syringe.

I omitted also to mention formerly, that when an accurate observation has been accidentally missed at first, or when from any cause it is wished to repeat an observation, the temperature ought to be allowed to rise 8° or 10° above the point to which it has fallen, before it is again slowly reduced; because as soon as the exhaustion is stopped, the comparatively warmer air heats the exterior surface of the bottle faster than the liquid and the thermometer are heated; and if the cooling is not again begun from 8° or 10° higher than the temperature has fallen to, the dew-point would be got *too low*, the surface taking some time to cool again.

I have the honour to remain,
Your very faithful Servant,
A. CONNELL.

XX. *On the Products of the Destructive Distillation of Animal Substances.*—Part III. *By* THOMAS ANDERSON, *M.D., Regius Professor of Chemistry in the University of Glasgow*[*].

IN the preceding parts of the investigation of the products obtained by the destructive distillation of animal substances, I have entered fully into the method of treating the raw material, and have shown the existence in it of not less than three different series of bases; one, that of which methylamine is the type; a second, of which picoline is an example; and a third series, not yet further examined, to which the provisional name of pyrole bases has been applied. Besides these, aniline is also met with, but whether as an isolated substance, or accompanied by the other members of its series, cannot be determined, as none of them possess sufficiently distinctive reactions to permit their detection in a complex mixture.

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[*] From the Transactions of the Royal Society of Edinburgh, vol. xxi. part 1; read April 17, 1854. [Part I. appeared in the September Number of this Journal for 1848; Part II. in our Number for December 1851.]

To the series of which picoline is a member my attention has hitherto been specially directed, and chiefly owing to the interest attaching to these bases from their identity in composition with the corresponding members of the aniline series, aniline and picoline being the first instance in which the isomerism of two organic bases, of which we have now so many examples, was distinctly made out. In the second part of the investigation, three members of the series in question are described, namely,—

Pyridine . . . . $C^{10} H^5 N$
Picoline . . . . $C^{12} H^7 N$
Lutidine . . . . $C^{14} H^9 N$

of which the two latter are isomeric with aniline and toluidine. It was further remarked, that the phænomena observed seemed to indicate that the members of this series present in Dippel's oil did not terminate with lutidine, but that bases of higher atomic weight and boiling-point manifestly existed in it. The object of the present paper is to show that this statement was well founded, by giving a description of another member of the series, and further to define their true constitution.

On pursuing the distillation of the different fractions of basic products obtained by the process described in the second part of this investigation, and distilling at temperatures above 305°, which is about the boiling-point of lutidine, it was found that, when converted into platinum salts, the per-centage of platinum gradually diminished as the boiling-point rose. Taking advantage of the well-known empirical law, that the boiling-points of homologous substances rise by 34° of Fahrenheit for every addition of $C^2 H^2$ to the atom, and from which the boiling-points of pyridine, picoline, and lutidine do not greatly differ, I directed my attention to the portion of mixed bases boiling about 340°, in which it was reasonable to expect that the next base of the series should be found. But even after repeated rectifications, the base distilling at this temperature still gave a very powerful reaction of aniline with chloride of lime, and the per-centage of platinum in its double compound was but little lower than that of the lutidine salt, or at all events never reached the number required by theory for the higher base. Being convinced that the separation of two bases approximating so closely in their boiling-points as aniline and the substance I expected to find, could not be effected by fractionated distillation, or at least only by an expenditure of time, labour and material, altogether out of proportion to the importance of the object to be attained, I endeavoured to accomplish it by crystallization. Having observed that the other members of the picoline series gave highly soluble and even deliquescent oxalates, I conceived that by converting the mixed bases into salts of that acid, it would be easy to separate the rather sparingly soluble and highly crystallizable oxalate of aniline, and obtain the oxalate of the other base in a state of purity. But this expectation was not confirmed by experiment; for neither from the portion boiling about 340°, nor even from that collected at 360°, and corresponding therefore with the boiling-point of pure aniline, could the slightest trace of crystallized oxalate of aniline be obtained, although both fractions gave the reaction of that base in the most powerful manner. The experiment was varied in every possible way by the use of water, spirit, and absolute alcohol, but by allowing these fluids to evaporate spontaneously only a thick syrup was obtained, without the slightest indication of crystallization. Even the addition of æther to its alcoholic solution gave only a syrupy fluid, and no crystals; and I was forced to conclude, that, even in the portion of the mixed bases corresponding to the boiling-point of aniline, its quantity was so small in proportion to the other substances, that the properties of its salts were entirely masked by them.

Not succeeding in obtaining the pure aniline, and so separating it from the other base, the question came to be, how to get rid of the former substance in the best possible way. For this purpose I availed myself of the extreme stability of the bases of the picoline series mentioned in my former paper, which is so great that they resist even the action of strong nitric acid, by which aniline is entirely destroyed. When the base, boiling between 340° and 345°, is mixed rapidly and in large quantity with nitric acid, much heat is evolved, and a brisk action takes place; and if the portion boiling about 360° is employed, the action is so violent as to be almost explosive, and it is requisite to add the base drop by drop to the acid, which must be kept carefully cool. The acid fluid acquires a deep red colour, and on boiling, red fumes are abundantly evolved, accompanied by an odour resembling that of bitter almonds. After the action has ceased, the fluid becomes muddy when mixed with water, and a thick reddish-yellow oil is deposited, which has exactly the odour of nitrobenzide, and resembles it in many of its properties. The quantity of this substance produced is by no means large, and it is evidently mixed with some resinous substance. Owing to this circumstance I have not been able to submit it to purification and analysis, so as to ascertain whether it really is nitrobenzide; but though that substance has not yet been obtained by a similar action on pure aniline, it is quite possible that it may be produced, and the reason why it has not hitherto been observed is probably because no one has had occasion to sacrifice large quantities of aniline in this manner. The acid solution of the undecomposed base is passed through a wet filter in order to separate the oil, and the fluid boiled for some time to expel the last traces. On saturation with potash and distillation, an oily base passed over with the water and collected on the surface. This base, on being converted into a platinum salt, was found still to give a result greatly above that required by theory for the substance of which I was in quest. On submitting it to distillation, it was found to commence boiling at about 320°, and hence to contain a large quantity of lutidine; and it was only the very last portion which gave a platinum salt corresponding with theory. It was clear that a large quantity of lutidine had been retained at a boiling-point above that which naturally belonged to it, by the presence of aniline, and that substance being destroyed, it came over at its natural boiling-point. The higher fractions of the oil containing aniline were therefore treated in a similar manner, and the undecomposed bases, which amounted to from a half to two-thirds of the original quantity acted on by nitric acid, were submitted to fractionated distillation. The product was found to spread over a considerable number of degrees, and a quantity of that collected between 340° and 345° was converted into a platinum salt and analysed, but the results indicated the presence of much lutidine. The product being still obviously impure, was submitted to a systematic fractionation, and it was observed that the thermometer remained remarkably steady about 354°. The portion boiling between 350° and 360° was collected apart, and after several rectifications a fraction was obtained which distilled entirely between 352° and 356°, and proved to be the pure base, to which I give the name of collidine.

Collidine.

Collidine is obtained in the form of a transparent and colourless oil, which may be preserved for a long time in bottles only partially filled with it, without acquiring colour. A rod dipped in hydrochloric acid brought in contact with it gives abundant white fumes. It is insoluble in water, and floats on its surface, without undergoing diminution. It dissolves a small quantity of water, which is readily separated by caustic potash. It is highly soluble in alcohol, æther, and the fixed and volatile oils. It dissolves with great facility in the acids, but even when added in large excess it does not neutralize them. It precipitates alumina, chromium, zinc, and peroxide of iron from their solutions, but gives no precipitate with baryta, lime, magnesia, manganese or nickel. It throws down oxide of lead from the nitrate, but not from the acetate, a remarkable peculiarity, which it shares with methylamine and æthylamine. With corrosive sublimate it forms a double salt, but from salts of the suboxide of mercury it throws down the oxide. Its odour is strong, aromatic, and far from unpleasant. Its specific gravity is 0·921, and it boils at 354°. The following results were obtained by analysis:—

I. 4·075 grs. of collidine gave 11·800 grs. carbonic acid and 3·450 grs. water.

II. 4·079 grs. of collidine gave 11·800 grs. carbonic acid and 3·393 grs. water.

III. 4·124 grs. of collidine gave 11·980 grs. carbonic acid and 3·560 grs. water.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon .	78·97	78·89	79·22	79·33	C^{15}	96
Hydrogen	9·40	9·24	9·58	9·09	H^{11}	11
Nitrogen	11·63	11·87	11·20	11·58	N	14
	100·00	100·00	100·00	100·00		121

These numbers correspond with the formula $C^{15} H^{11} N$. Collidine forms, therefore, another member of the picoline series, and corresponds in constitution with the base described by Cahours, under the name of xylidine, in the aniline series, with which, however, it is isomeric only, and not identical, its properties being different in all respects.

The salts of collidine are for the most part highly soluble and deliquescent. When evaporated, they form uncrystallizable gummy masses, some of which, on standing, show traces of crystallization. They are soluble also in alcohol, but not in æther. The only highly crystallizable compounds are the mercury and platinum double salts.

The mercury double salt is thrown down in the form of a curdy-white precipitate, on the addition of a solution of corrosive sublimate to a solution of the hydrochlorate of collidine. It dissolves in boiling spirit, and is deposited, on cooling, in needles. It could not be obtained of definite composition.

Platinochloride of Collidine is obtained when strong solutions of hydrochlorate of collidine and bichloride of platinum are mixed. It is slowly deposited in the form of orange-yellow prisms or needles, according to the degree of concentration of the fluids. It is readily soluble in water, but insoluble in alcohol and æther. Its analysis gave the following results:—

I. 6·013 grs. of platinochloride of collidine gave 6·345 grs. carbonic acid and 1·937 gr. water.

II. 5·040 grs. of platinochloride of collidine gave 5·360 grs. carbonic acid and 1·650 gr. water.

I. 5·620 grs. of platinochloride of collidine gave 1·705 gr. platinum.

II. 4·535 grs. of platinochloride of collidine gave 1·362 gr. platinum.

III. 5·097 grs. of platinochloride of collidine gave 1·657 gr. platinum.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon .	28·77	29·00	...	29·33	C^{16}	96
Hydrogen	3·57	3·63	...	3·66	H^{12}	12
Nitrogen	...	...	...	4·31	N	14
Chlorine	...	...	...	32·54	Cl^3	106·5
Platinum	30·33	30·03	29·89	30·16	Pt	98·7
				100·00		327·2

These results correspond with the formula $C^{16}H^{11}N, HCl+PtCl^2$, and entirely confirm the constitution of the base. The rest of its salts have not been particularly examined, as they did not present anything of interest.

[To be continued.]

XXI. *Notices respecting New Books.*

A Treatise on Frictional Electricity. By Peter Theophilus Riess. Berlin: 1853.

THE materials for a treatise on electricity lie in part scattered through former treatises, and, in detached portions, through the pages of scientific journals. A portion of these materials is doubtful; a part false; another part true, and therefore destined to live. To choose wisely from such a mass—to preserve the true, reject the false, and assign to the doubtful its proper value, requires a correctness of judgment which a practical cultivation of this portion of science can alone bestow. How many of our scientific books are compiled by persons who themselves have no clear conception of the things concerning which they write, and who therefore introduce confusion and dismay into the minds of their readers! For it is not by the mere withholding of knowledge that such writers do harm; this would be a negative evil, but it is not the worst; it is the sapping of the reader's faith in his own ability, the forcing upon him the conviction that he has no natural capacity for such studies, that does most mischief. If a scientific hand-book be defective in clearness or unsound in argument, the reader who comes to it for instruction, not possessing a sufficient amount of culture to enable him to form an independent judgment, accepts the fallacy, and attributes the absence of clearness to his own, rather than to his teacher's want of understanding, whereas the latter is often the true cause of failure. Now there is no task more responsible than that of the man who undertakes to write a book for the instruction of the community in science, and no man ought to embark in such an undertaking who is not prompted by higher motives than the mere hope of commercial success. He must love his subject. Years of alliance with it must have made him master of it up to its boundaries, and enabled him to detach the hypothetical from the true. He must know the true value of those images by which natural truths are connected, and rendered intelligible to the human mind; a knowledge necessary to preserve him from confounding the symbols of science with its verities. He must himself be an investigator, and his skill as such must be ratified by his own discoveries and his manner of communicating them, before his qualifications are complete.

Few, we imagine, possess the requirements to which we have alluded, in a greater degree than the writer of the book before us. His lifetime has been spent in the investigation of the subject on which he writes. On almost every page we find evidence that the man is not writing from hearsay merely, but that the experiments he records have been repeated by himself, and come to us with the sanction of his own authority. There is no portion of frictional electricity which has been left untouched: on all points the reader will obtain ample information; and while he reads, he may carry with him the comforting assurance that he possesses a teacher worthy of his confidence. There are many things introduced into this treatise which we have observed in no other; many, indeed, that we owe to the author himself, and which constitute the most important contributions to this portion of science which have been recently made.

To the man who wishes to pursue electricity as an earnest study this work will prove of great value, abounding, as it does, with practical suggestions which are founded on the experience of a lifetime. To the mathematician who desires a safe basis for his calculations the book will prove eminently useful. Many portions are beyond the range of the mere popular reader; they are addressed to the student; but even the popular reader, by omitting the portions thus referred to, will find all he needs remaining. He must not, indeed, expect to meet in the work an encyclopædia of electrical amusements: the writer has applied himself earnestly to his task, and has chosen those experiments which illustrate the principles of his science, and which prompt to further inquiry. His vocation is to teach, not to amuse; and to him who desires to obtain a comprehensive view of the laws and phænomena of this portion of science, we are acquainted with no work which we can recommend with greater confidence than the treatise before us.

The work consists of two volumes, large octavo; it is amply illustrated, and the plates are beautifully executed. The manner, indeed, in which the work is presented to the public reflects great credit upon its publisher.

XXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 73.]

Nov. 16, 1854.—Colonel Sabine, R.A., V.P. and Treas., in the Chair.

THE following communication was read:—

Letter from Lieutenant Maury to Admiral Smyth, For. Sec. R.S.

"National Observatory, Washington,
October 21, 1854.

"SIR,—I have the honour to state, for the information of the Royal Society, that a new asteroid was discovered here by Mr. James Ferguson, Assistant Astronomer, at 11 P.M., 2nd of Sept. 1854.

"He was observing Egeria at the time, and found that, the 13th, and this, the 31st, in the field together.

"I have delayed this communication, waiting to ascertain whether the planet might not have been discovered by observers in other parts of the world; and it appearing that it had not, the priority of the discovery, therefore, belongs to the National Observatory; and this new star is added to the family of asteroids as the first representative of America among them, and a memorial of her zeal in the cause of astronomy.

"As a testimony of the high appreciation in which the talents and the industry of Mr. Ferguson are held, the honour of naming this planet was left to him. Following the rule adopted by astronomers with regard to the asteroids, he has selected the graceful name of Euphrosyne.

"Its approximate ephemeris, with the last observations, are herewith enclosed.

"I have the honour to be,
"Respectfully, &c.,
"M. F. MAURY,
"Lieut. U.S.N."

"*Rear-Admiral W. H. Smyth, R.N.*"

Ephemeris of Euphrosyne.

M. T. Washington.		α.	δ.
	h m s	h m s	° ′ ″
1854, Oct. 19.	9 26 41·9	1 12 15·11	−1 56 7·21

Elements of Euphrosyne, computed by Prof. Keith, from observations of Sept. 2nd, 6th and 10th.

M.	13° 36′ 33″·3
Π	352 5 50·6
Λ	33 29 21·7
i	22 39 13·6
ϕ	4 22 30·2
log a	0·469530
log μ	2·845712

Sept. 2·721 M. T. Greenwich.
M. Equ. 1854 0.

Ephemeris for October.

M. T. Berlin.	α.	δ.	log r.	log Δ.
	h m s	° ′ ″		
1854, Oct. 19·5	1 12 0	1 59 21	0·43828	0·24622
23·5	1 7 49	1 47 29	0·43850	0·24937
27·5	1 3 49	1 33 49	0·43873	0·25345
31·5	1 0 3	1 18 18	0·43897	0·25861

Nov 23, 1854.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read :—

"On the Impregnation of the Ovum in the Stickleback." By W. H. Ransom, M.D.

I purpose placing before the Royal Society in this communication, the principal results of experiments made during the months of June and July last, on the impregnation of the ovum in *Gasterosteus leiurus* and *G. pungitius*, and hope to be able to furnish a more detailed account of my observations on a future occasion.

The ovarian ovum of these fishes, at a very early stage of its development, is provided with a proper investing membrane, the future chorion. At a later period, one portion of this membrane presents a number of cup-shaped pediculated bodies scattered over its surface, and in the centre of this part of the chorion there is a funnel-shaped depression, pierced by a canal which leads towards the centre of the egg.

In the nearly ripe ovum, the germinal vesicle occupies an excentric position with respect to the egg as a whole, but imbedded in the centre of a semi-solid accumulation of fine granular matter at that part of the surface which corresponds to the funnel-shaped depression; so that the apex of the funnel, projecting inwards beyond the level of the inner surface of the chorion, makes a depression in the centre of the layer of granular matter, and comes nearly into contact with the germinal vesicle.

For convenience of description, the funnel-shaped depression will now be called *micropyle*, and the layer of granular matter before impregnation, *discus proligerus*.

The germinal vesicle disappears before the ovum leaves the ovary, and no remnant of it or its spots can be seen.

A very delicate membrane invests the yelk within the chorion; this membrane is more distinct after impregnation, or after the action of water upon an unimpregnated egg; it may be isolated, and then exhibits a remarkable degree of elasticity. It is not a *yelk-membrane*, and it will be spoken of as the *inner membrane*.

The layer of the yelk immediately internal to the inner membrane passing over the discus proligerus, is formed by yellowish highly refractive drops which disappear in water, undergoing some remarkable changes, and by a fluid substance which water precipitates in a finely granular form.

The principal mass of the yelk consists of a clear and very consistent albumen. The oil is collected into a few very large drops which come up to the surface.

When the ovum escapes from the ovary, it enters a cavity which may be considered as the ovarian extremity of the oviduct, in which a considerable quantity of clear viscid fluid is previously secreted and collected, to be expelled with the ova.

More exact observation of the micropyle in the free eggs proves that the inner end of the canal is either open, or at most closed by a very delicate membrane. When looking into the funnel from the wide mouth, the apex being in focus, a bright, clear, round or oval spot, such as an aperture would produce, is always visible. If a section be made of the egg, and the apex brought into focus from within, the same clear spot is well seen, and the fine and regularly dotted structure of the chorion is seen to cease suddenly at the margin of the clear spot.

The general form of the egg after deposition is round, but it is rendered irregular by indentations caused by the pressure of other eggs. It is inelastic, and retains impressions made in it by a needle; and when placed in water, these characters remain for a long time if it be not impregnated,—a fact which indicates that water does not pass through the micropyle, or by imbibition through the chorion. The viscid secretion of the oviduct which invests the eggs may defend them against the action of water, in which it does not readily diffuse or dissolve. This secretion has an alkaline reaction. The substance of the yelk has a decidedly acid reaction,—more than enough to neutralize the alkalinity of the viscid secretion. This reaction is, I believe, due to a peculiar organic acid, but the experiments relating to this question are not yet complete. The seminal particles of the male continue to move for a considerable period in the viscid secretion which envelopes the ripe ova, but they very quickly become still in water.

In the act of impregnation one or more (as many as four have been seen) spermatozoids pass into the micropyle, and probably by their proper motion overcome the obstruction which prevents the entrance of water. Actively moving spermatozoids may remain in contact with the chorion for eighteen minutes at least without producing any sensible change in the ovum, provided none of them enter the micropyle, but when one is seen to enter, in about a quarter of a minute a change is observable.

The changes which are observed to follow the entrance of the spermatozoids into the micropyle are the following:—In about a quarter of a minute the tube is shortened, and very soon a clear space becomes visible within the chorion near the micropyle: this space, or respiratory chamber, gradually extends to the opposite pole of the egg and increases in diameter, as does also the whole ovum. During the formation of this space the surrounding fluid enters through the micropyle, and this gradually retracts and is at length closed. This entrance of fluid into the egg effaces the depressions, restores the round form, and makes it firm and elastic; but does not cause any such precipitation of granular matter as is produced by its artificial introduction.

While the respiratory chamber is yet in progress of formation, the yellow drops of the superficial layer of the yelk grow pale and disappear; the change beginning near the micropyle. As a result of this, the whole egg becomes clearer, and the *discus proligerus*, which may be now more correctly denominated the *germinal mass*, is more distinct.

The yelk now very slowly alters its form, one surface becoming flattened; but about fifteen or twenty minutes after impregnation a remarkable and more vivid contraction begins, causing the yelk to pass through a series of regularly recurring forms. The contraction begins on one side near the equator, and soon forms a circular constriction which gives the yelk the figure of a dumb-bell, the longer axis of which is the polar axis of the egg. The constriction travels towards the germinal pole, and next produces a flask-shaped figure; this is at length lost by the constriction passing on, and the round form is regained in about a minute. This wave reappears and travels forward again without any distinct period of rest, and I have seen these movements continue for forty-five minutes, though towards the latter part of this period they are less distinct and more limited in extent. The germinal mass has itself during these contractions, which strongly resemble the peristaltic movements of the intestine, undergone changes in form, and has increased in bulk and distinctness. These movements are unaffected by weak galvanic currents.

During the passage forward of each wave of contraction there is an oscillation of the whole mass of the yelk, so that its germinal pole passes once to the right and once to the left of the micropyle, to which it at first corresponded. The plane of this oscillation may be vertical, horizontal, or inclined, but always cuts the micropyle; it begins and ceases with the contractions already mentioned, and would seem to be a mechanical result of them.

For some time before cleavage begins, the only changes of form are the appearance of wave-like elevations and depressions along the under surface of the germinal mass, and its alternate concentration and diffusion. Cleavage begins in about two hours after impregnation; no embryonic cell was observed before it began, nor in any of the cleavage masses.

The inner membrane is folded in during cleavage; it is easily seen thrown into folds at the cleft, and for this reason I do not consider it a *yelk-membrane*, which term would be better applied to the chorion.

"On the Applicability of Gelatine Paper as a Medium for Colouring Light." By Horace Dobell, Esq.

The object of this communication is threefold.

(1.) To point out the properties of a material called Gelatine Paper, which render it applicable as a medium for colouring light.

(2.) Through the means of gelatine paper, to introduce the use of coloured light in the arts for the preservation of the sight of artisans.

(3.) To introduce the use of gelatine paper for the relief of persons suffering from impaired vision; for the preservation of the sight of travellers, and of all those who are much engaged in reading.

This material was invented in 1829 by the late M. Grenet, of Rouen, and was exhibited by him in its present state of perfection at the Great Exhibition of 1851. But up to the present time it has not been successfully applied to any more useful purposes than the manufacture of artificial flowers, address-cards, tracing-paper, wafers, wrappers for confectionary, and the like.

It is commonly manufactured in sheets, measuring 22 inches in length and 16 inches in diameter, which are sold at a small price; but the sheets can as easily be made of any dimensions not exceeding those of which plate-glass is capable. It can be made of any thickness, from that of the finest tissue paper upwards. It may be obtained as transparent as the best glass, and more free from colour, or of all colours and shades of colour, without interfering with its transparency. It is exceedingly light, and may be bent or rolled up without injury. It can be cut with scissors like ordinary paper, and may easily be stitched with a needle and thread. By means of an aqueous solution of gelatine, it can be made to adhere accurately to plates of glass without any interference with its transparency. When varnished with collodion it becomes perfectly waterproof, more pliable, capable of bearing a considerable degree of heat without injury, and its transparency is not affected.

Hence it appears, that, in addition to its transparency and susceptibility to various colours and forms, gelatine paper is cheap, portable, and durable.

Such being the properties of the material, the following are enumerated by the author as some of the forms in which he suggests that it may be employed, and in which it has already been found useful.

1. A small sheet of very pale green or blue gelatine paper, to be used in reading. The sheet is simply to be laid upon the page of the book, and the reading to be conducted through the coloured medium. If used in a faint light, the reading paper is to be raised a little from the book to admit more light beneath it.

2. A sheet of gelatine paper of pale green set in a light frame, and placed like a screen before the window or lamp of the engraver, the watchmaker, the jeweller, and the like; thus providing a light of genial colour in which they may pursue their occupations.

3. A similar appliance to the last-mentioned for the use of needlewomen. For this purpose screens are to be provided, both of green and of blue gelatine paper; so that the white materials employed in needlework may be changed to a pleasant green, by the screen of that colour, the yellow materials to a green by the blue screen, and by one or other of these screens the reds softened down into violets or browns.

4. For either of the two last purposes on a larger scale, the gelatine paper may be attached to the window glass of the apartment, thus colouring, if necessary, all the light admitted during daylight.

5. Shades for the eyes in certain affections of the sight, to take the place of the green or blue silk and card shades worn by many persons. The gelatine paper being transparent, will allow the wearer to see his way about, at the same time that the eyes are protected from a glaring light. This may be especially useful in cases where it is desired not only to shade a diseased eye, but also to protect its nerves from strong light admitted by the sound eye. When not only coloured light but a certain degree of darkness is required, this can be readily and delicately graduated by employing shades of different depths of colour.

6. Masks of gelatine paper for protecting the eyes of travellers against the glare of snow-fields and of sandy deserts.

XXIII. *Intelligence and Miscellaneous Articles.*

ON THE LIMIT OF VAPORIZATION OF MERCURY. BY C. BRAME.

FROM the numerous experiments which I have made on this subject, it appears,—1, that gold, which was employed for the purpose by Mr. Faraday, is not a sufficiently sensitive reagent for the detection of very small quantities of the vapour of mercury; and that if, as Faraday has done, we are able by this means to prove that at 68° to 77° F. mercury gives off vapour to a height of more than $0^{m}\cdot100$, and if the formation of this vapour can no longer be shown at more than $0^{m}\cdot020$ when the temperature is below 32° F., this does not prove that mercury has a limited atmosphere. 2. On the contrary, it is proved that the vapour of mercury rises easily to 1 metre at temperatures comprised between a few degrees below 32° F and 86° F. Moreover, it has been ascertained in M. Chevreul's laboratory, that when a tube of a diameter of $0^{m}\cdot050$, open at both ends, was placed over mercury at a temperature of 68° to 78° F. for a fortnight, the vapour of the latter gave a brown colour to vesicles of sulphur placed at a distance of $1^{m}\cdot440$ from the surface of the mercury. In the vaults of the observatory, vesicles of sulphur placed at a distance of $1^{m}\cdot760$ from the mercury were rendered entirely brown in four months; the sulphur was placed in a small tube open at both ends, and supported by a spiral iron wire in a larger tube closed at one end, and reversed over the mercury. The larger tube was $0^{m}\cdot021$ in diameter, and $1^{m}\cdot80$ in height. In twenty days, in the same situation, the vapour of 30 grms. of mercury, placed at the bottom of tubes from $1^{m}\cdot100$ to $1^{m}\cdot200$ in height, gave a brown colour to vesicles of sulphur deposited on the sides of little flasks which served as stoppers. The same result was obtained several times, the temperature sometimes remaining constant, as in the vaults of the observatory, sometimes varying 18° F. or more, as in the laboratory, &c.; the air in the tubes was sometimes more or less free, sometimes confined, without the appearance of any sensible difference. At all temperatures it is always sulphuret of mercury that is formed; at 32° F. and above that temperature it is of a deeper or paler brown, but at 18° F. the vapour of mercury gave a yellow or red colour to vesicles of sulphur placed at a distance of several centimetres; the vapour of soft amalgams had often the same effect. In the former case, by continuing the experiments for a sufficiently long time, ponderable quantities (0·006 to 0·0230 gr.) of mercury were condensed in the vesicles of sulphur, and the same was the case with mercurial ointment, amalgams of silver, tin, &c. Vesicular sulphur, the needles produced by fusion and hardened sulphur, which contain vesicles, take up mercury, even when placed at the bottom of tubes of half a metre in length resting upon mercury.

3. When, under the circumstances already indicated, vapour of iodine (produced in very small quantity at the ordinary temperature by a few centigrammes of iodine volatilized and condensed on the walls of a small flask serving as a stopper for the tube) descending from the upper part of the tube containing the mercury was substituted for the vesicular sulphur, the vapour of mercury appeared to form a limited atmosphere above the liquid metal. In fact, in this case, it

did not appear to rise beyond $0^{m}\cdot036$ to $0^{m}\cdot038$ at 69° F., and only to $0^{m}\cdot020$ to $0^{m}\cdot022$ at $53^{\circ}\cdot6$ F., as appeared from the iodides of mercury which were deposited upon the walls of the tube. In the vaults of the observatory, it is true, the red iodide formed a very thin ring, and this ring was only at a distance of a few millimetres from the mercury; this, which could not be foreseen from the previous results, must be attributed to the moisture, the iodine tubes having been fixed against the wall. In fifteen days, at a temperature of $72^{\circ}\cdot5$ F. in the meridian chamber of the observatory, an amalgam of silver gave rise to a much larger ring with vapour of iodine. This ring was formed of very distinct crystals of determinable form; it was at $0^{m}\cdot015$ from the amalgam. In most of the experiments with iodine, the ring formed consisted of several iodides of the metal, but the red crystallized iodide formed the greater portion, or existed alone when the vapour of iodine being in slight excess, the experiment was stopped at the proper time. By means of a tube of 1 metre in length placed vertically in a larger tube resting upon mercury, John Davy's experiment was verified, proving that iodine, placed at a distance from mercury at the ordinary temperature, absorbs the vapour of the latter. This agrees with the result furnished by vesicular sulphur, and shows well that if the mercurial atmosphere appears to be limited when the iodine is in the state of vapour, this arises solely from the circumstance, that the vapour of iodide of mercury, and even that of iodine, have a considerable density compared with the air. Hence it results that the vapour of mercury is repelled to a certain limit which varies with the temperature, whilst the rings formed by the iodides of mercury are at a tolerably constant height at the same temperature. The vapour of bromine at ordinary temperatures has the same action as that of iodine, as also the vapour of turpentine and gaseous chlorine at 167° F. &c.

Conclusions.

1. Vapour of mercury appears to be subjected, like other vapours, *to the law of mixture of gases and vapours*, at all temperatures between a few degrees below 32° F. and 86° F.; and all the experiments performed with sulphur in various states concur to show that the limit set to the vaporization of mercury in consequence of the exclusive employment of gold as a reagent, is exceeded beyond anything that could have been foreseen from Faraday's experiments, as it has been ascertained that the vapour of mercury can rise to a height of $1^{m}\cdot76$ at a constant temperature of $52^{\circ}\cdot7$ F.

2. If vapour of iodine, under certain circumstances, gives a result which at first sight appears to indicate that vapour of mercury forms a limited atmosphere, this appears to depend entirely upon the considerable density, compared with the air, of the vapour of iodine and of that of the iodide or iodides formed, and upon the slight volatility of the latter. The same conclusion applies to vapour of bromine and gaseous chlorine.

3. On the other hand, the volatility of the product formed by the combination of vapour of mercury with sulphur, and the very small quantity of vapour of comparatively small density furnished by sul-

phur at ordinary temperatures, remove all difficulties in explaining the results obtained with this substance. The vesicles of sulphur absorb the vapour of mercury with great energy, and when sulphur is employed, nothing prevents the mixture of air with the vapours produced. The principal result obtained in this case may therefore be considered the normal result.

4. By bringing together the fact observed by Faraday, which is otherwise perfectly correct, and the views of Poisson upon the state of the last strata of the terrestrial atmosphere, the limitation of the vapour of mercury has been employed in the highest discussions of general physics, both in treating of the limitation of the atmosphere of the earth and of the divisibility of matter; questions which, since the time of Wollaston, have been regarded as correlative. The results of my experiments appear to me to prove that it is impossible to refer to the limitation of the vaporization of mercury in speculations of this nature.—*Comptes Rendus*, Nov. 20, 1854, p. 1013.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1854.

Chiswick.—December 1. Clear and fine. 2. Overcast: very large halo round the moon. 3. Cloudy: clear. 4. Clear and fine. 5. Fine: cloudy: clear at night: rain. 6, 7. Fine: cloudy. 8. Densely overcast. 9. Cloudy. 10. Clear and cold. 11. Sharp frost: fine. 12. Very fine. 13. Hazy: overcast: rain. 14. Densely clouded. 15. Dusky flying clouds: overcast: rain. 16. Cloudy: fine. 17. Clear: fine: rain. 18. Rain: heavy clouds: clear and cold: rain. 19. Very fine: rain. 20. Stormy and wet. 21. Fine: rain: overcast. 22. Cloudy and boisterous: rain at night. 23. Fine: cloudy: overcast: rain. 24. Fine: rain. 25. Densely clouded: rain: clear and fine. 26. Clear and fine. 27. Fine. 28. Clear and frosty: very fine: frosty. 29. Overcast. 30. Cloudy: fine. 31. Overcast: cloudy: boisterous.

Mean temperature of the month	39°·35
Mean temperature of Dec. 1853	32 ·49
Mean temperature of Dec. for the last twenty-eight years ...	39 ·64
Average amount of rain in Dec.	1·50 inch.

Boston.—Dec. 1, 2. Fine. 3. Cloudy: rain P.M. 4. Fine. 5. Cloudy. 6. Fine: rain A.M. and P.M. 7. Fine. 8. Cloudy. 9. Fine: rain and snow P.M. 10, 11. Fine. 12. Fine: rain P.M. 13. Cloudy. 14. Cloudy: rain A.M. 15. Fine. 16. Fine: rain A.M. 17. Fine. 18. Rain A.M. and snow P.M. 19. Fine: rain P.M. 20. Rain A.M. 21. Cloudy: rain A.M. 22. Cloudy. 23, 24. Fine. 25. Cloudy: rain P.M. 26. Cloudy. 27. Fine: rain A.M. and P.M. 28. Fine. 29—31. Cloudy.

Sandwick Manse, Orkney.—Dec. 1. Snow-showers A.M.: hail-showers P.M. 2. Snow-showers A.M.: cloudy P.M. 3. Rain A.M.: showers P.M. 4. Showers A.M. and P.M. 5. Showers A.M.: cloudy P.M. 6. Showers A.M. and P.M. 7. Cloudy A.M.: sleet-showers P.M. 8. Showers A.M.: sleet-showers P.M. 9. Showers A.M. and P.M. 10. Bright A.M.: snow P.M. 11. Cloudy A.M.: showers P.M. 12. Sleet-showers A.M.: showers P.M. 13. Cloudy A.M.: showers, thunder and lightning P.M. 14. Sleet-showers A.M.: showers P.M. 15. Sleet-showers A.M. and P.M. 16. Hail-showers A.M.: sleet-showers P.M. 17. Sleet-showers A.M.: hail-showers, frost P.M. 18. Hail-showers, frost A.M. and P.M. 19. Cloudy, frost A.M.: rain P.M. 20. Sleet-showers A.M.: clear P.M. 21. Drizzle A.M.: showers P.M. 22. Damp A.M.: sleet-showers P.M. 23. Hail-showers A.M.: sleet-showers P.M. 24. Showers A.M.: sleet-showers P.M. 25. Showers A.M.: hail-showers P.M. 26. Snow A.M.: hail-showers P.M. 27. Snow A.M.: snow, clear P.M. 28. Snow, cloudy A.M.: rain P.M. 29. Drizzle A.M. and P.M. 30. Sleet-showers A.M.: hail-showers P.M. 31. Showers A.M.: rain P.M.

Mean temperature of Dec. for twenty-seven previous years .	41°·10
Mean temperature of this month	39 ·13
Mean temperature of Dec. 1853	38 ·97
Average quantity of rain in Dec. for fourteen previous years	3·95 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1854. Dec.	Max.	Min.		9¼ a.m.	8¾ p.m.	Max.	Min.		9¼ a.m.	8¾ p.m.						
1.	29·730	29·422	29·08	29·12	29·30	51	30	37	32	32	w.	w.	nw.	....	·12	·19
2.	29·951	29·768	29·40	29·42	29·64	46	30	37	32	31½	nw.	nw.	se.	....	....	·19
3.	29·937	29·762	29·46	29·18	28·92	50	41	42	46½	44½	nw.	w.	wsw.	....	....	1·10
○ 4.	30·025	29·894	29·55	29·41	29·11	50	38	42	42½	43	w.	wnw.	s.	....	·02	·09
5.	29·509	29·318	29·15	28·57	28·57	51	32	47·5	44	42	sw.	sw.	ssw.	·04	....	·26
6.	29·656	29·424	28·98	29·12	29·76	45	31	40	41½	38	w.	w.	nw.	....	....	·15
7.	30·137	30·035	29·71	30·00	29·42	42	23	34	37	36	n.	nnw.	s.	....	·13	·05
8.	29·892	29·483	29·44	29·13	29·02	50	33	42	45½	40	sw.	wsw.	w.	·01	....	·45
9.	29·634	29·442	29·07	29·23	29·69	45	30	36	39	37	w.	nw.	nw.	·04	....	·28
10.	30·057	29·833	29·56	29·91	29·70	39	19	33	38	35	n.	nw.	sse.	....	·10	·03
11.	30·045	29·856	29·68	29·24	29·15	[illegible]	24	27	47½	40	sw.	sw.	wsw.	....	....	·17
☾ 12.	30·057	30·002	29·32	29·42	29·22	45	30	34	40½	43	sw.	w.	sw.	·03	....	·22
13.	30·190	29·967	29·80	29·22	29·24	52	42	37	41	40	sw.	sw.	w.	·02	·18	·16
14.	30·017	29·945	29·42	29·40	29·50	55	49	51	42½	41	sw.	w.	w.	·01	·05	·40
15.	30·112	29·808	29·46	29·45	29·50	59	42	51	38	37	w.	w.	nw.	·09	....	·16
16.	29·869	29·657	29·32	29·50	29·53	47	26	39	37½	39	w.	w.	wnw.	....	·58	·12
17.	29·939	29·752	29·55	29·35	29·27	45	33	33	39	33	nw.	w.	wnw.	·10	....	·41
18.	29·464	29·002	28·73	29·28	29·61	39	29	38·5	35	35	n.	n.	nw.	·14	·09	·18
● 19.	29·730	29·701	29·40	29·45	28·94	44	33	29	35	38½	w.	nnw.	se.	·36	·04	....
20.	29·955	29·406	29·14	29·46	29·84	44	24	36	39½	38	n.	n.	nw.	·08	·16	·62
21.	30·125	29·953	29·72	29·51	29·48	51	39	36	47	43	sw.	w.	sw.	·12	·04	·12
22.	29·882	29·828	29·35	29·23	29·37	54	28	53	41	37	sw.	w.	nw.	·06	·07	·33
23.	29·895	29·778	29·48	29·56	29·50	47	26	37	38	38	sw.	w.	wnw.	·05	....	·18
24.	29·879	29·768	29·44	29·32	29·19	52	40	34	41½	41½	sw.	w.	sw.	·06	....	·34
25.	29·778	29·528	29·10	28·66	28·94	54	33	51	36	36	sw.	wnw.	nw.	·04	....	·18
☽ 26.	29·818	29·722	29·36	29·17	29·29	46	32	37	33½	35	w.	w.	wnw.	....	·08	·13
27.	29·948	29·811	29·44	29·62	30·02	41	25	34	25	36	nw.	wnw.	nw.	·01	·02	·20
28.	30·431	30·313	30·02	30·25	29·94	40	20	30	32	38	nw.	nw.	s.	....	·03	·20
29.	30·443	30·396	30·07	29·77	29·86	41	34	33	48½	48	sw.	wnw.	sw.	·01	....	·25
30.	30·444	30·354	29·97	29·60	29·76	47	32	44	41	42½	sw.	w.	nw.	....	....	·24
31.	30·328	30·101	29·87	29·60	29·21	47	35	44	46	45	nw.	w.	nw.	....	....	·35
Mean.	29·963	29·775	29·45	29·392	29·402	47·03	31·67	39·0	39·45	38·81				1·27	1·71	7·75

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[FOURTH SERIES.]

MARCH 1855.

XXIV. *Further Observations on associated cases, in Electric Induction, of Current and Static Effects. By* Professor FARADAY, *D.C.L., F.R.S.* &c.*

MELLONI, whose loss science must deeply feel, was engaged in the latter part of his life in investigations relating to static electricity, especially concerning induction, conduction, &c. He desired, in reference to these and the results I had published respecting the charge of, and conduction by, subterraneous and subaqueous insulated wires†, to know whether there was any difference in the *time* of transmission through such wires, of currents having greater or less intensity, *i. e.* of currents from batteries of different numbers of plates. I applied to Mr. Latimer Clark on the subject; and he with the same earnestness as on the former occasion, sought and seized the opportunity of making experiments of the like kind, and gave me the results, which I sent to Melloni. The latter published them with some observations in an Italian Journal (whose title is not on the paper which he sent to me), and soon after he was suddenly removed from us by death. As Mr. Clark's results are not yet known in this country, I have thought that a brief account of them would be valuable. His process records, by the printing telegraph of Bain, the results obtained with 768 miles of copper wire covered with gutta percha, and laid in the ground in four lines between London and Manchester, so connected that the beginning and the end of the whole length was in London. The following are his words, dated May 31, 1854:—

"I have tried a few experiments on the relative velocities of currents of different intensities, and I enclose you some strips of paper showing the results. I was unable to equalize the deflections of a galvanometer by currents of intensity with small plates as compared with currents from a few large plates, for no size of plate would make up for the deficiency in intensity. I allude to the form of experiment suggested by Melloni;—but I believe they will be of interest to him.

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* Communicated by the Author.

† Royal Institution Proceedings, i. 345; or Phil. Mag. 1854, vii. p. 197.

"The experiments were made through 768 miles of gutta percha wire, viz. from London to Manchester and back again twice, with our ordinary sulphate of copper batteries, plates 3 inches square, and with intensities varying from 31 cells to sixteen times 31 cells, or 500 cells.

"In the accompanying strips the upper line indicates the time during which the current was sent, being made by a local arrangement.

"The second line (of dots) indicates *time by seconds*, being made by a pendulum vibrating seconds, and striking a light spring at the centre of its arc of vibration.

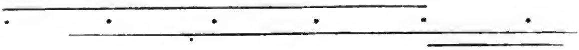
"The third line indicates the time at which the current appeared at (what we may call) the distant end of the line, 768 miles off.

"The fourth line merely shows the residual discharge from the near end of the wire, which was allowed to communicate with the earth as soon as the batteries were disconnected; this has no reference to the subject of our inquiries.

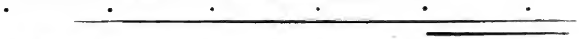
"It will be seen by the *third line*, that about two-thirds of a second elapsed in every case before the current became apparent at the distance of 768 miles, indicating a velocity of about 1000 miles a second; but the most interesting part appears to be, that this velocity is *sensibly uniform for all intensities* from 31 cells to 500."

Melloni has then given a copy of the records made when 31 pair and 500 pair of plates were employed; unfortunately the copy is inaccurate, since it makes the fourth line commence as to time at the termination of the third, whereas it ought to correspond with the termination of the first; also the third line on each does not thin off as those upon the record do. The following is a copy from other slips obtained at the same time from the Bain's printing apparatus. Experiments with 62, 125, and 250 cells, gave like results with those of 31 and 500 cells.

31 cells.



500 cells.



After certain observations, which are mainly upon the manner of the experiments, and the way in which practical difficulties were avoided, Melloni says, "It appears, then, that when the electric current possesses sufficient force to overcome the sum of the resistance offered by a given conductor, whatever its length may be, an augmentation of its intensity ten or twentyfold does not alter the velocity of its propagation. This fact is in open contradiction with the general meaning attributed to the denominations of *quantity* and *intensity*; since the first compares the mass of electricity to that of a fluid, and the second represents its elasticity or tendency to motion. The equal velocity of currents of various tension offers, on the contrary, a fine argument in favour of the opinion of those who suppose the electric current to be analogous to the vibrations of air under the action of sonorous bodies. As sounds, higher or lower in pitch, traverse in air the same space in the same time, whatever be the length or the intensity of the aërial wave formed by the vibration of the sonorous body; so the vibrations, more or less rapid or more or less vigorous, of the electric fluid excited by the action of batteries of a greater or smaller number of plates, are propagated in conductors with the same velocity. Every one will see how the hypotheses imagined by us to give a reason for natural phænomena, will serve to suggest certain experimental investigations, the results of which will test their validity or insufficiency."

Melloni then says that he shall shortly have occasion to publish facts which clearly demonstrate the errors of certain conclusions admitted up to the present time respecting electro-static induction; and I am aware, from written communications with him, that he considered the results arrived at by Coulomb, Poisson, and others since their time, as not accordant with the truth of nature*. In the mean time he died, and whether his researches are sufficiently perfected for publication or not, I do not know.

The uniformity in the time and appearance of currents of different intensities at the further end of the same wire in the same inductive state, is a very beautiful result. It might at first be supposed to be in opposition to the views I set forth some years ago on induction and conduction, and the statements more recently made with regard to *time*. That, however, does not appear to me to be the case, as a few further observations on Mr. Clark's recent experiments will perhaps show. When the smaller battery is used, much less electricity passes into the wire in a given time, than when the larger one is employed. Suppose that the batteries are so different that the quantities are as 1 to 10; then, though a pulse from each would take the same time for transmission through the wire, still it is evident that the wire would be a tenfold better conductor for the weak current than for the strong one; or in other words, that a wire having only one-tenth of the mass of that used for the greater current should be employed for the smaller one, if the resistance for *equal quantities* of electricity having different intensities is to be rendered equal.

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* He says, "I deceive myself much, or else the fundamental theorem of electrical induction, as we find it ordinarily announced, ought to be modified so as not to confound two effects completely distinct—the electric state during induction, and after the contact and separation of the inducing body. We know perfectly what occurs in the latter case, but not in the former," &c. Again, "In my last letter I raised doubts with regard to the consequences which have up to the present been deduced from the experiments serving as a base for the fundamental theorem of electro-static induction. These doubts have passed to a state of certitude in my mind, ... and behold me at this time thoroughly convinced that the enunciation of that theorem ought to be essentially modified." (July 1854.)

My views connect the retardation of the transmitted current with the momentary induction set up laterally by the insulated and externally coated wire. The induction will be proportionate to the intensity, and therefore its especial effect on the time of retardation proportionately diminished with the less intense current,—a result of action which will aid in rendering the time of retardation of the two currents equal.

The difference of *time* in the former experiments with air wires, and earth or water wires, very clearly depends upon the difference of lateral induction; the air wire presented a retardation scarcely sensible, the earth wire one amounting to nearly two seconds. If the insulating layer of gutta percha could be reduced from 0·1 to 0·01 of an inch in thickness, and mercury could be placed on the outside of that instead of water or earth, I do not doubt that the time would be still more increased. Yet there is every probability that in any one of these varying cases, electric currents of high and of low intensity would appear at the end of the same long wire after equal intervals of time.

Mr. Clark's results may be stated thus:—A given quantity of electricity at a high intensity, or a smaller quantity at a proportionally lower intensity, will appear at the further end of the same wire after the lapse of the same period of time. My statement assumed the discharge of the *same* quantity at *different* intensities through the *same wire*, and the quantities in the illustrative experiments were measured by a Leyden jar. In the consideration and further development of these results, it must be remembered that it is not the difference either in time, velocity, or transmission of a *continuous* current which constitutes the object in view, for that is the same both for an air wire and a subterraneous wire, but it is the difference in the *first appearance* only of the same current when wires under these different conditions are employed. After the first appearance both wires are alike in power unto the end of the current, and then a difference again appears which is complementary to the first.

There are many variations of these experiments which one would wish to make, if possible, and perhaps by degrees the possibility, or else equivalent experiments in other forms, may occur. If the wire employed were changed from a cylinder to a flat ribbon of equal weight, or to several small wires, all being equally coated with gutta percha and submerged, differences would probably arise in the time of delay with the same current; and I think that the ribbon, presenting more induction surface than the cylinder, would cause more delay; but probably any one of these, or of like varieties, would cause the *same* delay for currents of different intensities. Again, one can scarcely doubt that with different conducting substances, as iron and copper, the delay would vary, as is the case in the transmission of sound and light. That the delay for currents of high and low intensity should be the same for the same wire in any one of such cases may still be expected, but it would be very interesting to *know* what would be the fact.

The prosecution of these results and the principles concerned in them, through the various forms they may assume by such like variations of the conductors and also of the currents, offers, as Melloni has observed, most extensive and interesting inquiries: even the power of a current to induce a current in neighbouring wires and conductors is involved in the inquiry, and also the phænomena and principles of magneto-electric induction.

Royal Institution, Feb. 7, 1855.

XXV. *On certain Propositions in Algebra connected with the Theory of Probabilities. By* GEORGE BOOLE, *LL.D., Professor of Mathematics in Queen's College, Cork*[*].

BEFORE entering upon the immediate subject of this paper, I wish to state the connexion in which it stands with my previous papers on the theory of probabilities, published in this Journal.

If the reader will refer to what I have said in the conclusion of my second paper, published in the August Number of this Journal for 1854, he will find that the claims of the doctrine which I have put forth are made to rest upon its satisfaction of the following requirements:—

1st. That the principles upon which its methods are founded should be of an axiomatic nature.

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[*] Communicated by the Author.

2nd. That they should lead to results capable of verification, wherever verification is possible.

3rd. That they should be capable of a systematic development consistent in all its parts and processes, and neither acknowledging nor imposing any limitations but those which exist in the nature of things.

Now the principles in which, and in the mode of the application of which, that doctrine consists, have been fully stated in this Journal (December 1854). In so far as they may be considered novel, they are certainly in no proper sense mathematical. I do not indeed conceive that they are in any respect novel, except that they are brought into a novel connexion, and are made the basis of a new application. I do not think that any person competent to form an opinion upon these subjects would dispute such positions as the following: viz. 1st, that probability is *relative*—that it depends, not upon the actual connexions of things, but upon those connexions *as known* to us; 2nd, that the distinction of events into simple and compound is relative, and the selection of simple terms arbitrary and the offspring of language; 3rd, that merely logical connexions which are founded upon definition possess only a derived necessity, and that from the contemplation of things or events *defined*, and subject *as such* to relations founded upon definition, we may ascend to the contemplation of things free, and only to be expressed by signs of larger comprehension, but from which, by introducing the limitation again, the conception of the more restricted class may be derived. I say, for instance, that between the term "king" and some other term which we will suppose to designate a member of a particular legislative body, relations founded upon definition of the respective terms might exist, which relations would cease to exist if we ascended to the larger terms of "ruler" and "legislator," and would be restored when we imposed upon those terms the former limitations. Now these are principles which are not in any proper sense mathematical. They would seem to belong far more to the psychologist and to the inquirer into the nature of language than to the mathematician. I have, however, shown that in connexion with received principles they form a valid basis for the theory of probabilities; and in my treatise on the Laws of Thought numerous verifications of the results of that theory will be found. I proceed now to that which I consider the most important of all, the proof that this theory satisfies the third and last of the conditions above adverted to.

The argument which I purpose to develope is the following. In the demonstration of the general method in probabilities, published in this Journal (December 1854) and in the Laws of Thought, we ascend, in accordance with the third of the principles above stated, from the events whose probabilities are given, and whose logical connexion is explicitly determined from their *definition*, to events which are free, and from which by limitation the former are derived. Of those "free" events, the unknown probabilities s, t, &c. are involved in the final algebraic equations to which the method conducts. As *probabilities* they ought to admit of positive fractional values. Now it will be shown that they do admit of such values when the data represent a possible experience, and not otherwise. The conditions of possibility in the data are identically the conditions of mathematical consistency in the method. Other important consequences are connected with this demonstration, to which attention will be directed.

Proposition I.

Let V *be a rational and integral function of the* n *unknown quantities* $x, y, \ldots z$, *consisting wholly of positive terms, and involving all such terms as can be formed without introducing powers of* $x, y, \ldots z$ *higher than the first; also let* V *represent the sum of those terms in* V *which contain* x *as a factor,* V_y *the sum of those which contain* y *as a factor, and so on. Then the system of equations*

$$\frac{V_x}{V}=p, \quad \frac{V_y}{V}=q \ldots \frac{V_z}{V}=r, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$p, q, \ldots r$ *being positive fractions, admits of one solution, and of only one, in positive values of the quantities* $x, y, \ldots z$.

To exemplify this proposition, let us suppose

$$V=axyz+byz+cxz+dxy+ex+fy+gz+h,$$

then it is affirmed that the system of equations

$$\frac{axyz+cxz+dxy+ex}{axyz+byz+cxz+dxy+ex+fy+gz+h}=p \quad . \quad . \quad (2)$$

$$\frac{axyz+byz+dxy+fy}{axyz+byz+cxz+dxy+ex+fy+gz+h}=q \quad . \quad . \quad (3)$$

$$\frac{axyz+byz+cxz+gz}{axyz+byz+cxz+dxy+ex+fy+gz+h}=r, \quad . \quad . \quad (4)$$

all the coefficients a, b, c, &c. being positive and the quantities p, q, r positive and fractional, admits of one, and only one, solution in positive values of x, y, z.

It is evident that the proposition is true when $n=1$; for then we have $V=ax+b$, and the system (1) is reduced to the single equation

$$\frac{ax}{ax+b}=p,$$

whence

$$x=\frac{bp}{a(1-p)};$$

and this single value is positive if a, b, and p are positive, and the last fractional.

Now the general proof of the proposition consists in showing, that if it is true for a particular value of n, it is true for the next greater value, and so on *ad infinitum*. Wherefore, since it is actually true for the case of $n=1$, it is true universally.

We shall therefore begin by assuming that the proposition is true for $n-1$ variables. And making this assumption, let us suppose the first $n-1$ equation of the system (1) to be satisfied while z varies from 0 to ∞, and seek under these circumstances the nature of the variation which the first member of the final equation of the system undergoes.

Now if a particular positive value be given to z, the $n-1$ first equations of the system (1) will assume the form of a corresponding system relative to the $n-1$ variables x, y, ... which remain. Thus if in the equations (2) and (3) we give to z a particular positive value, and then make

$$az+d=\mathrm{A},\ \ cz+e=\mathrm{B},\ \ bz+f=\mathrm{C},\ \ gz+h=\mathrm{D},$$

those equations will become

$$\frac{\mathrm{A}xy+\mathrm{B}x}{\mathrm{A}xy+\mathrm{B}x+\mathrm{C}y+\mathrm{D}}=p,$$

$$\frac{\mathrm{A}xy+\mathrm{C}y}{\mathrm{A}xy+\mathrm{B}x+\mathrm{C}y+\mathrm{D}}=q,$$

A, B, C, D being known positive constants; and this is a binary system derived from the function

$$\mathrm{V}=\mathrm{A}xy+\mathrm{B}x+\mathrm{C}y+\mathrm{D},$$

just as the system (2), (3), (4) was derived from the expression for V by which it was preceded.

Hence if the proposition be true for a system involving $n-1$ unknown quantities, it will be true for that system which is formed by giving to z in the $n-1$ first equations of (1) a particular positive value. And therefore to each value of z there will correspond a single set of positive values of the $n-1$ unknown quantities x, y, ... determined by the solution of that system. That set of values will of course vary as z varies. Moreover, when $z=0$, the first member of the final equation will be 0. This is evident from the form of that member as developed in (4), or in any other special instance. When $z=\infty$, the first member of the final equation is 1, as is in like manner made evident. Hence as z varies from 0 to ∞, while the $n-1$ first equations of the system (1) are satisfied, the function $\frac{V_z}{V}$ in the final equation varies continuously from 0 to 1. In such variations it must at least once coincide with the value r. Hence there must be at least one solution of the entire system (1) in positive values of $x, y, \ldots z$. It remains then to show that there cannot be *more* than one such solution. If there be more than one such solution, the value of $\frac{V_z}{V}$ must, in the variation above described, more than once coincide with r; and therefore, varying continuously, it must between such points of coincidence admit of a maximum or minimum value. But it may be shown that there exists no such value. The nature of the proof of this proposition will be best illustrated by a particular example.

Let us take the particular case in which $n=2$. Our equations then are

$$\frac{axy+bx}{axy+bx+cy+d}=p, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{axy+cy}{axy+bx+cy+d}=q; \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and we are to inquire if it is possible to satisfy, by positive values of x and y, the conditions

$$\frac{axy+bx}{axy+bx+cy+d}=p,$$

$$\frac{axy+cy}{axy+bx+cy+d}=\text{maximum or minimum.}$$

Differentiating in the usual way, we find, after slight reductions,

$$(ay+b)(cy+d)dx+(ad-bc)xdy=0,$$

$$(ad-bc)ydx+(ax+c)(bx+d)dy=0,$$

from which, eliminating dx and dy,

$$(ay+b)(cy+d)(ax+c)(bx+d)-(ad-bc)^2xy=0;$$

or developing,

$$a^2bcx^2y^2+ab(ad+bc)x^2y+ac(ad+bc)xy^2+ab^2dx^2$$

$$2abcdxy+ac^2dy^2+bd(ad+bc)x+cd(ad+bc)y+bcd^2=0. \quad (7)$$

Now this equation, since its first member consists wholly of terms of positive sign, cannot be satisfied by any set of positive values of x and y. Hence, by the previous reasoning, the system (5), (6) admits of one, and of only one, solution in positive values of x and y.

The class of functional determinants, to use Jacobi's expression, exemplified in the first member of (7), and obtained in like manner from the ternary, quaternary, and higher systems derived from (1) by the assigning of the successive values of 3, 4, &c. to n, is possessed of remarkable properties. Representing by H_n the function thus formed from the system of n equations, I have obtained by a laborious analysis, the relation

$$H_n=V^{n-1}K, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where K is a rational and integral function of the n unknown quantities $x, y, \ldots z$, consisting wholly of positive terms. Thus, in the above case of $n=2$, where H_2 is expressed in full by the first member of (7), it would follow that

$$H_2=V(abcxy+abdx+acdy+abd), \quad . \quad . \quad . \quad (9)$$

wherein

$$V=axy+bx+cy+d, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

as may easily be verified. In this case the functions V and K are *reciprocally* related. If to V we assigned the actual form of K, we should find for K the form of V, with the addition of a constant factor.

When $n=3$, we have, in like manner,

$$H_3=V^2K, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

wherein

$$V=axyz+byz+cxz+dxy+ex+fy+gz+h;$$

and here K is a wholly positive function of the sixth degree, which, if we exhibit only its initial and final terms, may be thus expressed,

$$\begin{aligned}K=&\,abcdx^2y^2z^2+(adfc+abdc)x^2y^2z+(abce+acdg)x^2yz^2\\&+(abcf+abdg)xy^2z^2\ldots+efgh. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)\end{aligned}$$

The proof which I have obtained of the simply positive character of the function K is rigorous up to the case of $n=4$, and it is further of such a nature as to leave upon my mind no doubt of its *general* truth. Assuming, as I venture to do, that it is generally true, the argument stands thus:

Since H_n is wholly positive, the equation

$$H_n=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

admits of no solution in positive values of $x, y, \ldots z$. Hence the function $\frac{V_z}{V}$ in the final equation of the system (1) admits of no maximum or minimum value, while x, y, z are positive, and the $n-1$ first equations of the system are satisfied. Hence in the variation of z from 0 to $\propto$, there is one point, and only one, at which the entire system (1) is satisfied by positive values of $x, y, \ldots z$. Hence, finally, that system admits of one, and only one, solution in positive values of those quantities.

Proposition II. *with connected problem.*

If V *be a rational and integral function of the n variables $x, y, \ldots z$, consisting wholly of positive terms, but involving no powers of the variables higher than the first, and if V_x represent the aggregate of those terms in* V *which contain x as a factor, V_y the aggregate of those which contain y as a factor, and so on; then if we form the system of n equations*

$$\frac{V_x}{V}=p, \quad \frac{V_y}{V}=q \ldots \frac{V_z}{V}=r, \text{ \&c.}, \quad . \quad . \quad . \quad (14)$$

that system cannot have more than one solution in positive values of $x, y, \ldots z$. And in order that it may have one such solution, certain conditions must in general be satisfied by the quantities $p, q, \ldots r$; which conditions it is required to investigate.

Of the general proposition I shall not attempt to give here a fully developed proof. I purpose only to give such an account of the proof, together with special illustrations, as may place the reader in a position to supply deficiencies.

I shall begin with the investigation of the conditions between p, q, &c.

When x, y, z are positive, the first member of each equation of the system (14) is in general the sum of a series of proper fractions. Thus if

$$V=axyz+byz+cxz+dxy+e,$$

in which case the system (14) becomes

$$\left.\begin{aligned}\frac{axyz+cxz+dxy}{axyz+byz+cxz+dxy+e}&=p\\ \frac{axyz+byz+dxy}{axyz+byz+cxz+dxy+e}&=q\\ \frac{axyz+byz+cxz}{axyz+byz+cxz+dxy+e}&=r\end{aligned}\right\}; \quad . \quad . \quad (15)$$

and if we make

$$\frac{axyz}{V}=\lambda, \quad \frac{byz}{V}=\mu, \quad \frac{cxz}{V}=\nu, \quad \frac{dxy}{V}=\rho, \quad \frac{e}{V}=\sigma,$$

it is evident that λ, μ, ν, ρ, and σ will be proper fractions, and that the system will assume the form

$$\left.\begin{aligned}\lambda+\nu+\rho&=p\\ \lambda+\mu+\rho&=q\\ \lambda+\mu+\nu&=r\\ \lambda+\mu+\nu+\rho+\sigma&=1\end{aligned}\right\}, \quad . \quad . \quad . \quad . \quad . \quad (16)$$

the last equation resulting from definition. And to these equations we may add the inequations

$$\lambda \geqq 0, \quad \mu \geqq 0, \quad \nu \geqq 0, \quad \rho \geqq 0, \quad \sigma \geqq 0. \quad . \quad . \quad (17)$$

The mode of eliminating λ, μ, ν, &c. from the above combined systems is given in my paper 'On the Conditions by which the Solutions of Questions in the Theory of Probabilities are limited,' published in this Journal (August 1854). And the actual elimination leads to the following results: viz. 1st, that p, q, and r must be positive fractional quantities; 2nd, that they must satisfy the conditions

$$q+r \geqq p, \quad p+r \geqq q, \quad p+q \geqq r.$$

These conditions, then, *at least* must be satisfied in order that the system (15) may admit of solution in positive values of x, y, z. To the quantity r they assign the inferior limits $p-q$ and $q-p$, and the superior limits $p+q$ and 1.

Exactly the same method is applicable to every form and variety of the system (14), the final result always being a system of linear inequations connecting the fractional positive quantities p, q, . . r.

The conditions above determined are, moreover, the *only* ones which are necessary in order that the system (14) may admit of a solution in positive finite values of the variables x, y, . . z, that case alone excepted in which one of the variables appears as a common factor in, or is wholly absent from, the terms of V, such variable admitting then the values 0 and ∞. This proposition is true in the case of $n=1$, as is obvious; and it may be shown that if it is true for any particular value of n, it is true for the next greater value, and so on *ad infinitum*. Whence its general truth follows. The most important steps of the proof I subjoin.

Assigning to z in the $n-1$ first equations of the system (14) any particular value, those equations degenerate into a system of equations among the $n-1$ variables x, y, &c., from which, by assumption, a single positive set of values of those $n-1$ variables may be determined. These values substituted, together with the assumed value of z in the first member of the last equation of the system $\frac{\mathrm{V}_z}{\mathrm{V}}$, will give to that member a corresponding value t. In order, then, that the system (14) may admit of a solution in positive values of x, y, z, it suffices that the limits of the quantity t, as z varies through the whole extent of positive magnitude from 0 to ∞, should not transcend those of r. For if the limits of r be wider than those of t, then r will admit of values, which, while they satisfy the linear conditions before assigned, do not permit us to satisfy the system (14) by positive values of x, y, z.

Now as z varies from 0 to ∞, t will vary continuously from the highest of the inferior to the lowest of the superior limits of r. Of this proposition I am not prepared to offer at present a perfectly general proof, though I have no doubt that I possess all the elements of such a proof. I am content to state that I have met with no special case in which I have not been able to prove it, and by a general method; but that a difficulty, probably of notation, intervenes at present between this and the general proof. The following particular verification may easily be applied to all the cases in which $n=3$. I apply it to the system (15).

If we clear of fractions the two first equations of that system, and arrange the result with reference to x and y, and if at the same time we equate the first member of the last equation of that system to t, clear of fractions, and arrange in like manner, we shall have, writing for simplicity p', q', t' for $1-p$, $1-q$, $1-t$ respectively,

$$\begin{aligned}(ap'z+dp')xy+cp'zx-bpzy-ep&=0\\(aq'z+dq')xy-cqzx+bq'zy-eq&=0\\(at'z-dt)xy+ct'zx+bt'zy-et&=0.\end{aligned}$$

The elimination of x and y from this system will conduct to a final equation between z and t, the solution of which will determine the value of t when z is given. Hence it will determine the value of the first member of the final equation of the system (15) when a particular value is assigned to z, while the two first equations of the system are satisfied.

Effecting, then, the required elimination, we obtain a result which may be thus written,

$$AD-BC=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

the functions A, B, C, D being eliminants* having the following expressions:

$$A=\left\{\begin{array}{ccc}ap'z+dp', & cp'z, & -bpz\\ aq'z+dq', & -cqz, & bq'z\\ at'z-dt, & ct'z, & bt'z\end{array}\right\},$$

$$D=\left\{\begin{array}{ccc}cp'z, & -bpz, & -ep\\ -cqz, & bq'z, & -eq\\ ct'z, & bt'z, & -et\end{array}\right\},$$

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* I use this term with Professor De Morgan in preference to the term "determinants." Mr. De Morgan's reasons, which appear to me conclusive, will be found in a note to a very able memoir 'On some points in the Theory of Differential Equations,' in a recent Number of the Cambridge Philosophical Transactions.

$$B=\left\{\begin{matrix} -bpz, & -ep, & ap'z+dp' \\ bq'z, & -eq, & aq'z+dq' \\ bt'z, & -et, & at'z-dt \end{matrix}\right\},$$

$$C=\left\{\begin{matrix} -ep, & ap'z+dp', & cp'z \\ -eq, & aq'z+dq', & -cqz \\ -et, & at'z-dt, & ct'z \end{matrix}\right\}.$$

From the consideration of these forms, we see that (18) may be expressed in the form

$$Pz^5+Qz^4+Rz^3+Sz^2=0, \quad . \quad . \quad . \quad . \quad (19)$$

P, Q, R, and S not involving z, but being functions of t and the constants of the original system (15).

The limiting forms of the above equation, as t approaches the respective limits 0 and infinity, are evidently

$$S=0, \quad P=0;$$

the former of these gives, on constructing the expression for S by means of (18) and of the expressions for A, B, C, D,

$$\left\{\begin{matrix} -bp, & -ep, & dp' \\ bq', & -eq, & dq' \\ bt', & -et, & -dt \end{matrix}\right\} \times \left\{\begin{matrix} -ep, & dp', & cp' \\ -eq, & dq', & -cq \\ -et, & -dt, & ct' \end{matrix}\right\}=0;$$

whence, developing and dividing by the constant factors *bed* and *edc*, we get

$$(p+t-q)(q+t-p)=0,$$

$$\therefore\ t=p-q \text{ or } q-p. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

In precisely the same way the equation P=0 gives for the values of t corresponding to $z=\infty$, the relations

$$\left\{\begin{matrix} ap', & cp', & -bp \\ aq', & -cq, & bq' \\ at', & ct', & bt' \end{matrix}\right\} \times \left\{\begin{matrix} cp', & -bp, & -ep \\ -cq, & bq', & -eq \\ ct', & bt', & -et \end{matrix}\right\}=0;$$

which on development and division by the constant factors *abc* and *cbe*, gives

$$(1-t)(p+q-t)=0,$$

whence

$$t=1 \text{ or } p+q. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

But the values (20) and (21) thus determined for the first member of the final equation of the system (15), when z assumes the respective values 0 and infinity, are precisely the values before determined for the limits of r.

Further, it appears, as was to be shown, that if we assume x, y, z to be *positive* quantities satisfying the two first equations of the system (15), the first member of the final equation will, when $z=0$, coincide with the highest of the inferior limits of r above determined, and when $z=\infty$ with the lowest of the superior limits; for the highest inferior and the lowest superior limit are the true limits of r, as above determined, on the assumption that x, y, z are positive quantities. And therefore they are the true limits of the first member of the final equations under the same conditions.

Thus it has been shown that as z varies from 0 to infinity, while x and y are positive quantities constantly determined so as to satisfy the two first equations of the system (15), the first member of the final equation of (15) will vary from the highest of the inferior to the lowest of the superior limits of r. In the variation it must once, or more often, coincide with the actual value of r. But that it cannot more often than once coincide with r is evident from the constitution of H_n (Prop. I.), which does not permit that the function in question should ever resume a former value while z continues to increase. For if such resumption were possible, a maximum or minimum value of the function would intervene; and this, as H_n continues to consist of positive terms only, cannot happen. One solution therefore exists, and one only, of the system (15) in positive values of $x, y, \ldots z$ when the linear conditions of inequality among the quantities $p, q, \ldots r$ are satisfied, and then only.

The reasoning is general, and serves to establish the general truth of the proposition.

Proposition III.

The conditions which must be satisfied in order that the final algebraic system of equations

$$\frac{V_s}{p}=\frac{V_t}{q}\ldots=V, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (22)$$

to which, by a general method, the solution of questions in the theory of probabilities is reduced (Laws of Thought, p. 270, Phil. Mag. December 1854), *admits of a single solution or positive fractional values of s, t, &c., are identical with the conditions of possible experience in the data of the problem.*

The function V in this proposition is the sum of a series of terms which we may by analogy term algebraic constituents. If the number of variables s, t, &c. be two, the possible algebraic constituents are

$$st,\ s(1-t),\ (1-s)t,\ (1-s)(1-t),$$

and V consists of some or the whole of these. V_s is the sum of those constituents of which s is a factor, and so on for the others. The algebraic system (22) is derived from a logical equation in which st, $s(1-t)$, &c. occur as the constituents, properly so called, of a logical development, having therein a logical, and not a quantitative interpretation. That the conditions of possible experience may be deduced from such a development has been shown in my last paper on the Theory of Probabilities, published in this Journal (Dec. 1854, *note*). It is most convenient, indeed, to deduce them from the immediate data. This is the course which I have adopted in my paper 'On the Conditions, &c.' (Phil. Mag. August 1854). But they may also be deduced from the final logical development; and the results are *necessarily* equivalent, inasmuch as the final development and the initial data are connected by a chain of analysis founded upon those laws of thought which are the basis of ordinary deductive reasoning.

And hence it will suffice to show, that the required conditions of algebraic solution of the system (22) are the same as the conditions of possible experience determined from the logical development from which the algebraic system is derived.

To determine the conditions of algebraic solution, let us first place the system in the form

$$\frac{V_s}{V}=p, \quad \frac{V_t}{V}=q, \text{ \&c.},$$

and then assuming

$$\frac{s}{1-s}=x, \quad \frac{t}{1-t}=y, \text{ \&c.},$$

we obtain a result of the form

$$\frac{V_x}{V}=p, \quad \frac{V_y}{V}=q, \text{ \&c.},$$

agreeing with the form considered in Prop. II.

Thus, if we have

$$V=stu+st(1-u)+s(1-t)u+(1-s)tu+(1-s)(1-t)(1-u), \quad (23)$$

in which case the system (22) becomes

$$\left.\begin{aligned}\frac{stu+st(1-u)+s(1-t)u}{V}&=p\\ \frac{stu+st(1-u)+(1-s)tu}{V}&=q\\ \frac{stu+st(1-t)u+(1-s)tu}{V}&=r\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (24)$$

And if we therein make

$$\frac{s}{1-s}=x, \quad \frac{t}{1-t}=y, \quad \frac{u}{1-u}=z,$$

we obtain a system which agrees with (15), the constants a, b, c, d being made severally equal to 1.

The conditions which must be fulfilled in order that the transformed system may have a single solution in positive values of x, y, z, depend, as it has been seen, on the analysis of (16), (17). Let us then seek what would be the conditions of possible experience in a problem, in the solution of which we should be conducted by the general method in probabilities to the algebraic system (24).

That system must arise from a logical equation

$$\mathrm{V}=1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

and a connected system of data,

$$\text{Prob. } s=p, \quad \text{Prob. } t=q, \text{ \&c.},$$

s, t, &c. being logical symbols. Moreover, V as a logical function is identical in *form* with V as an algebraic function (23). Now by virtue of (25) all constituents vanish except those which are found in V. Hence

$$\left.\begin{array}{l}\text{Prob. } s=\text{ Prob. }\{stu+st(1-u)+s(1-t)u\}=p\\ \text{Prob. } t=\text{ Prob. }\{stu+st(1-u)+(1-s)tu\}=q\end{array}\right\}. \quad (26)$$

Moreover, as $\mathrm{V}=1$ denotes, as a logical equation, the *certainty* of the event V, we shall have

$$\text{Prob. } \mathrm{V}=1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

Hence if we make

$$\text{Prob. } stu=\lambda, \quad \text{Prob. }(1-s)tu=\mu, \quad \text{Prob. } s(1-t)u=\nu,$$
$$\text{Prob. } st(1-u)=\rho, \quad \text{Prob. } (1-s)(1-t)(1-u)=\sigma,$$

we shall have from (26) and (27)

$$\begin{array}{l}\lambda+\nu+\rho=p\\ \lambda+\mu+\rho=q\\ \lambda+\mu+\nu=r\\ \lambda+\mu+\nu+\rho+\sigma=1\,;\end{array}$$

to which, λ, μ, ν, &c. being *probabilities*, we may add

$$\lambda \overset{=}{>} 0, \quad \mu \overset{=}{>} 0, \quad \nu \overset{=}{>} 0, \quad \rho \overset{=}{>} 0, \quad \sigma \overset{=}{>} 0.$$

This combined system is, moreover, identical with the combined system (16), (17); and the mode of elimination being the same, it will furnish the same final relations among p, q, r, for the conditions of possible experience, as are furnished by (16), (17) for the conditions of algebraic solution of (15) in *positive*, and as we have afterwards shown, of (24) in *positive fractional* values of the variables involved.

Now there is nothing special in this reasoning. It is equally applicable to every form of V, and establishes the identity of the conditions of possible experience in the data of a question of probabilities, and those of mathematical consistency in the final processes by which the question is solved.

Other remarkable consequences, to which I shall but slightly advert, flow from these investigations.

Thus it follows from them that the numerical value of any probability determined by the general method will always, provided that the problem be a *real* one, satisfy those necessary and sufficient conditions of limitation, the mode of determining which I have illustrated in the August Number of this Journal (1854).

It follows hence, that two modes of procedure are open to us for the limiting of the solutions of questions of probability. We can either determine simultaneously the conditions of possible experience and those of final limitation, by the special method for this purpose developed in this Journal (August 1854), or, dispensing with this preliminary inquiry, we can so order the process of solution by the general method as to cause the different sets of values of the quantities *s*, *t*, &c. to be distinctly evolved. Then, if among those sets there be found one that is wholly positive, the conditions of possible experience will be implicitly satisfied, and the final value of the probability sought will itself also satisfy the conditions of limitation to which the preliminary inquiry would have conducted us. It is obvious that the former is the easier mode of procedure.

I would beg, in conclusion, to observe that I have in the analytical portion of this paper aimed at little more than to give an account of researches not yet quite finished. The general proof of the properties of the functions K and $\frac{V_s}{V}$, I hope at no distant time to be able to complete and lay before the public. In the meanwhile I do not desire that they should be received with any greater confidence than the verifications actually furnished may seem to warrant. Of the function K it seems not improbable that other and important applications may yet be made. The conclusions to which these researches point is this,—that the theory which they are designed to test is a rigorously consistent one; that the conditions of its mathematical validity are identical with the conditions of possible experience. Now I apprehend that this character is *peculiar* to the theory under consideration. Upon no *merely* mathematical basis could a doctrine possessing such capacity of verification *à posteriori* rest, because other elements than that of number are involved in the inquiry. Some attention must be paid to the philosophy of language; so much at least as is necessary in order to apprehend the distinction between whatsoever is transitory or accidental in its structure, and whatsoever belongs to its foundations in thought,—foundations not less deep, and not less solid, than those of geometry itself.

Queen's College, Cork.

XXVI. *Analysis of the Mineral "Edingtonite."*

By M. Forster Heddle, *M.D., F.R.P.S.*[*]

THE unsatisfactory nature of Dr. Turner's analysis of Edingtonite,—unsatisfactory as to the little insight it affords us into the composition of the mineral, on account of the large loss sustained,—has long made it desirable that a second analysis should be undertaken; the extreme rarity of the substance, however, has hitherto rendered this impossible.

Several years ago I was so fortunate as to obtain a specimen of this mineral, and by inducing the dealer from whom it was procured to revisit the locality, several others were found, and I was thus enabled to sacrifice one. I have little to add to the mineralogical characters already known.

The associated minerals are *analcime*, *harmotome* in twins, *calc-spar*, *rubin-glimmer*, *green-earth* in pseudomorphs after *calcite*, and *cluthalite*.

Specific gravity (determined on 130 grs.) 2·694.

Translucent to opake.

Colour—white, grayish-white, pink.

Powdered mineral absorbs 1·614 per cent. of moisture.

Occurs both crystallized[†] and massive[‡].

On finding that the crystals were invariably associated with *harmotome*, I was led to suspect the presence of baryta in this mineral, and my conjecture was verified by the qualitative analysis, which showed silica, baryta, alumina, water, and traces of lime and soda.

The quantitative analysis (made on 25 grs.) afforded,—

	Per cent.	Oxygen.		
Silica . . .	36·98	19·229	21·14	21
Alumina . .	22·63	10·555	11·62	12
Baryta . . .	26·84	2·724	3	3
Lime . . .	trace			
Soda . . .	trace			
Water . . .	12·46	11·077	12·12	12
	98·91			

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[*] Communicated by the Author.

[†] New figures, showing an additional face, will be given in the forthcoming "British Mineralogy" of Messrs. Greg and Lettsom.

[‡] One specimen in my possession, partly massive, weighs 2¼ oz.

which gives the formula—

$$3(BaO, Si\,O^3) + 4(Al^2\,O^3, Si\,O^3) + 12HO,$$

the calculated per-centages of which are,—

	Atoms.	Per cent.
Silica . . .	7	37·274
Alumina . .	4	23·751
Baryta . . .	3	26·514
Water . . .	12	12·461

In another analysis I got ·08 of strontia (?) and ·22 of lime.

For the sake of comparison, Dr. Turner's analysis is appended in a foot note*; it will be observed that the *lime* and *loss* taken together approximate the quantity of *baryta* obtained in the new analysis.

I would account for Dr. Turner's mistaking baryta for lime, and also so far for the loss sustained, by supposing that, from having merely a few chips of the mineral, he was unable to institute a preliminary qualitative analysis; and that he (under the impression that, like most zeolites, it would be found to contain lime) added oxalate of ammonia, and so got the partially soluble oxalate of baryta, which in the washing passed to a great extent through the filter, and if it was again noticed, was set down as "probably potash or soda." I would explain the mistake thus, because I believe, not only that Dr. Turner was a most correct analyst, but that he never willingly shirked any of the intricacies; at the same time a readier explanation might be offered in the fact, that at this time baryta was *often* taken for lime. Dr. Thomson, for instance, was detected by Connel mistaking baryta and strontia for lime in *Brewsterite*, and by Damour in a similar mistake with regard to "*Morvenite.*"

In virtue of its composition, as above stated, Edingtonite will take a most interesting place in the system, ranking with harmotome and Brewsterite as the third barytic zeolite; thus

Brewsterite $(BaO, SrO)\,SiO^3 + Al^2\,O^3, 3Si\,O^3 + 5HO.$
Edingtonite $3(BaO, Si\,O^3) + 4(Al^2\,O^3, Si\,O^3) + 12HO.$
Harmotome $3BaO, 2Si\,O^3 + 4(Al^2\,O^3, 2Si\,O^3) + 18HO.$

The original specimen of this mineral was stated to have occurred in cavities of *Thomsonite*. I have never seen it thus associated.

Before leaving *Edingtonite*, we would venture to throw out the

---

* By Turner:—

Silica	35·09
Alumina . . .	27·69
Lime	12·68
Water	13·32
Loss	11·22

hint that the "*Glottalite*" of Thomson, of which only one specimen has been found, and about which nothing is known, may turn out to be nothing but an impure mixture of *Edingtonite* and *harmotome* (which, as we have stated, invariably accompanies it); the locality and characters are much the same. Turner mistook lime for barytes, and why not Thomson, who had before made the same mistake. Granting this error, both contain the same elements; the quantity of silica, which no one could mistake, agrees, and a person not very conversant with crystallography might fancy he saw both octohedrons and cubes in the faces of Edingtonite, especially when the "crystals were so mixed together that only a small portion of each could be seen." We may also mention that "*Glottalite*" was procured from the same dealer from whom we purchased our *Edingtonites*.

XXVII. *On the Law of Absorption of Gases.* *By* R. Bunsen.

[Concluded from p. 130.]

9. *Oxygen in Water.*

OXYGEN gas, prepared in the usual manner from chlorate of potash, gave,—

No.	° C.	Coefficient.
1	6·0	0·04609
2	8·3	0·04186
3	11·6	0·03921
4	18·1	0·03715
5	22·8	0·03415

During the agitation in the absorptiometer the water became turbid from the formation of a black powder, and it was feared that the metals dissolved in the mercury had been oxidized at the expense of the oxygen in the water, and hence too large a coefficient obtained. The mercury employed was therefore purified from all foreign metals, as perfectly as several long digestions with concentrated nitric acid would permit. The experiment conducted with the purified mercury gave the following results:—

No.	° C.	Coefficient.	Difference.
1	19·4	0·03109	+0·00090
2	19·6	0·03199	+0·00003
3	19·4	0·03202	+0·00052
4	19·5	0·03254	−0·00009
5	19·5	0·03245	+0·00047
6	19·5	0·03292	+0·00221
7	19·0	0·03513	−0·00057
8	19·0	0·03456	

The experiment gave therefore, in fact, rather a smaller coefficient. In spite, however, of the most careful purification, the mercury always caused a black turbidity in the water, which perceptibly increased with agitation. This circumstance, and the fact that the coefficients determined one after the other in the same liquid and at the same temperature always regularly increased, showed that this method was not to be relied upon for exact results. I have preferred, therefore, to determine the coefficients by an indirect method.

If atmospheric air perfectly free from carbonic acid and ammonia be passed into boiled water, the amount of oxygen absorbed V_0, and the amount of absorbed nitrogen V, is found from the following equations formerly deduced:—

$$V_0=\frac{\alpha POV_1}{0{\cdot}76(N+O)}; \quad \ldots\ldots\ldots \quad (3)$$

$$V=\frac{\beta PNV_1}{0{\cdot}76(N+O)}. \quad \ldots\ldots\ldots \quad (4)$$

The first of these equations divided by the second gives $\frac{NV_0}{OV}\beta=\alpha$. As the composition of atmospheric air, *i. e.* the proportion $\frac{N}{O}$, as well as the absorption-coefficient β of nitrogen is known, we only require to determine the proportion $\frac{V_0}{V}$, or the composition of the air dissolved in the water, in order to calculate α, or the absorption-coefficients of oxygen. The elements for this calculation are found in the following experiments. The air, perfectly freed from carbonic acid and ammonia, was passed in a strong stream for half a day through the water, previously well boiled, which was kept at a constant temperature by immersion in a water-bath. The purification of the water must be conducted with the greatest care. It must not be distilled in any vessel previously used for any organic preparation, as the slightest trace of volatile organic matter present is sufficient to convert a part of the oxygen into carbonic acid. It is therefore necessary to prove the absence of carbonic acid in the air boiled out from the water by a special experiment. The gases dissolved in this water were collected by a method which I made use of in my researches upon the gases of the islandic springs, and more fully described by Dr. Baumert, in his admirable research upon the respiration of the *Cobitis fossilis*.

Air from Water saturated at 1° C.

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Volume of gas employed ...	216·85	0·2644	9·7	55·374
+ hydrogen	376·16	0·4170	9·8	151·414
After the explosion	289·83	0·3340	9·9	93·420

Oxygen	34·91
Nitrogen	65·09
	100·00

Air from Water saturated at 13° C.

	Volume.	Pressure[*].	° C.	Vol. at 0° and 1 m.
Volume of gas employed ...	165·99	0·2198	9·0	53·324
+ hydrogen	346·28	0·3914	9·5	130·994
After the explosion	288·78	0·3371	9·2	94·18

Oxygen	34·73
Nitrogen	65·27
	100·00

Air from Water saturated at 23° C.

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Volume of gas employed ...	206·67	0·2577	9·3	51·497
+ hydrogen	400·29	0·4437	9·2	171·828
After the explosion	323·95	0·3746	8·7	117·608

Oxygen	35·08
Nitrogen	64·92
	100·00

From these experiments, it is seen that the composition of the air dissolved in water at various temperatures is always constant. The mean composition is,—

Oxygen	$34·91 = V_0$
Nitrogen	$65·09 = V$
	100·00

The curve which represents the increase of the absorption-coefficient of oxygen for decreasing temperatures is therefore parallel to the corresponding curve for nitrogen.

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[*] In all statements of pressure in this research the correction for tension of the vapour of water is included.

If we take the following as the true composition of atmospheric air,—

Oxygen	0·2096=O
Nitrogen	0·7904=N
	1·0000

and substitute the values of V_1, V, O, and N in the former equation, we obtain the value of the absorption-coefficient of oxygen $\alpha=2{\cdot}0225\beta$, when β is the absorption-coefficient of nitrogen.

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·04114	0·00107	7	0·03465	0·00079	14	0·03034	0·00048
1	0·04007	0·00100	8	0·03389	0·00076	15	0·02989	0·00045
2	0·03907	0·00097	9	0·03317	0·00072	16	0·02949	0·00040
3	0·03810	0·00093	10	0·03250	0·00067	17	0·02914	0·00035
4	0·03717	0·00089	11	0·03189	0·00061	18	0·02884	0·00030
5	0·03628	0·00084	12	0·03133	0·00056	19	0·02858	0·00026
6	0·03544		13	0·03082	0·00051	20	0·02838	0·00020

10. *Atmospheric Air in Water.*

We have seen in the former part of the paper that the relative proportion in which the constituents of a mixture of gases are absorbed by water does not alone depend upon their several absorption-coefficients, but also upon the relative proportions in which they are mixed. If the absorption-coefficients are different, the gases dissolved in the water are not in the same relation as those in the free gas. This last undergoes therefore a change in its composition, which varies with the relation of the mass of the water to that of the gas. For this reason the absorption-coefficient of a mixed gas can only be calculated from the relative proportions of the constituents and their several absorption-coefficients, when the volume of the gas is so great in comparison with the mass of the absorbing liquid, that the alteration effected by the absorption in the composition of the residual gas is inappreciable. The true absorption-coefficient of air is therefore found only in these phænomena of absorption in which these conditions are fully carried out.

If we take, as above, for the mean atmospheric composition,—

Oxygen	0·2096=O
Nitrogen	0·7904=N
	1·0000

by substituting the value 1 for N+O, and for V and for P the value 0·76, and lastly, for N and O the above values, we obtain the required coefficient c for air and water from the following equation:—

$$V_0=\frac{\alpha POV_1}{0\cdot76(N+O)}, \text{ and } V=\frac{\beta PNV_1}{0\cdot76(N+O)},$$

which give

$$c=0\cdot2096\alpha+0\cdot7904\beta.$$

The following table is calculated by the aid of this formula:—

° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.	° C.	Coefficient.	Difference.
0	0·02471		7	0·02080	0·00048	14	0·01822	0·00029
1	0·02406	0·00065	8	0·02034	0·00046	15	0·01795	0·00027
2	0·02345	0·00061	9	0·01992	0·00042	16	0·01771	0·00024
3	0·02287	0·00058	10	0·01953	0·00039	17	0·01750	0·00021
4	0·02232	0·00055	11	0·01916	0·00037	18	0·01732	0·00018
5	0·02179	0·00053	12	0·01882	0·00034	19	0·01717	0·00015
6	0·02128	0·00051	13	0·01851	0·00031	20	0·01704	0·00013

Fig. 5 (Plate I.) gives a graphic representation of the solubility in water of some of the gases examined.

The following absorption-coefficients of chlorine, sulphurous acid, and sulphuretted hydrogen, were not determined in the absorptiometer, but by chemical methods, which will be fully explained by Dr. Schönfield in his research upon this subject.

° C.	SO_2.	SH.	Cl.	° C.	SO_2.	SH.	Cl.
0	68·861	4·3706		21	34·986	2·8430	2·1148
1	67·003	4·2874		22	33·910	2·7817	2·0734
2	65·169	4·2053		23	32·847	2·7215	2·0322
3	63·360	4·1243		24	31·800	2·6623	1·9912
4	61·576	4·0442		25	30·766	2·6041	1·9504
5	59·816	3·9652		26	29·748	2·5470	1·9099
6	58·080	3·8872		27	28·744	2·4909	1·8695
7	56·369	3·8103		28	27·754	2·4357	1·8295
8	54·683	3·7345		29	26·780	2·3819	1·7895
9	53·021	3·6596		30	25·819	2·3290	1·7499
10	51·383	3·5858	2·5852	31	24·873	2·2771	1·7104
11	49·770	3·5132	2·5413	32	23·942	2·2262	1·6712
12	48·182	3·4415	2·4977	33	23·025	2·1764	1·6322
13	46·618	3·3708	2·4543	34	22·122	2·1277	1·5934
14	45·079	3·3012	2·4111	35	21·234	2·0799	1·5550
15	43·564	3·2326	2·3681	36	20·361	2·0332	1·5166
16	42·073	3·1651	2·3253	37	19·502	1·9876	1·4785
17	40·608	3·0986	2·2828	38	18·658	1·9430	1·4406
18	39·165	3·0331	2·2405	39	17·827	1·8994	1·4029
19	37·749	2·9687	2·1984	40	17·013	1·8569	1·3655
20	36·216	2·9053	2·1565				

The coefficients of absorption of a number of gases having been thus obtained, we are enabled to verify experimentally the formulæ which were deduced from Dalton's hypothesis.

If a volume of a simple gas, whose absorption-coefficient is α, be twice absorbed from the same volume h of water, at the same temperature, but under different pressures P and P_1, the amount of gas absorbed in the two cases is, according to formula (2),—

$$g=\frac{\alpha P h}{0{\cdot}76}, \text{ and } g_1=\frac{\alpha P_1 h}{0{\cdot}76}.$$

Hence follows

$$\frac{g}{g_1}=\frac{P}{P_1}.$$

The following determinations made with the absorptiometer show, within the limit of the experimental errors, that the amount of carbonic acid g_n absorbed in the same volume of water at the same temperature increases proportionally to the corresponding pressures P_n.

Carbonic Acid at 19°·9 C.

No.	P.	g.	$\frac{P}{P_1}$.	$\frac{g}{g_1}$.
1	0·7255	38·61		
2	0·5245	27·24	1·38	1·42
3	0·5237	27·08	1·39	1·43
4	0·5231	27·28	1·39	1·42

Carbonic Acid at 3°·2 C.

No.	P_n.	${}_n$.	$\frac{P}{P_1}$.	$\frac{g}{g_1}$.
1	0·5244	31·41		
2	0·6467	38·66	0·8109	0·8125
3	0·6470	38·49	0·8105	0·8161

This ratio between the absorbed gaseous volume and the pressure is more clearly seen in cases in which the partial pressures occur, *i. e.* in which alterations in the pressure are effected by dilution with another gas. The formula (14) gives a convenient statement of this relation,

$$\frac{x}{x+y}=\frac{W-B}{A-B}\cdot\frac{A}{W}.$$

By means of this formula the composition of a mixture of two gases can be calculated when the following quantities are given: α_1 the absorption-coefficient of the first gas, β_1 that of the second; V the common volume of both gases before the absorption under the pressure P; V_1 the residual volume after the absorption under the pressure P_1; and lastly, the volume h of the absorbing water. If the composition of the mixture calculated from these experimental elements coincides with that found directly by eudiometrical analysis, it may be concluded that the formulæ based upon the original premises are true, and that the law is not only applicable for total, but also for partial pressures.

Mixtures of carbonic acid and hydrogen were employed in the following experiments:—

Experiment I.

Eudiometric determination.

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Hydrogen	120·6	0·7214	13·6	82·87
After addition of carbonic acid.	129·4	0·7269	13·5	89·63

Composition of the gas in 100 parts:—

Hydrogen	92·46
Carbonic acid . . .	7·54
	100·00

Absorptiometric Determination of the same Gas.

	Volume.	Pressure.	° C.	Vol. at 0°.
Gas employed...........	180·94	0·5368	15·4	171·29
After absorption.........	122·01	0·6809	5·5	119·61

Volume of absorbing water .	=356·7
	356·1
Mean	356·4.

From this the following elements of the calculation are obtained:—

$P=0·05368$; $P_1=0·6809$; $\alpha=1·4199$; $h=356·4$;
$V=171·29$; $V_1=119·61$; $\beta=0·0193$;

and hence the composition,—

	Absorptiometric.	Eudiometric.
Hydrogen . .	0·9207	$0·9246=v_1$
Carbonic acid .	0·0793	$0·0754=v$
	1·0000	1·0000

By the help of the formulæ,

$$u=\frac{x}{x+y}=\frac{AB_1}{AB_1+A_1B}, \quad u_1=\frac{y}{x+y}=\frac{A_1B}{AB_1+A_1B},$$

the composition of the gas remaining after absorption over the water is obtained.

Hydrogen . . .	0·9829
Carbonic acid . .	0·0171
	1·0000

As the gaseous mixture after absorption was subjected to a pressure $P_1=0^m·6809$, the partial pressure upon the carbonic acid was in this experiment $uP_1=0^m·0116$, that of the hydrogen $u_1P_1=0^m·6692$.

Experiment II.

Eudiometric determination.

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Hydrogen	62·2	0·6449	7·5	39·03
After addition of carbonic acid.	82·1	0·6649	6·4	53·34

Composition of the gas:—

Hydrogen . . .	0·7319
Carbonic acid . .	0·2681
	1·0000

Absorptiometric Analysis of the same Gas.

	Vol. at 0°.	Pressure.	° C.
Gas employed............	119·03	0·4951	6·8
First absorption	72·02	0·6116	5·1
Second absorption	60·39	0·7297	12·8
Third absorption	75·71	0·6020	23·3

Volume of absorbing water:—

	206·83
	206·61
	206·61
	207·11
Mean . . .	206·79

Hence follows for the first absorption,—

$P=0·4951$; $P_1=0·6116$; $\alpha=1·4434$; $h=206·79$;
$V=119·03$; $V_1=72·02$; $\beta=0·0193$.

For the second absorption:—

$P=0·4951$; $P_1=0·7297$; $\alpha=1·0726$; $h=206·79$;
$V=119·03$; $V_1=60·39$; $\beta=0·0193$.

For the third absorption:—

$P=0·4951$; $P_1=0·6020$; $\alpha=0·8555$; $h=206·79$;
$V=119·03$; $V_1=75·71$; $\beta=0·0193$.

The calculation of the first absorption gives,—

	Absorptiometric.	Eudiometric.
Hydrogen . . .	0·7343	0·7319
Carbonic acid . .	0·2657	0·2681
	1·0000	1·0000

The composition of the residual gas remaining after absorption, calculated from the eudiometric analysis, is,—

Carbonic acid . . .	0·0699
Hydrogen	0·9301
	1·0000

Hence the partial pressure of the hydrogen is 0$^{\mathrm{m}}$·5688, and of the carbonic acid 0$^{\mathrm{m}}$·04275.

From the second absorption experiment we obtain,—

	Absorptiometric.	Eudiometric.
Hydrogen	0·7372	0·7319
Carbonic acid . . .	0·2628	0·2681
	1·0000	1·0000

Also the residual gas after absorption :—

Carbonic acid	0·07712
Hydrogen	0·92288
	1·00000

Hence follow the partial pressures under which the absorption took place, for the carbonic acid 0$^{\mathrm{m}}$·0563, for the hydrogen 0$^{\mathrm{m}}$·6734. The third absorption gives the following composition :—

	Absorptiometric.	Eudiometric.
Hydrogen	0·7285	0·7319
Carbonic acid . . .	0·2715	0·2681
	1·0000	1·0000

This gives a residual gas of the composition,—

Carbonic acid	0·1036
Hydrogen	0·8964
	1·0000

The pressure of the carbonic acid is here 0$^{\mathrm{m}}$·06236, and of the hydrogen 0$^{\mathrm{m}}$·5396.

The mean from these three determinations, compared with the eudiometric analysis, gives,—

	Absorptiometric analysis.	Eudiometric analysis.
Hydrogen	26·67	26·81
Carbonic acid . . .	73·33	73·19
	100·00	100·00

How far beyond the examined limits the law may be considered as true cannot be *à priori* determined. It is, however, more than probable that in this law, as in the law of Mariotte, a limit exists beyond which the regularity of the action is disturbed by varying molecular influences. The limits of the exact action of the law are however quite extensive enough to enable us to draw some very interesting conclusions from the subject. Eudiometry, for example, gains from the law of absorption an entirely new sphere of action, enabling it not only to determine, without any chemical experiments, the simple or complex constitution of a gas, but also to recognize the nature of the component parts, even indeed to estimate their several proportions, when once for all the absorption-coefficients of the gases are known. To show that such an absorptiometric determination can serve as a reaction for the detection of gases, I chose an experiment with marsh gas, which satisfactorily proves that results are attainable even when the values of the absorption-coefficients employed in the calculation differ but little from each other.

Relying on the results of eudiometrical analysis, it has been hitherto supposed that the gas obtained by the action of a hydrated alkali upon an alkaline acetate at a high temperature was marsh gas. Although this supposition has scarcely ever been questioned, still all positive proof of the fact fails. Frankland and Kolbe have shown that two volumes of marsh gas by eudiometrical explosion react exactly as a mixture of equal volumes of hydrogen and methyle. Both give for every volume a volume of carbonic acid, and require for their combustion the double amount of oxygen. Eudiometric analysis leaves it then undecided whether the gas evolved from the alkaline acetates is to be considered as marsh gas, or as a mixture of methyle and hydrogen. By means of absorptiometric analysis this question is very readily and surely answered. If we start from the supposition that the gas in question is a mixture of equal volumes of methyle and hydrogen, a volume V of the gas, at 0° and under 0·76 pressure, measured in the absorptiometer would consist under the pressure 0·76 of,—

$$\frac{PV}{2\times0{\cdot}76}\text{ methyle, and }\frac{PV}{2\times0{\cdot}76}\text{ hydrogen.}$$

If this gas be agitated with h_1 volumes of water, the observed volume of the residual unabsorbed gas being V_1 under the pressure P_1, the sum of the absorption-coefficients at the temperature of absorption (for hydrogen α_1 and for methyle β_1) can be calculated from the observations. If we call the residual hydrogen x_1, and the residual methyle y_1 (both reduced to 0° and 0·76), this x_1 will, in consequence of its dilution with methyle, be subject to the partial pressure $\frac{x_1 0{\cdot}76}{V_1}$. It is however absorbed under this pressure by the volume h_1 of water. The absorbed volume of hydrogen, reduced to 0·76 pressure and 0° C., is therefore, according to the law of absorption, $\frac{x_1}{V_1}\, \alpha_1,\, h_1$. This absorbed hydrogen + unabsorbed x_1 is equal to the hydrogen originally present, namely,—

$$\frac{PV}{2\times 0{\cdot}76}=x_1\left(1+\frac{\alpha_1 h_1}{V_1}\right) \text{ or } x_1=\frac{PV}{2\times 0{\cdot}76\left(1+\frac{\alpha_1 h_1}{V_1}\right)}.$$

If the value of x_1 is substituted in the expression $\frac{0{\cdot}76 x_1}{V_1}$, we obtain for the pressure of the hydrogen in the residual gas $\frac{PV}{2(V_1+\alpha_1 h_1)}$, and for the pressure of the methyle by similar considerations $\frac{PV}{2(V_1+\beta_1 h_1)}$.

It follows, however, from the law of absorption, that the sum of the two pressures is equal to the observed pressure P_1. We have then,—

$$P_1=\frac{PV}{2(V_1+\alpha_1 h_1)}+\frac{PV}{2(V_1+\beta_1 h_1)},$$

or,

$$\alpha_1+\beta_1=\frac{PV}{2P_1 h_1}-\frac{2V_1}{h_1}. \quad . \quad . \quad . \quad . \quad (19)$$

An experiment made by Dr. Pauli with a gas prepared by heating acetate of potash with hydrate of potash, carefully freed from elayle and carbonic acid with fuming sulphuric acid and potash, gave the following elements for calculation:—

Original volume of gas reduced to 0°	$V = 116{\cdot}42$
The pressure on this volume	$P = 0{\cdot}5065$
Volume of gas employed reduced to 0° after first absorption	$V_1 = 75{\cdot}18$
Corresponding pressure	$P_1 = 0{\cdot}6615$
Volume of absorbing water	$h_1 = 318{\cdot}11$
Temperature of the experiment	12°·8 C.
Absorption-coefficient of hydrogen at 12°·8	$\alpha_1 = 0{\cdot}01930$
Absorption-coefficient of methyle gas at 12°·8	$\beta_1 = 0{\cdot}05446$
Absorption-coefficient of marsh gas at 12°·8	$\gamma_1 = 0{\cdot}04106$
The volume after second absorption reduced to 0°	$V_n = 79{\cdot}04$
Corresponding pressure	$P_n = 0{\cdot}6561$
Volume of absorbing water	$h_n = 325{\cdot}05$
Temperature of experiment	$t_n = 24°{\cdot}6$
Absorption-coefficient of hydrogen at 24°·6	$\alpha_n = 0{\cdot}01930$
Absorption-coefficient of methyle at 24°·6	$\beta_n = 0{\cdot}04181$
Absorption-coefficient of marsh gas at 24°·6	$\gamma_n = 0{\cdot}03166$

When this is calculated, negative values for $\alpha_1+\beta_1$ and for $\alpha_n+\beta_n$ are found from both absorption experiments, namely, $-0{\cdot}3325$ and $-0{\cdot}34807$, instead of the sums of the coefficients found in the tables for methyle and hydrogen, $+0{\cdot}07376$ and $+0{\cdot}06111$. Hence the gas in question cannot consist of equal volumes of hydrogen and methyle.

If, on the contrary, the same elements are used in the calculation of γ_1 and γ_n, under the supposition that the gas is a simple one, two absorption-coefficients are obtained, which are almost exactly the same as those found in the table for marsh gas, at the temperatures 12°·8 C. and 24°·6 C. The formula

$$\frac{\mathrm{VP}}{h_1\mathrm{P}_1}-\frac{\mathrm{V}_1}{h_1}=\gamma_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

gives for the absorption at 12°·8 $\gamma_1=0{\cdot}0439$ instead of the actual value 0·0411, and for the absorption at 24°·6 $\gamma_n=0{\cdot}0333$ instead of 0·03166. From this agreement, we may conclude that the marsh gas prepared from acetate of potash is neither a mixture of hydrogen and methyle, nor a substance isomeric with natural marsh gas, but that it is actually the same substance which streams from the mud-volcanoes of Bulganak in the Crimea.

Any general reaction to distinguish between the constituents of a gaseous mixture has hitherto been wanting. The quantitative composition of a gas obtained by eudiometrical analysis, depends almost entirely upon certain suppositions concerning its qualitative constitution. If, for instance, eudiometrical analysis points out the presence of marsh gas, it remains quite undecided, as I have just shown, whether this gas is a mixture of equal volumes of methyle and hydrogen. If analysis shows the presence of a mixture of marsh gas and hydrogen, it is uncertain whether we are not experimenting upon mixtures of methyle and hydrogen, or of methyle, marsh gas, and hydrogen. All analyses in which the two latter gases occur together may be calculated according to either of these assumptions, without it having been hitherto possible to prove the accuracy of either one.

It is easy, by means of the law of absorption, to remove these doubts, for the absorption-coefficients serve as the reagents which fail in gas analysis, and they present also the peculiarity, that they not only show the qualitative, but at the same time the quantitative composition of the gas. Let us, for example, suppose that an unknown gas be mixed in an unknown volume x, with an unknown volume y of another unknown gas, we can then, by means of three absorptiometric experiments, determine,—1st, what gases are present in the mixture, 2nd, in what proportions they occur.

The following is the method of solving this problem:—A sufficient quantity of the gas to be examined is collected in the absorptiometer, and its volume, pressure and temperature observed.

If the originally observed volume reduced to 0° be called V, and its pressure P, we obtain the equation

$$1=\frac{x}{\mathrm{VP}}+\frac{y}{\mathrm{VP}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Three absorptions of the gas are made with the volumes of water h_1, h_2 and h_3, and the corresponding volumes and pressures reduced to 0°, V_1P_1, V_2P_2, V_3P_3, observed for a constant temperature t. From these determinations, according to equation (6), we obtain the following, in which α signifies the absorption-coefficient of the first gas, and β that of the second at the temperature t:—

$$1=\frac{x}{(V_1+\alpha h_1)P_1}+\frac{y}{(V_1+\beta h_1)P_1},$$

$$1=\frac{x}{(V_2+\alpha h_2)P_2}+\frac{y}{(V_2+\beta h_2)P_2},$$

$$1=\frac{x}{(V_3+\alpha h_3)P_3}+\frac{y}{(V_3+\beta h_3)P_3}.$$

From these four equations the unknown quantities x, y, α, and β are easily found. The two last are the ordinates of absorption of two gases for the temperature-abscissa t. If the numerical values of these are calculated, it is easy to find in the table the gas which has the same absorption-coefficient for corresponding temperatures, and thus the nature of the mixture is determined. The values of x and y give also the relative proportion between the constituents. The determination of α and β is, in the case of two gases, not difficult. If we place $PV=a$, $P_1V_1=a_1$, $P_2V_2=a_2$, $P_3V_3=a_3$ and $P_1h_1=b^1$, $P_2h_2=b_2$, $P_3h_3=b_3$, we obtain first,—

$$\alpha+\beta=\frac{a_1b_2b_3(a-a_1)(b_2-b_3)-a_2b_1b_3(a-a_2)(b_1-b_3)+a_3b_1b_2(a-a_3)(b_1-b_2)}{b_1b_2b_3[a_1(b_2-b_3)-a_2(b_1-b_3)+a_3(b_1-b_2)]}$$

$$\alpha\beta=\frac{a_2b_3(a-a_2)-a_3b_2(a-a_3)-b_2b_3(a_2-a_3)\mathrm{A}}{b_2b_3(b_2-b_3)},$$

and when the expressions on the right of these equations be represented by A and B,—

$$\alpha+\beta=\mathrm{A}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

$$\alpha-\beta=\pm\sqrt{\mathrm{A}^2-4\mathrm{B}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

The sum of these two equations gives the value of α, their difference that of β.

By help of these values α and β, we obtain, lastly,—

$$y=\frac{(a_2-a+\alpha b_2)(a_2+\beta b_2)}{b_2(\alpha-\beta)}. \quad . \quad . \quad . \quad . \quad . \quad . \tag{22}$$

$$x=a-y. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \tag{23}$$

As an example of such a calculation, I select the qualitative and quantitative determination of the gas prepared by heating oxalic acid with concentrated sulphuric acid. As this gas always contains small quantities of sulphurous acid mixed with it, it was first passed through water in which binoxide of manganese was suspended, and the gas collected after the water had been saturated, and the atmospheric air contained in the apparatus displaced. A eudiometric analysis of the purified gas gave,—

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Original volume......................	142·9	0·6965	20·2	92·70
After absorption of carbonic acid	74·6	0·6637	19·0	46·29

	Found.	Calculated.
Carbonic oxide	50·06	50·00
Carbonic acid	49·94	50·00
	100·00	100·00

The absorptiometric determination, which was conducted in such a manner that the amount of absorbing water was increased after every observation, gave the following element:—

	Vol. at 0°.	Pressure.	° C.	Volume of water—h.
Gas employed	500·8	0·5760	19·0	0
After the first absorption	384·0	0·6882	19·0	81·6
After the second absorption.......	340·0	0·7015	19·2	186·9
After the third absorption	283·3	0·7415	19·0	335·5

From these elements, the value of β is found by means of formulæ (20) and (21) to be 0·9248. The tables of absorption show that carbonic acid gas has for the observed temperature a corresponding absorption-coefficient; for the table gives for this gas the number 0·9150, which closely approximates to the experimental coefficient β. We conclude from the constitution of a precipitate, from its colour, from its solubility, &c., that a certain substance is present. It is here not a precipitate, but a certain ordinate of a solubility-curve which serves as our test. But as there are certain precipitates produced by different substances resembling each other so closely that they cannot be used as distinguishing tests, it is possible that these ordinates may differ so little by the temperatures at which the curves of absorption approach, cut, or are tangent to each other, that a second absorptiometric experiment is necessary. The foregoing experiments, for example, give a value for α, at the temperature 19°, of 0·0204: this varies very slightly from the absorption-coefficient of carbonic oxide at 19°, which is found in the table to be 0·0233. The absorption-coefficient of hydrogen at 19° is, however, 0·0193, and that of æthyle gas at the same temperature is 0·0207, and these differ so slightly from the number found for carbonic oxide, that it must remain doubtful which gas is present. In this case the number derived from an experiment of absorption in water, is comparable to a reagent which indicates the presence of a group of substances. It only remains therefore to determine by further absorptiometric experiments, either at different temperatures or with other liquids, which of the gases indicated by the first experiment is really present. The determination of the absorption-coefficients of gases for alcohol, for solutions of salts and other liquids, forms therefore an important element in gas analysis, as from these any number of conditional equations may be obtained, each of which possesses the importance and value of a new reagent.

If the material nature of the gas has been determined by the method described by means of α and β, it is only necessary to substitute the values for α and β in the equations (22) and (23), in order to find the quantitative relation in which the two gases are mixed.

If this calculation is made for the above experiment with the values of α and β as found in the absorption tables for carbonic acid and carbonic oxide, we obtain—

	Eudiometrical.	Absorptiometrical.			
		1st Exp.	2nd Exp.	3rd Exp.	Mean.
Carbonic acid.........	50·06	50·00	50·03	50·34	50·12
Carbonic oxide	49·94	50·00	49·97	49·66	49·88
	100·00	100·00	100·00	100·00	100·00

The same elements which have served to determine the qualitative nature of the mixture of gases, give therefore the quantitative composition with a degree of exactness scarcely surpassed by eudiometric analysis.

Another problem which may be easily solved by means of the absorptiometer, concerns the alterations which a mixture of gases undergoes by contact with water. The following example of a similar mixture of carbonic acid and carbonic oxide shows how considerable such alterations, even with relatively small amounts of liquid, may become under certain circumstances, and to what considerable errors those eudiometric experiments may be subject in which the gases are confined over water, or liquids used as a means of absorption.

For this experiment, the mixture from oxalic acid and sulphuric acid was again employed. The elements for the calculation of formulæ (14) and (15), are taken from an experiment made with the absorptiometer by Dr. Atkinson; they are as follows:—

$$V=388{\cdot}4;\quad P=0{\cdot}6557;\quad h_1*=315{\cdot}3;\quad \alpha=0{\cdot}9124;$$
$$V_1=247{\cdot}69;\quad P_1=0{\cdot}7395;\quad \alpha_1\dagger=0{\cdot}02326.$$

The following composition of the gas employed is obtained by substituting these values in formula (15):—

	Absorptiometric.	Theory.
Carbonic acid . . .	49·55	50·00
Carbonic oxide . . .	50·45	50·00
	100·00	100·00

The absorptiometric analysis gives therefore again the values—

$$V=0{\cdot}4955 \text{ and } V_1=0{\cdot}5045.$$

From these is obtained, by means of formulæ (12) and (13), the following composition for the residual unabsorbed gas:—

Carbonic acid	31·87
Carbonic oxide	68·13
	100·00

A eudiometrical analysis of this residual gas, made by Dr. Atkinson, agrees sufficiently with this calculated composition. He found,—

	Volume.	Pressure.	° C.	Vol. at 0° and 1 m.
Original volume	96·1	0·6721	18·7	60·45
After absorption of carbonic acid	68·0	0·6556	19·3	41·64

Carbonic acid	31·12
Carbonic oxide	68·88
	100·00

The quantity of carbonic oxide contained in the mixture has therefore increased from 50·45 to 68·88, although the volume of the absorbing water was not so large as that of the gas.

The phænomena which accompany the evolution of gas in mineral springs, can only be fully understood by the help of the law of absorption. Among the non-alkaline springs, containing but a small quantity of dissolved salts, there are some whose absorption-coefficients differ but slightly from those of pure water, and contain carbonic acid gas alone in solution. If such springs, as is usually the case, are saturated with gas, a certain limit for the amount of contained carbonic acid may be found. This limit of the quantity of carbonic acid depends upon,—1st, the temperature of the spring; 2nd, the depth of the shaft of the spring; 3rd, the height of the spring above the sea. Springs of the above sort, which are saturated with a stream of chemically pure carbonic acid, and rise without pressure at the level of the sea, give according to their temperature very different amounts of gas. They contain in one litre of water the following amounts of gas for the corresponding temperatures:—

---

* Called h in formula (5).

† Called β in formula (5).

°C.	Cubic centimetres of gas in 1 litre of water.
0	1796·7
5	1449·7
10	1184·7
15	1002·0
20	901·4

If the same spring under otherwise similar circumstances rose at an elevation above the sea, where the average atmospheric pressure was only two-thirds of the mean height of the barometric column, it would contain only two-thirds of the above amount of dissolved carbonic acid.

Hence it will be perceived that the amount of gas in a spring which is saturated with pure carbonic acid, may be considerably augmented by deepening the spring shaft, and thus increasing the column of water under which the gas issues from the earth, as Bischoff has indeed already shown in his admirable researches on the phænomena of springs. If, for example, the depth of the shaft from the surface of the spring to the ground is 15 feet, the water where it bubbles out from the earth will contain one-third more carbonic acid than the above amounts show. The water in rising to the surface loses a part of the dissolved gas in proportion as the pressure diminishes, but the statical equilibrium which ensues in consequence of the law of absorption requires a certain time for its restoration. Thus the Peters spring in Petersthal in the Schwarzwald, which has a temperature of 10° C., contains at the surface of the spring, under a pressure $0^{m}\cdot735$, 1270·4 cubic centimetres of carbonic acid in the litre, whereas, according to the absorption-coefficient of carbonic acid for $10^{\circ}\cdot3$, it should only contain 1133·3 cubic centimetres under the same pressure. The water is therefore oversaturated with carbonic acid. This excess of gas is seen to escape in small bubbles from the water when a vessel filled at the spring is allowed to stand. By agitation the equilibrium is restored in a few moments, and the gas dissolved in the water reduced to its normal amount. By similar considerations, it is easy to see that many of the statements, with regard to the amount of carbonic acid contained in springs, must be false. Thus, for instance, the amount of carbonic acid contained in the "Fürsten Quelle" in Imnau, is given by Sigwart to be 2500 cubic centimetres in the litre. Under the mean pressure and at the temperature of the spring 6°·3 C., the water can, however, according to the law of absorption, only contain 1373·2* cubic centimetres after the equilibrium has been established. The amount of gas, 2500, requires a pressure of $1^{m}\cdot3836$ of mercury, or a column of water of $8^{m}\cdot449$, to be added to the mean barometric pressure. As, however, it is impossible to suppose that the Imnau spring rises under the pressure of a column of water at least 25 feet high, and as a saturation of nearly double the amount of gas is as improbable, we are compelled to assume that Sigwart's experiments are based upon error. The falsity of many other similar statements may thus be easily shown.

The relations which are found to exist between the free and absorbed gases of a spring by means of the law of absorption, give a fixed starting-point from which to estimate the influence which an amount of nitrogen in the free gas in a spring exerts upon the quantity of carbonic acid dissolved in the water. The second and third columns of the following table, calculated from the preceding formulæ, show the per-centage amount of carbonic acid and nitrogen in the absorbed gas for the corresponding percentages of nitrogen in the free gas given in the first column. The temperature of the water is supposed to be 15°·1 C.

Amount of nitrogen in the free gas.	Gas absorbed in the spring water.	
	Nitrogen.	Carbonic acid.
I.	II.	III.
10 per cent.	1·613	98·387
20 ,,	3·558	96·442
30 ,,	5·949	94·051
40 ,,	8·958	91·042
50 ,,	12·861	87·139
60 ,,	18·127	81·873
70 ,,	25·623	74·377
80 ,,	37·123	62·877
90 ,,	57·052	42·948

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* The small amount of solid constituents contained in the water (not more than 9 grains in a pound) cannot appreciably alter the absorption-coefficients, certainly not increase them.

From this table it is plainly seen, that if the gas passing through a spring at 15°·1 contains only 10 per cent. of carbonic acid with 90 per cent. of nitrogen, the gas dissolved in the spring water will contain 42·948 per cent. of carbonic acid. In this way it is easy, in analyses of mineral waters, to calculate the composition of the gases contained in the water, if the composition of the gas which is set free in the spring is known by experiment. If the composition of both gases is directly determined, and the experimental composition agrees with that found by calculation, we have a most valuable confirmation of the correctness of both analyses.

All these deductions from the law of absorption are of course only applicable to cases in which a statical equilibrium between the free and dissolved gases can ensue. This is not only the case in springs through which gases pass, but particularly in rain and dew, and for these is the law applicable with its greatest precision.

The amount of carbonic acid contained in the air varies, according to Saussure's numerous experiments, in 10,000 parts of air between 5·74 and 3·15, giving a mean of 4·15. According to this mean value the air is composed of—

Oxygen	20·9512
Nitrogen	79·0073
Carbonic acid . . .	0·0415
	100·0000

From this analysis, by means of equations (7) and (8), we obtain the following table, representing the mean constitution of the gases contained in rain water for the temperatures given in the upper line.

	0° C.	5° C.	10° C.	15° C.	20° C.
Nitrogen	63·20	63·35	63·49	63·62	63·69
Oxygen	33·88	33·97	34·05	34·12	34·17
Carbonic acid	2·92	2·68	2·46	2·26	2·14
	100·00	100·00	100·00	100·00	100·00

It is directly seen from this table that the amount of carbonic acid which the plants receive from the rain decreases for increasing temperatures.

The mean yearly amount of rain which falls on the surface of the earth is, according to Berghaus, equal to a layer of water of only $1^{m}\cdot5$ in height. By the help of formula (3) it is found, that with this stratum of water a volume of carbonic acid is conveyed to the earth which has a height of only 1·293 millimetre at 0° and 0·76 pressure; this gives for every square metre, or about 9 square feet of land, only 1·293 litre or 2·569 grms. of carbonic acid. For every temperature above 0° this amount of course decreases. The carbonic acid carried down with rain from the atmosphere is therefore so inconsiderable, that it appears to be quite inadequate for the nourishment of plants. Still it must not be forgotten that this amount of carbonic acid originally present in the rain-water, in proportion as it is absorbed by the plants, is again taken up from the passing currents of air by the moisture of the leaves and ground. Dew, rain, and even the juices of plants serve then, as means by which the nourishing substances of the atmosphere are transferred to the bodies of the plants. The supply of substances capable of assimilation is therefore regulated by the law of absorption; it varies with the temperature and barometric pressure. The volume of carbonic acid which is contained in the unit-volume of the sap of the plant and moistening surface of water at the mean barometric pressure, as obtained from formula (3), varies with the temperature in the following ratio:—

° C.	Volume.
0	0·000744
5	0·000600
10	0·000490
15	0·000415
20	0·000373

The plant therefore finds in a volume of the water with which it is moistened at 15°, the same amount of carbonic acid which a like volume of air contains; above this temperature, however, there is less, and below more.

It is scarcely possible to overlook in these phænomena an endeavour of nature to assist the tardy vegetation of the cold north by a richer nourishment, and by a sparing supply to keep the luxurious growth of the tropics within limits.

Similarly to carbonic acid, oxygen plays an important part in these phænomena, for under the influence of the decomposing organic constituents of the ground it is converted into carbonic acid. The mixture containing the greatest amount of carbonic acid which can thus be formed is—

Nitrogen	79·007
Carbonic acid	20·993
	100·000

If we suppose this mixture of gases to remain constant by continual additions from without, according to formula (3), the unit-volume of the moisture contained in the ground surrounding the roots will take up of carbonic acid,—

Temperature. ° C.	Volume.
at 0	0·00862
5	0·00760
10	0·00681
15	0·00626
20	0·00595

Hence it follows that the amount of carbonic acid which a cubic millimetre of the sap* from the leaves exposed to the air is able to absorb at 15° and $0^{m}\cdot76$ pressure, is never more than 0·000415 cubic millimetre, whilst the moisture in the loose earth around the roots may contain 0·00626 cubic millimetre, or nearly fifteen times as much. The great productiveness of mould rich in humus, and a part of the advantages which the agriculturist derives from breaking up his land, is easily explained from these phænomena.

The phænomena of the oceanic atmosphere, as well as of the absorption of air in the blood, which are deducible from the law of absorption, I reserve for my own future investigation.

It must also remain for future research to estimate again with all the exactitude necessary for normal determinations, the absorption-coefficients detailed in the present memoir, an absorptiometer of larger dimensions being made use of. When these experiments are carried out, it will be possible to determine the limits at which the law of absorption, similarly to that of Mariotte, begins to be disturbed by foreign influences.

In conclusion, my best thanks are due to Mr. Roscoe, who has kindly translated this memoir into English.

Heidelberg, February, 1855.

XXVIII. *The Theory of the Moon's Motion.—Explanation of the occurrence of non-periodic Factors in the Development of the Radius-vector.* *By* Professor CHALLIS†.

IN the Postscript to my communication to the Philosophical Magazine for February (p. 137), I have stated that the occurrence of terms containing factors which may increase indefinitely with the time in the development of the moon's radius-vector, is not owing to the circumstance that the approximation commences from a fixed ellipse, as has been generally admitted, but is wholly due to the mode of integration. I now propose to explain why one mode of integration rather than another leads to such terms, and to indicate processes of integration which are appropriate to the lunar problem.

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* Here the average amount of carbonic acid in the air is taken, and the absorption-coefficients of the sap considered the same as those of water.

† Communicated by the Author.

It will suffice for this purpose to suppose the force to be wholly central, and to be equal to $\mu u^2 - \frac{\mu'}{u}$, u being the reciprocal of the radius-vector. Thus we have the known equation,

$$\frac{d^2u}{d\theta^2} + u - \frac{\mu}{h^2} + \frac{\mu'}{h^2u^3} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Multiplying by $2\frac{du}{d\theta}$ and integrating,

$$\frac{du^2}{d\theta^2} + u^2 - \frac{2\mu u}{h^2} - \frac{\mu'}{h^2u^2} + C = 0.$$

Hence, making $\frac{du}{d\theta} = 0$, the equation for determining the apsidal distances is

$$u^4 - \frac{2\mu u^3}{h^2} + Cu^2 - \frac{\mu'}{h^2} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now, since C is positive for an elliptic orbit, or one approaching to an ellipse, if this equation contains two positive roots, it must, by the theory of equations, contain a third positive root, because the last term is negative. Hence there must be a *third* apsidal distance in addition to the two belonging to the approximate ellipse. The third apse is accounted for by considering that if the radius-vector be very large, and consequently u very small, the repulsive part of the force, viz. $-\frac{\mu'}{u}$, may exceed the attractive part μu^2, even when μ' is supposed to be small. Hence the analysis embraces a separate curve, containing infinite branches and one apsidal distance, in addition to the eccentric orbit. If, therefore, it be required to integrate the equation (1) on the supposition that the orbit is of small eccentricity and approximately elliptical, some method must be adopted which will exclude the third apsidal distance. One method proper for this purpose is that indicated by Mr. Airy (Mathematical Tracts, 3rd edition, Art. 44* of the Lunar Theory, p. 32), which consists in substituting $b+w$ for u, and neglecting powers of w above the first. This is virtually the same as the method employed in Pratt's Mechanical Philosophy (Art. 334, p. 300), where $b+\overline{u-b}$ is substituted for u, and powers of $u-b$ above the first are omitted. Making the latter substitution, the equation (1) becomes,

$$\frac{d^2u}{d\theta^2} + u\left(1 - \frac{3\mu'}{h^2b^4}\right) - \frac{\mu}{h^2} + \frac{4\mu'}{h^2b^3} = 0,$$

the integral of which is,

$$u=\frac{\mu}{h^2}-\frac{\mu' h^4}{\mu^3}+\text{A}\cos\left(\sqrt{1-\frac{3\mu' h^6}{\mu^4}}\,\theta+\text{B}\right),$$

neglecting powers of μ' above the first. It is plain that by supposing the arbitrary constant A to be small, we satisfy the condition that w shall be small *for all values of* θ, and consequently that the orbit shall be nearly circular. In this mode of integrating we have excluded the third apsidal distance, by giving to u the general form $b+w$, which admits of satisfying, by one of the arbitrary constants, the case of approximate circularity.

But when a particular form is given to u, the analysis will include the case of a third apsidal distance, so far as the form admits of satisfying the condition that $\frac{du}{d\theta}$ be a small quantity for values of the radius-vector contiguous to that apsidal distance. Thus let $u=b+be\cos(\theta+\gamma)$ nearly, the second term being supposed to be small compared with the other. Substituting this value of u in the last term of equation (1), and retaining only the first power of $e\cos(\theta+\gamma)$, we have

$$\frac{d^2u}{d\theta^2}+u-\frac{\mu}{h^2}+\frac{\mu'}{h^2b^3}(1-3e\cos(\theta+\gamma))=0.$$

This equation gives by integration,

$$u=\text{A}\cos(\theta+\text{B})+\frac{\mu}{h^2}-\frac{\mu'}{h^2b^3}+\frac{3\mu' e}{2h^2b^3}\,\theta\sin(\theta+\gamma).$$

Hence by comparison with the assumed value of u,

$$b=\frac{\mu}{h^2}-\frac{\mu'}{h^2b^3}=\frac{\mu}{h^2}-\frac{\mu' h^4}{\mu^3}\text{ nearly,}$$

$$be=\text{A},\qquad\text{and }\gamma=\text{B}.$$

Consequently the integral of equation (1) in this case is

$$u=\frac{\mu}{h^2}-\frac{\mu' h^4}{\mu^3}+\text{A}\cos(\theta+\text{B})+\frac{3\mu' h^4}{2\mu^4}\text{A}\theta\sin(\theta+\text{B}).$$

Now since the application of this integral is limited by the condition that u must differ little from a constant value, it follows that the angle θ must be taken within certain limits, otherwise that condition is violated. Putting for the sake of brevity q for $\frac{3\mu' h^4}{2\mu^4}$, it will be found that $\frac{du}{d\theta}=0$ gives

$$\tan(\theta+\text{B})=\frac{q\theta}{1-q}$$

$$\text{and }\frac{d^2u}{d\theta^2}=\text{A}\cos(\theta+\text{B})\,.\left\{-1+2q-\frac{q^2\theta^2}{1-q}\right\}.$$

These results show, since θ may be supposed positive, that $\theta+\mathrm{B}$ will be less than 90°, and that, as A is positive, u has a maximum value if $2q$ be less than unity, but cannot in that case have a minimum value. Hence as q is by supposition a small quantity, it is clear that the maximum does not apply to the elliptical orbit, but *to the third apse;* for if it applied to the elliptical orbit, u would also have a minimum value.

If, however, the above approximation, viz. $u=b+be\cos(\theta+\gamma)$, be derived by integration from the approximate equation

$$\frac{d^2u}{d\theta^2}+u-\frac{\mu}{h^2}=0,$$

the second approximation can have no reference to the third apsidal distance, and ought to be true for all values of θ. But this cannot be the case, unless the above integral be considered to be derived by a diverging expansion from the equation,

$$u=\frac{\mu}{h^2}-\frac{\mu' h^4}{\mu^3}+\mathrm{A}\cos\left(\sqrt{1-\frac{3\mu' h^4}{\mu^4}}\,\theta+\mathrm{B}\right).$$

The foregoing reasoning shows that when the integral of equation (1) contains a term which involves θ as a factor, this circumstance is due to the particular process of integration, by which the values of the radius-vector for points contiguous to the third apse are taken into account, and that although the same integral approximates to an orbit of small eccentricity, it is attended with the inconvenience of presenting the approximation under a divergent form. I have already pointed out one method of obviating this inconvenience, and excluding the third apse from the investigation. The same purpose is answered by approximating to the two nearly *equal* roots of the apsidal equation (2), which is the principle of the process I have adopted for finding in a direct manner the mean motion of the apses of the moon's orbit. A third method will be seen in my article in the February Number (p. 132), in which the approximation commences from a fixed ellipse. The reason of the success of this method is, that by so commencing the approximation, the elliptic apsidal distances are taken account of, to the exclusion of the third apsidal distance.

All the above considerations, which for simplicity have been applied to the case of a central force, are equally applicable to the moon's orbit. I think I have now clearly pointed out the origin, in the Lunar Theory, of terms which admit of indefinite increase, and how they may be avoided. It appears that they have no relation whatever to the circumstance of commencing the approximation from a fixed ellipse. I confess that when I entered upon these researches, I little expected such a result.

In the February Number (p. 133), I have adduced an argument to prove, on the assumption of the illegitimacy of starting from a fixed ellipse, that the apsidal equation contains the disturbing force as a factor. It is now shown that the illegitimacy of that procedure cannot be assumed on the ground of its giving rise to non-periodic factors in the development of the radius-vector. I shall take a future occasion for showing that the argument, together with the new theorem respecting the eccentricity of the moon's orbit, which depends upon it, may still be maintained.

Cambridge Observatory,
Feb. 16, 1855.

XXIX. *On the Existence of a Magnetic Medium in Space.*

By JOHN TYNDALL, *Ph.D., F.R.S.*

MY DEAR MR. FARADAY,

FEW, I imagine, who read your memoir in the last Number of the Philosophical Magazine, will escape the necessity of reconsidering their views of magnetic action. We are so accustomed to regard the phænomena of this portion of science through the imagery with which hypothesis has invested them, that it is extremely difficult to detach symbols from facts, and to view the latter in their purity. This duty, however, is now forced upon us; for the more we reflect upon the results of recent scientific research, the more deeply must we be convinced of the impossibility of reconciling these results with our present theories[*]. In the downfall of hypotheses thus pending, the great question of a universal magnetic medium has presented itself to your mind. Your researches incline you to believe in the existence of such a medium, and lead you, at the same time, to infer the perfect identity of magnetism and diamagnetism.

In support and illustration of your views, you appeal to the following beautiful experiments:—Three solutions of protosulphate of iron are taken; the first, *l*, contains 4 grains; the second, *m*, 8 grains; and the third, *n*, 16 grains of the salt to a cubic inch of water. Enclosed in hollow globules of glass, all these solutions, when suspended in the air before the pole of a magnet, are attracted by the pole. You then place a quantity of the medium solution, *m*, in a proper vessel, immerse in it the globule containing the strong solution *n*, and find that the latter is still attracted; but that when the globule containing the solution *l* is immersed, the latter is *repelled* by the magnetic pole. Substituting elongated tubes for spheres, you find that when a tube containing a solution of a certain strength is suspended in a weaker solution, between the two poles of a magnet, the tube sets from pole to pole; but that when the solution *without* the tube is stronger than that *within* it, the tube recedes from the pole and sets equatorially.

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[*] Some of the reasons which induce the writer to hold this opinion are given in the Bakerian Lecture of the Royal Society for the present year.

Here then, you state, are the phænomena of diamagnetism. It is maintained by some, that, to account for these phænomena, it is necessary to assume, in the case of diamagnetic bodies, the existence of a polarity the reverse of that of iron. But nobody will affirm that the mere fact of its being suspended in a stronger solution reverses the polarity of a magnetic liquid: —to account for the repulsion of the weak solution, when submersed in a stronger one, no such hypothesis is needed; why then should it be thought necessary in the case of so-called diamagnetic bodies? It is only by denying that space presents a medium which bears the same relation to diamagnetic bodies that the stronger magnetic solution bears to the weaker one, that the hypothesis of a distinct diamagnetic polarity is at all rendered necessary.

The effects upon which the foregoing striking argument is based are differential ones, and are embraced, as already observed by M. E. Becquerel, by the so-called principle of Archimedes. This principle, in reference to the case before us, affirms that the body immersed in the liquid is attracted by a force equal to the difference of the attractions exerted upon the liquid and the body immersed in it. Hence, if the attraction of the liquid be less than that of the immersed body, the latter will approach the pole; if the former attraction be the greater, the immersed body recedes from the pole, and is apparently repelled. The action is the same as that of gravity upon a body immersed in water: if the body be more forcibly attracted, bulk for bulk, than the water, it sinks; if less forcibly attracted, it rises; the mechanical effect being the same as if it were repelled by the earth.

The question then is, are all magnetic phænomena the result of a differential action of this kind? Does space present a medium less strongly attracted than soft iron, and more strongly attracted than bismuth, thus permitting of the approach of the former, but causing the latter to recede from the pole of a magnet? If such a medium exists, then diamagnetism, as you incline to believe, merges into ordinary magnetism, and "the polarity of the magnetic force," in iron and in bismuth, is one and the same.

Pondering upon this subject a few evenings ago, and almost despairing of seeing it ever brought to an experimental test, a thought occurred to me, which, when it first presented itself, seemed to illuminate the matter. Such illuminations vanish in nine cases out of ten before the test of subsequent criticism; but the thought referred to, having thus far withstood the criticism brought to bear upon it, I am emboldened to submit it to you for consideration.

I shall best explain myself by assuming that a medium of the nature described exists in space, and pursuing this assumption to its necessary consequences.

Let a cube, formed from the impalpable dust of carbonate of iron*, which has been compressed forcibly in one direction, be placed upon the end of a torsion beam, and first let the line in which the pressure has been exerted be in the direction of the beam. Let a magnet, with its axis at right angles to the beam, and hence also at right angles to the line of pressure, be brought to bear upon the cube. The cube will be attracted, and the amount of this attraction, at any assigned distance, may be accurately measured by the torsion of the wire from which the beam depends. Let this attraction, expressed in degrees of torsion, be called a. Let the cube now be turned round 90°, so that the line of pressure shall coincide with the direction of the axis of the magnet, and let the attraction a' in this new position be determined as in the former instance. On comparison it will be found that a' exceeds a; or, in other words, that the attraction of the cube is strongest when the force acts parallel to the line of compression†.

Instead of carbonate of iron we might choose other substances of a much feebler magnetic capacity, with precisely the same result. Let us now conceive the magnetic capacity of the compressed cube to diminish gradually, and thus to approach the capacity of the medium in which, according to our assumption, the carbonate of iron is supposed to be immersed. If it were a perfectly homogeneous cube, and attracted with the same force in all directions, we should at length arrive at a point, when the *magnetic weight* of the cube, if I may use the term, would be equal to that of the medium, and we should then have a substance which, as regards magnetism, would be in a condition similar to that of a body withdrawn from the action of gravity in Plateau's experiments. Such a body would be neither attracted nor repelled by the magnet. In the compressed cube, however, the magnetic weight varies with the direction of the force; supposing the magnetic weight, when the force acts along the line of compression, to be equal to that of the medium, then if the force acted across the line of compression, the magnetic weight of the cube would be less than that of the medium. Acted upon in the former direction, the cube would be a neutral body; acted upon in the latter direction, it would be a diamagnetic body. If the magnetic capacity of the cube diminish still further, it will, according to your hypothesis, become wholly diamagnetic. Now it is evident, supposing the true magnetic excitement to continue, that the cube, when acted on by the magnet in the direction of compression, *will approach nearer to the magnetic weight of the medium* in which we suppose it immersed, than when the action is across the said line; and, hence, the repulsion of the cube, when the force acts along the line of compression, must be *less* than when the force acts across it.

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* For an ample supply of this most useful mineral, I am indebted to the kindness of J. Kenyon Blackwell, Esq., F.G.S.

† Phil. Mag. Sept. 1851; Pogg. *Ann.* 1851.

Reasoning thus from the assumption of a magnetic medium in space, we arrive at a conclusion which can be brought to the test of experiment. So far as I can see at present, the assumption is negatived by this test; for in diamagnetic bodies the repulsion along the line in which the pressure is exerted is proved by experiment to be a *maximum* *. An ordinary magnetic excitement could not, it appears to me, be accompanied by this effect.

The subject finds further, and perhaps clearer, elucidation in the case of isomorphous crystals. It is not, I think, questioned at present, that the deportment of crystals in the magnetic field depends upon their molecular structure; nor will it, I imagine, be doubted, that the molecular structure of a complete crystal of carbonate of iron is the same as that of an isomorphous crystal of carbonate of lime. In the architecture of the latter crystal, calcium simply takes the place which iron occupies in the former. Now a crystal of carbonate of iron is attracted most forcibly when the attracting force acts parallel to the crystallographic axis †. Let such a crystal be supposed to diminish gradually in magnetic capacity, until finally it attains a magnetic weight, *in a direction parallel to its axis*, equal to that of the medium in which we assume it to be immersed. Such a crystal would be indifferent, if the force acted parallel to its axis, but would be repelled, if the force acted in any other direction. If the magnetic weight of the crystal diminish a little further, it will be repelled in all directions, or, in other words, will become diamagnetic; but it will then follow, that the repulsion in the direction of the axis, if the nature of the excitement remain unchanged, will be less than in any other direction. In other words, a diamagnetic crystal of the form of carbonate of iron will, supposing magnetism and diamagnetism to be the same,

* Phil. Mag. Sept. 1851. Pogg. *Ann.* 1851.

† Phil. Mag. Sept. 1851. Pogg. *Ann.* 1851.

be repelled with a *minimum* force when the repulsion acts parallel to the axis. Here, as before, we arrive at a conclusion which is controverted by experiment; for the repulsion of a crystal of carbonate of lime is a *maximum* when the repelling force acts along the axis of the crystal. Hence I would infer that the excitement of carbonate of iron cannot be the same as that of carbonate of lime.

Such are the reflections which presented themselves to my mind on the evening to which I have referred. I now submit them to you as a fraction of that thought which your last memoir upon this great question will assuredly awaken.

Believe me,
Dear Mr. Faraday,
Yours very faithfully,
JOHN TYNDALL.

Royal Institution,
February 1855.

XXX. *Theorems on the Quadrature of Surfaces and the Rectification of Curves. By the* Rev. ROBERT CARMICHAEL, *A.M., Fellow of Trinity College, Dublin, and Examiner in Mathematics for the year* 1854 *in the Queen's University in Ireland*[*].

1. IT is well known that there are many plane curves whose equations are more easily expressed in polar than in rectangular coordinates, and for whose rectification we employ the formula

$$S=\int_{\theta_1}^{\theta_2}\sqrt{\{r^2+(D_\theta r)^2\}}\,d\theta.$$

Of this class are, the spiral of Archimedes,

$$r=a\theta;$$

the lituus,

$$r^2=\left(\frac{a}{\theta}\right)^2;$$

the lemniscate,

$$r^2=a^2\cos 2\theta;$$

the logarithmic spiral,

$$r=ce^{\frac{\theta}{a}};$$

and the cardioid,

$$r=a(1-\cos\theta).$$

2. I am not aware that any mathematician has attempted to trace the surfaces analogous to these; but for the *quadrature* of such surfaces, when discovered, it is absolutely necessary that we should have a general expression *in polar coordinates* for the element of any surface. Such an expression is not found in the

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[*] Communicated by the Author.

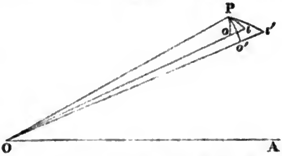
ordinary works upon the differential and integral calculus. In the elaborate treatise upon this subject by M. L'Abbé Moigno (Paris, 1844, vol. ii. p. 235), the expression is investigated, by the usual analytical method, transformation of coordinates, from the well-known expression in rectangular coordinates,

$$d\sigma = \sqrt{(1+p^2+q^2)}dx\,dy,$$

and is given in the following shape,

$$d\sigma = \sqrt{\{r^2\sin^2\theta + \sin^2\theta(\mathrm{D}_\theta r)^2 + (\mathrm{D}_\phi r)^2\}}\,r\,d\theta\,d\phi.$$

A short geometrical deduction of this expression, whose merit I have great pleasure in sharing with my friend, Alexander Jack, Esq., A.B., may not be unacceptable to the student.



Let P be any point on the surface. Through the axis OA and OP describe a plane, and round the axis describe, with the same line, a cone. The surface may then be supposed to be divided into its elements by planes and cones consecutive to these respectively (the planes all passing through the axis and the cones round it), half of one such element being represented by ιPι'. Then, remembering that the planes cut the cones orthogonally, we have

$$d\sigma = \mathrm{P}\iota\,.\,\mathrm{P}\iota'\,.\,\sin\iota\mathrm{P}\iota' = \mathrm{P}\iota\,.\,\mathrm{P}\iota'\,.\,\sqrt{(1-\cos^2\iota\mathrm{P}\iota')},$$

whence

$$d\sigma = \mathrm{P}\iota\,.\,\mathrm{P}\iota'.\sqrt{(1-\sin^2\iota\mathrm{P}o\,.\,\sin^2\iota'\mathrm{P}o')} = \sqrt{(\mathrm{P}\iota^2\,.\,\mathrm{P}\iota'^2 - o\iota^2.o'\iota'^2)},$$

o and o' being the points where the sphere described round the origin with radius OP intersects the consecutive radii vectores to the points ι, ι'; or

$$d\sigma = \sqrt{[\{r^2\sin^2\theta d\phi^2 + (\mathrm{D}_\phi r)^2 d\phi^2\}\{r^2 d\theta^2 + (\mathrm{D}_\theta r)^2 d\theta^2\} - (\mathrm{D}_\theta r)^2 d\theta^2\,(\mathrm{D}_\phi r)^2 d\phi^2]},$$

or finally,

$$d\sigma = \sqrt{\{r^2\sin^2\theta + \sin^2\theta(\mathrm{D}_\theta r)^2 + (\mathrm{D}_\phi r)^2\}}\,r\,d\theta\,d\phi.$$

3. From this expression we may readily derive that for the perpendicular from the origin upon the tangent plane, in *polar* coordinates. In *rectangular* coordinates it is known to be

$$\mathrm{P} = \frac{z-px-qy}{\sqrt{(1+p^2+q^2)}};$$

but the transformation of this to polar coordinates would be troublesome and tedious. We may easily derive the required expression from the volume of the elementary cone, for

$$\mathrm{P}d\sigma = r^3\sin\theta\,d\theta\,d\phi,$$

and therefore

$$P=\frac{r^2\sin\theta}{\sqrt{\{r^2\sin^2\theta+\sin^2\theta(D_\theta r)^2+(D_\phi r)^2\}}}.$$

4. As an example of the application of the formula for the quadrature of surfaces, let us suppose that it is required to investigate the quadrature, between given limits, of the surface

$$r=me^{-\phi}\cos\theta.$$

Then

$$D_\theta r=-me^{-\phi}\sin\theta,\quad D_\phi r=-me^{-\phi}\cos\theta;$$

therefore

$$d\sigma=\sqrt{(m^2e^{-2\phi}\cos^2\theta\sin^2\theta+m^2e^{-2\phi}\sin^4\theta+m^2e^{-2\phi}\cos^2\theta)}\,rd\theta\,d\phi,$$

or

$$d\sigma=m^2e^{-2\phi}\cos\theta\,d\theta\,d\phi;$$

whence

$$\Sigma=m^2\int e^{-2\phi}(\sin\theta_2-\sin\theta_1)d\phi.$$

Let us suppose the limits to be given by the intersections, with the given surface, of the cones

$$\theta_2=a\phi,\quad\theta_1=b\phi,$$

and

$$\Sigma=m^2\int_{\phi_1}^{\phi_2}e^{-2\phi}(\sin a\phi-\sin b\phi)\,d\phi,$$

an integral which is susceptible of easy reduction, since we know that

$$\int e^{-m\phi}\sin a\phi\,d\phi=-e^{-m\phi}\frac{m\sin a\phi+a\cos a\phi}{m^2+a^2}.$$

5. As a second example, let it be proposed to investigate the quadrature, within given limits, of the surface

$$r=m\cos\phi\sin\theta.$$

Here

$$D_\theta r=m\cos\phi\cos\theta,\quad D_\phi r=-m\sin\phi\sin\theta,$$

and

$$d\sigma=m^2\cos\phi\sin^2\theta\,d\theta\,d\phi;$$

whence

$$\Sigma=m^2\int_{\theta_1}^{\theta_2}(\sin\phi_2-\sin\phi_1)\sin^2\theta\,d\theta;$$

and, if the limits be given as before, there is no difficulty in determining the quadrature completely.

6. In the masterly treatise upon the Calculus of Variations by the Rev. Professor Jellett (Dublin, 1850, p. 262), it is shown that the surface which, within given limits, renders the double integral

$$\iint\sqrt{(p^2+q^2)}dxdy,$$

or, γ being the angle made by the radius vector with the axis of z,

$$\iint \sin\gamma \,.\, d\sigma,$$

a minimum, is given by the partial differential equation

$$q^2r - 2pqs + p^2t = 0,$$

whose integral is known to be

$$x\mathrm{F}_1(z) + y\mathrm{F}_2(z) = 1,$$

representing the gauche surface generated by a right line which, gliding upon two fixed directrices, remains constantly parallel to the plane of the axes of x and y; as indeed might be anticipated from a consideration of the question in its second form.

In the same manner it might be shown that the surface which, within given limits, renders the double integral

$$\iint \sqrt{\{(\mathrm{D}_\theta r)^2 + (\mathrm{D}_\phi r)^2\}}\, d\theta\, d\phi$$

a minimum, is given by the equation

$$\phi\mathrm{F}_1(r) + \theta\mathrm{F}_2(r) = 1.$$

If it be proposed to investigate the property of this surface corresponding to the character of the generation of the analogous surface in rectangular coordinates, as the latter character is exhibited by the supposition $z = const.$, so the former property may be investigated by the supposition $r = const.$ Let, then, the surface be supposed to intersect a sphere described round the origin, and let the nature of the curve of intersection be examined. If we resolve any element into its rectangular components, one such component is $r\,d\theta$, and the other $r\sin\theta\, d\phi$. Let i be the inclination of the element to the meridional plane described through its extremity and the fixed axis, and it is evident that

$$\tan i = \frac{r\sin\theta\, d\phi}{r\, d\theta} = -\frac{\mathrm{F}_2(c)}{\mathrm{F}_1(c)}\sin\theta,$$

c being the radius of the sphere; or *the tangent of the angle of inclination of the curve to the meridional plane is proportional to the sine of the angle made by the radius vector with the axis.*

7. It may be well here to indicate certain desiderata, the knowledge of which might lead to the discovery of some interesting properties of surfaces.

The *measure of curvature* at any point of a surface is expressed in rectangular coordinates by the formula

$$\frac{1}{\mathrm{R}_1\mathrm{R}_2} = \frac{rt - s^2}{(1 + p^2 + q^2)^{\frac{3}{2}}}:$$

we have no corresponding expression in polar coordinates. Such might be discovered by the investigation of the analogue of the known formula for plane curves

$$\rho = r\frac{dr}{dp}.$$

Again, the *sum of the curvatures* at any point of a surface is expressed by the formula, in rectangular coordinates,

$$\frac{1}{R_1} + \frac{1}{R_2} = -\frac{(1+q^2)r - 2pqs + (1+p^2)t}{(1+p^2+q^2)^{\frac{3}{2}}}:$$

we have no corresponding expression in polar coordinates. Other desiderata will readily suggest themselves.

8. With regard to the rectification of curves, it may be useful to make a few observations upon a subject which has recently attracted much attention among French mathematicians. In the Notes by M. Liouville to his valuable edition of the *Application de l'Analyse à la Geometrie* of the illustrious Monge, will be found (p. 558) the following remarks:—

"M. Serret a fait usage de certaines variables qu'il avait déjà employées au tome xiii. du *Journal de Mathématiques*, pour resoudre le problème suivant: x, y, z, s étant quatre fonctions d'une variable indépendente θ assujetties à verifier l'équation

$$dx^2 + dy^2 + dz^2 = ds^2,$$

exprimez sans forme finie et sans aucun signe d'intégration, les valeurs générales de ces fonctions. La solution de ce problème conduit, par exemple, à trouver des courbes à double courbure qui soient à la fois algébriques et rectifiables algébriquement, ou dont l'arc dépende d'une transcendante donnée. Le problème analogue pour les courbes planes dépend de l'équation plus simple

$$dx^2 + dy^2 = ds^2,$$

et se resout, comme on sait, par les formules

$$x = \psi'(\theta)\sin\theta + \psi''(\theta)\cos\theta,$$
$$y = \psi'(\theta)\cos\theta - \psi''(\theta)\sin\theta,$$
$$s = \psi'(\theta) + \psi''(\theta),$$

où la fonction ψ est arbitraire. Les formules de M. Serret pour l'équation

$$dx^2 + dy^2 + dz^2 = ds^2,$$

sont beaucoup plus compliquées, et, pourtant, beaucoup moins utiles."

It appears to me that the integration of these equations may be effected directly, and with great simplicity, by employing the Calculus of Quaternions.

Thus, in the notation of this Calculus, the first equation

$$dx^2 + dy^2 = ds^2$$

is equivalent to

$$-(idx+jdy)^2=-(d\rho)^2,$$

or

$$idx+jdy=d\rho;$$

whence

$$ix+jy=\rho+\alpha,$$

α being an arbitrary vector; or, between given limits,

$$i(x_2-x_1)+j(y_2-y_1)=\rho_2-\rho_1,$$

an identity, as it ought to be.

Similarly, the second equation

$$dx^2+dy^2+dz^2=ds^2$$

is equivalent to

$$-(idx+jdy+kdz)^2=-(d\rho)^2,$$

or

$$idx+jdy+kdz=d\rho;$$

whence

$$ix+jy+kz=\rho+\alpha,$$

α being an arbritrary vector, or, between given limits,

$$i(x_2-x_1)+j(y_2-y_1)+k(z_2-z_1)=\rho_2-\rho_1,$$

an identity, as it ought to be.

5 Trinity College, Dublin,
November 1854.

XXXI. *On the Products of the Destructive Distillation of Animal Substances.*—Part III. *By* Thomas Anderson, *M.D., Regius Professor of Chemistry in the University of Glasgow.*

[Concluded from p. 150.]

Constitution of the Bases of the Picoline Series.

HAVING in this and the previous part of these researches accumulated sufficient evidence of the existence of a class of bases isomeric with that of which aniline is the type, it became important to determine to which of the three classes of volatile bases they belong. For this purpose, pyridine, picoline, and collidine were submitted to the action of iodide of æthyle. The experiments were carried out in considerable detail with picoline, but with the other two salts no more was done than sufficed to substantiate the fact, that iodide of æthyle acted on them in a similar manner.

Action of Iodide of Æthyle on Picoline.

Anhydrous picoline and iodide of æthyle were mixed, in the proportion of one volume of the former to two of the latter, and sealed hermetically in a combustion tube. The two fluids mix readily, but if the tube containing them be gently heated, by plunging it for the space of half a minute into the water-bath, an action takes place attended with the evolution of much heat, the fluid becomes muddy, and separates into a thick oily stratum which rises to the surface, and a more fluid one which descends. On cooling, the former solidifies into a highly crystalline mass, and well-formed crystals appear in the latter, which consists of the excess of iodide of æthyle. Even without the application of heat the action takes place, though more slowly, and it is necessary to have them in contact for some days before the action is finished. When heat is applied, the action is complete in ten minutes, but in every instance the tubes were allowed to stand for twenty-four hours, so as to present the complete crystallization of the new compound. The tubes were then opened, the whole contents thrown upon a filter, and the crystals slightly washed with a mixture of alcohol and æther, pressed between folds of filtering paper, and dissolved in the smallest possible quantity of a mixture of boiling alcohol and æther. On cooling, the new substance is deposited in beautiful silvery plates. It is highly soluble in water, and though not deliquescent, it becomes slightly damp in moist air. Its aqueous solution, on evaporation, solidifies into a mass of crystals. It is readily soluble in alcohol, especially when boiling, and the hot solution on cooling becomes filled with crystals. It is less soluble in æther. It fuses below 212° into an oily fluid. Its analysis gave these results:—

5·350 grs., dried *in vacuo*, gave 7·570 grs. carbonic acid and 2·375 grs. water.

6·440 grs., dried *in vacuo*, gave by direct precipitation 6·065 grs. iodide of silver.

	Experiment.	Calculation.		
Carbon . .	38·57	38·70	C^{16}	96
Hydrogen .	4·93	4·83	H^{12}	12
Nitrogen . .	5·61	5·67	N	14
Iodine . .	50·89	50·80	I	126
	100·00	100·00		248

These results agree with the formula $C^{16} H^{11} N HI$, which is that of hydriodate of æthylopicoline.

On the addition of a few drops of caustic potash to the solution of this salt no odour of a volatile base is evolved, nor is there any separation of an oily layer, but the addition of a large quantity of strong potash causes the precipitation of a viscid oil, which solidifies on standing for some hours into a mass of crystals, generally much coloured, and which proves to be the hydriodate partially altered by decomposition. When boiled with strong potash, a volatile base is slowly formed, which is a product of decomposition, and will be afterwards referred to. From these characters it is obvious that æthylopicoline belongs to the ammonium class of bases, and hence picoline itself must be a nitryle base; and this being the case, the formula of the iodine compound might be written thus, $C^{16} H^{12} N + I$, representing it as the iodide of a base corresponding to ammonium, of which the constitution must be $C^{16} H^{12} N$, and whose oxide, $C^{16} H^{12} NO$, must exist in its oxygen acid salts. If we adopt the nomenclature proposed by Hofmann for the ammonium bases, we should have a very clumsy name for this substance, and I shall therefore continue to call it æthylopicoline, which, though not perfectly correct, is sufficiently distinctive.

Æthylopicoline, or rather its oxide, is readily obtained by agitating the aqueous solution of the iodide with moist oxide of silver, when iodide of silver is precipitated, and the base obtained in solution. In performing this process, heat must be avoided, as oxide of silver decomposes the base at a high temperature, fine violet streaks appearing in the fluid, which rapidly acquires a deep crimson colour. The same change occurs, though more slowly, in the cold, especially if the oxide of silver be added in large excess, and it is therefore desirable that the solution should be separated as rapidly as possible. If this be done, a colourless solution is obtained, having a faint peculiar odour and highly alkaline properties. It restores the blue of reddened litmus, and gives an intense brown with turmeric; it has a powerfully caustic taste, and produces a soapy sensation when rubbed between the fingers. It absorbs carbonic acid from the air, precipitates alumina, and redissolves it when added in excess. From a solution of corrosive sublimate it throws down the oxide, and with the metallic salts generally it reacts in precisely the same manner as potash or soda. On boiling, its solution acquires a deep red colour, and the odour of a volatile base becomes apparent. By evaporation *in vacuo*, a hard gummy mass with a green metallic lustre is left behind, which gives a magnificent blood-red solution with water, and deliquesces when exposed to the air. At first I entertained the hope that this substance, though coloured, might, when submitted to analysis, give the results of æthylopicoline; a very few experiments, however, sufficed to show that it had undergone decomposition, and no attempt was made to analyse it, but attention was directed to obtaining such double salts as might serve to confirm the constitution of the base.

Platinochloride of Æthylopicoline.—In order to obtain this salt, nitrate of silver was added to the iodide as long as a precipitate was formed, which was separated by filtration, and the excess of silver thrown down by hydrochloric acid. The filtrate was then mixed with a strong solution of bichloride of platinum and set aside. In the course of a few hours the salt was deposited in orange-red tabular crystals of remarkable beauty, and often of considerable size. It is readily soluble in cold water, and still more so in hot, and is deposited unchanged from its solution. It possesses considerable stability, but by long-continued boiling it undergoes decomposition. Its analysis gave,—

6·015 grs. of platinochloride of æthylopicoline gave 6·430 grs. carbonic acid and 2·040 grs. water.

6·825 grs. of platinochloride of æthylopicoline gave 2·031 grs. platinum.

6·910 grs. of platinochloride of æthylopicoline gave 2·058 grs. platinum.

6·970 grs. of platinochloride of æthylopicoline gave 2·085 grs. platinum.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon .	29·15	...	...	29·33	C^{16}	96
Hydrogen	3·76	...	...	3·66	H^{12}	12
Nitrogen	...	...	...	4·31	N	14
Chlorine	...	...	...	32·54	Cl^3	106·5
Platinum	29·75	29·78	29·91	30·16	Pt	98·7
				100·00		327·2

Aurochloride of Æthylopicoline.—This compound is readily formed by adding a solution of chloride of gold to the nitrate, with excess of hydrochloric acid, obtained from the iodide, in the manner employed for the production of the platinum salt. It is slowly deposited in the form of golden-yellow flattened prisms of great beauty. It is sparingly soluble in cold water, readily in hot, and is deposited unchanged on cooling. It is insoluble in alcohol and æther. Ammonia converts it into a cinnamon-brown powder, and it is instantly blackened on the addition of potash to its hot solution. The specimen analysed was dried at 212°, and burnt with chromate of lead.

6·745 grs. of aurochloride of æthylopicoline gave 5·093 grs. carbonic acid and 1·675 gr. water.

5·300 grs. of aurochloride of æthylopicoline gave 2·265 grs. gold.

	Experiment.	Calculation.		
Carbon	20·59	20·83	C^{16}	96
Hydrogen . .	2·75	2·60	H^{12}	12
Nitrogen . .	...	3·06	N	14
Chlorine	...	30·82	Cl^4	142
Gold	42·73	42·69	Au	196·6
		100·00		460·6

Corresponding with the formula $C^{16} H^{12} N Cl + Au Cl^3$.

It has been already mentioned that though æthylopicoline is fixed and inodorous, its iodide cannot be distilled with potash, or the base itself boiled or even evaporated *in vacuo*, without undergoing a decomposition, attended with the evolution of volatile base. In the latter case the decomposition is slow; and even after the ebullition has been continued for some hours, the odour is given off with undiminished intensity, till by long-continued boiling it at length becomes extremely faint, although it does not altogether disappear. When the iodide is boiled with potash, the change is more rapid, and after three or four hours' boiling a considerable quantity of base is found in the receiver. The product has a pungent and putrid odour, fumes strongly with hydrochloric acid, and forms with it a salt entirely soluble in absolute alcohol. Two analyses were made of the platinum compound of this base; the one from a portion collected at the commencement, the other towards the end of the distillation, which show that the product was of uniform composition throughout. The results were as follows:—

I. 6·440 grs. of platinochloride gave 2·430 grs. carbonic acid and 1·920 gr. water.

II. 11·775 grs. of platinochloride gave 4·210 grs. carbonic acid and 3·457 grs. water.

I. 4·385 grs. of platinochloride gave 1·705 gr. platinum.

II. 6·580 grs. of platinochloride gave 2·575 grs. platinum.

	Experiment.		Calculation.		
	I.	II.			
Carbon	10·29	9·75	9·55	C^4	24
Hydrogen . . .	3·31	3·26	2·78	H^8	8
Nitrogen . . .	...	...	5·99	N	14
Chlorine	...	...	42·39	Cl^3	106·5
Platinum . . .	38·88	39·23	39·29	Pt	98·7
			100·00		251·2

Its formula, therefore, is $C^4 H^7 N, H Cl + Pt Cl$, and the base itself is æthylamine.

The base obtained by the distillation of the æthylopicoline alone was found to have the same composition, for 6·177 grs. of its platinum salt gave 2·413 grs. of platinum, equal to 39·06 per cent.

The decomposition which thus occurs is very remarkable, and differs entirely from that observed by Hofmann in the ammonium bases examined by him. The oxide of tetræthylammonium, for instance, is not decomposed when evaporated *in vacuo*. Even at 212° it undergoes no change until it becomes nearly dry, but then a base and a permanent gas are evolved, the former being triæthylamine, and the latter olefiant gas. In this case, one out of the four æthyle atoms which the complex base contained is decomposed, and the other three remain with the ammonia in the form of a nitryle base; in fact, we may fairly assume that the atom of æthyle added to the triæthylamine to convert it into tetræthylammonium is decomposed, and the base which formed the starting-point of that action is regenerated. With methæthylopicoline the case is different; we start, indeed, from a nitryle base, but in place of reproducing it in the decomposition, the atom of æthyle which has been added takes possession of the ammonia, and produces an amide base, leaving the radicals, which we must assume to have replaced the three atoms of hydrogen in the ammonia from which the picoline was originally produced, in some other form of combination. In another point, also, the decomposition of æthylopicoline differs from that of tetræthylammonium. According to Hofmann, the latter base is entirely converted into triæthylamine and olefiant gas; but æthylopicoline, even after long-continued boiling, gives an abundant residue on evaporation. The substance so obtained is amorphous, has an intense blood-red colour, and is a base forming a platinum salt insoluble in water. Although these experiments were made on a very small scale, and the slowness of the action rendered it impossible to say with certainty whether the decomposition was complete, this platinum compound was analysed, and the results were—

5·840 grs. of the platinum salt gave 8·550 grs. carbonic acid and 2·390 grs. water.

7·555 grs. of the platinum salt gave 1·652 gr. platinum.

Carbon	39·92
Hydrogen . . .	4·54
Platinum	21·86

From a single analysis such as this it is impossible to deduce a formula; but it is obvious that a base of much higher atomic weight than æthylopicoline has been produced, the further examination of which must be deferred to a future paper, and which will probably lead to interesting results.

Action of Iodide of Æthyle on Pyridine.

When pyridine is treated with iodide of æthyle, the action, as might be expected, is in all respects similar to that which occurs with picoline. A homogeneous mixture is first formed, and then, on gently warming, the action takes place, with the evolution of much heat, and the hydriodate of æthylopyridine rises to the surface as an oily layer. The crystallization of this substance, as it cools, is an extremely beautiful phænomenon. Minute rhombs make their appearance here and there in the viscid fluid, where they increase in size so rapidly that they may actually be seen to grow; and in a successful operation they sometimes increase to the size of from a quarter to three-eighths of an inch in diameter in the course of half an hour. By and by the crystals come into contact with one another, and the fluid is converted into a solid crystalline mass. The crystals are removed from the tube, pressed in folds of filtering paper, and crystallized from a mixture of absolute alcohol and æther. They then form fine silvery plates, highly soluble in water, and slightly deliquescent; in alcohol and æther they are also extremely soluble, though less so than in water. With reagents, their behaviour is so exactly the same as that of the æthylopicoline salts, that it is unnecessary to enter into any details. By analysis the following results were obtained:—

6·110 grs. of hydriodate of æthylopyridine gave 8·105 grs. carbonic acid and 2·525 grs. water.

5·445 grs. of hydriodate of æthylopyridine gave 5·395 grs. iodide of silver.

	Experiment.	Calculation.		
Carbon . .	36·17	35·89	C^{14}	84
Hydrogen .	4·59	4·27	H^{10}	10
Nitrogen . .	5·70	6·04	N	14
Iodine . .	53·54	53·80	I	126
	100·00	100·00		234

The formula of the substance, therefore, is $C^{14} H^{10} N I$.

Æthylopyridine itself may be separated from the salt by the action of oxide of silver. It forms a highly alkaline fluid, which undergoes decomposition when heated, with the evolution of a base which is no doubt æthylamine, and agrees completely with it in properties, though the small scale on which the experiment was performed prevented my establishing this fact by analysis. It unites with acids, and forms salts, which are all crystallizable, and generally highly soluble. The platinum and gold salts are extremely beautiful compounds.

Platinochloride of Æthylopyridine.—This salt was prepared in the same manner as the corresponding æthylopicoline compound. It is sparingly soluble in cold water, and insoluble in a mixture of alcohol and æther. When slowly formed, it is obtained in beautiful garnet-coloured rhomboidal plates with bevelled edges, which are easily got of a quarter of an inch in diameter, even when operating on very small quantities. Its analysis gave—

7·152 grs. of æthylopyridine platinum salt gave 6·905 grs. carbonic acid and 1·885 gr. water.

6·435 grs. of æthylopyridine platinum salt gave 2·035 grs. platinum.

	Experiment.	Calculation.		
Carbon . .	26·33	26·81	C^{14}	84
Hydrogen .	2·92	3·19	H^{10}	10
Nitrogen . .	...	5·56	N	14
Chlorine . .	...	32·93	Cl^3	106·5
Platinum . .	31·62	31·51	Pt	98·7
		100·00		313·2

The formula of the compound is $C^{14} H^{10} N Cl + Pt Cl^2$.

The gold compound of æthylopyridine is obtained in fine yellow plates of extreme beauty, sparingly soluble in cold water, and readily decomposed in boiling, especially if an excess of chloride of gold be present. They were not analysed.

Action of Iodide of Æthyle on Collidine.

Iodide of æthyle and collidine react upon one another in the same manner as the bases already mentioned. An oily layer separates on heating the mixture, which refuses to crystallize on cooling. After removal from the tube in which the action was effected, and separation from the excess of iodide of æthyle, the fluid was allowed to stand for some time, but no crystals appeared. It was then exposed to cold, in the hope of inducing crystallization, but without success; and no better result followed the attempts made by dissolving in the smallest possible quantity of absolute alcohol, and adding æther. As the properties of the compound did not appear promising, no further experiments were made with it; but it was converted into a platinum salt, for the purpose of ascertaining whether the collidine had actually combined with æthyle. The process employed was the same as that used for preparing the æthylopicoline salt. A sparingly soluble and scarcely crystalline compound was obtained, the platinum of which was determined by the following experiment:—

5·855 grs. of the platinum salt gave 1·618 gr. platinum.

	Experiment.	Calculation.		
Carbon . .	...	34·06	C^{20}	120
Hydrogen .	...	4·50	H^{16}	16
Nitrogen .	...	3·68	N	14
Chlorine .	...	29·98	Cl^3	106·5
Platinum .	27·65	27·78	Pt	98·7
		100·00		355·2

This corresponds completely with the platinum salt of æthylocollidine, but as that substance did not appear likely to give results of interest, I contented myself with this experiment as a sufficient proof of its existence.

The experiments described in the preceding pages sufficiently establish the fact that picoline and its homologues must be considered as nitryle bases, that is to say, bases capable of taking up only one additional atom of æthyle or any similar radical, by doing which they are converted into fixed compounds, of the class designated ammonium bases. If this be their constitution, we must, according to the views at present entertained, assume that these bases are formed from ammonia by the replacement of its three atoms of hydrogen by as many different radicals. Of the exact nature of these radicals, the experiments we at present possess afford no data for drawing definite conclusions; but a moment's consideration suffices to show that they must be substances remarkable for the simplicity of their constitution. If we confine our attention to pyridine, as the fundamental member of the series, it is obvious that the ten equivalents of carbon and five of hydrogen which it contains must be distributed among these three substances; and although we cannot, without further researches, determine how they are distributed, it is at least sufficiently obvious that the choice among different speculative arrangements is by no means large. In fact, our knowledge of the laws governing the constitution of organic compounds, enables us to see that the total number of possible permutations[†] of the elements of pyridine is only eight. They are as follows:—

$$\left.\begin{array}{l}C^4\,\overset{*}{H}{}^2\\ C^4\,H^2\\ C^2\,H\end{array}\right\}N \qquad \left.\begin{array}{l}C^4\,H^3\\ C^4\,H\\ C^2\,H\end{array}\right\}N \qquad \left.\begin{array}{l}C^4\,H\\ C^4\,H\\ C^2\,H^3\end{array}\right\}N \qquad \left.\begin{array}{l}C^4\,H\\ C^4\,H^2\\ C^2\,H^2\end{array}\right\}N.$$

$$\left.\begin{array}{l}C^6\,\overset{*}{H}{}^2\\ C^2\,H^2\\ C^2\,H\end{array}\right\}N \qquad \left.\begin{array}{l}C^6\,\overset{*}{H}{}^3\\ C^2\,H\\ C^2\,H\end{array}\right\}N \qquad \left.\begin{array}{l}C^6\,H\\ C^2\,H\\ C^2\,H^3\end{array}\right\}N \qquad \left.\begin{array}{l}C^6\,H\\ C^2\,H^2\\ C^2\,H^2\end{array}\right\}N.$$

Involving the existence of the following nine radicals, all, with the exception of methyle, at present unknown:—

$$\begin{array}{lll}C^6\,H^3 & C^4\,H^3 & C^2\,H^3\\ C^6\,H^2 & C^4\,H^2 & C^2\,H^2\\ C^6\,H & C^4\,H & C^2\,H\end{array}$$

Of these, two at least, $C^6\,H$ and $C^4\,H$, are so extremely improbable, that we may, without much hesitation, pronounce against them; and if so, the probable formulæ of pyridine are reduced to those marked with an asterisk. The question for consideration is, whether even these can be supposed to represent the constitution of the base in a feasible manner. On this point no experimental evidence can at present be adduced; but taking into account all the circumstances connected with them, my impression is, that none of them give the rational expression of its constitution, and that pyridine and its homologues belong to a class of bases of which we have as yet no other examples.

[†] I assume, with Gerhardt, that the number of atoms of carbon in any radical must always be divisible by two.

In illustration of this opinion, it is necessary to enter into some details regarding the constitution of the bases generally. It is scarcely necessary to remind the reader that when Hofmann described his two new series of volatile alkaloids, he applied to those already known the name of amide, and to the new series those of imide and nitryle bases. This nomenclature, which has been more than once employed in the preceding pages, was founded on the analogy in constitution of those substances with the well-known amides, imides, and nitryles. A very little consideration, however, suffices to show that this analogy is by no means complete. The first series of bases may be correctly compared to the amides, but the other two have no close resemblance to the imides and nitryles. On the contrary, they are strictly comparable with the secondary and tertiary amides recently described by Gerhardt and Chiozza, which are formed from the primary amides by a process similar to that employed by Hofmann to produce his two classes of bases. The closeness of this analogy may be seen from the subjoined comparison of these methyle bases with the benzoyle amides.

$$\begin{array}{ccc}
\text{Methylamine.} & \text{Bimethylamine.} & \text{Trimethylamine.} \\
\left.\begin{array}{r} C^2H^3 \\ H \\ H \end{array}\right\}N &
\left.\begin{array}{r} C^2H^3 \\ C^2H^3 \\ H \end{array}\right\}N &
\left.\begin{array}{r} C^2H^3 \\ C^2H^3 \\ C^2H^3 \end{array}\right\}N \\
\left.\begin{array}{r} C^{14}H^5O^2 \\ H \\ H \end{array}\right\}N &
\left.\begin{array}{r} C^{14}H^5O^2 \\ C^{14}H^5O \\ H \end{array}\right\}N &
\left.\begin{array}{r} C^{14}H^5O^2 \\ C^{14}H^5O^2 \\ C^{14}H^5O^2 \end{array}\right\}N \\
\text{Primary benzamide.} & \text{Secondary benzamide.} & \text{Tertiary benzamide.}
\end{array}$$

From which we see that in every case hydrogen is replaced, atom for atom, by a compound radical, the only difference being, that in the one set of substances the ammonia retains, in the other it loses its basic properties.

But the constitution of an imide or a nitryle is materially different. Of the former, indeed, we know too little to admit of any satisfactory conclusions regarding their constitution; but taking benzonitryle with the formula $C^{14}H^5N$, as an example of its class, and examining its constitution in the same point of view, we may consider it as an ammonia, in which three atoms of hydrogen have been replaced by a single radical $C^{14} H^{5}$. While, therefore, an amide is formed by the replacement of one or more atoms of hydrogen in ammonia by an equal number of molecules of a monobasic radical, a nitryle may be viewed as an ammonia with its three atoms of hydrogen replaced by one atom of a tribasic radical; and in the same manner there must exist a class of compounds, which for the present we may call imides, although they are not comparable with the substances known under that name, in which part of the hydrogen is replaced by a bibasic radical. The different forms of combination possible under this view may be best rendered intelligible if we make use of general formulæ, and take X', X'', and X''' as representing respectively a monobasic, a bibasic, and a tribasic radical. We have then the following expressions for the different classes:—

(1.)

$$\left.\begin{matrix} X' \\ H \\ H \end{matrix}\right\} N$$

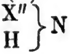
(2.)

$$\left.\begin{matrix} X' \\ X' \\ H \end{matrix}\right\} N$$

(3.)

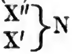
$$\left.\begin{matrix} X' \\ X' \\ X' \end{matrix}\right\} N$$

(4.)



$$\left.\begin{matrix} X'' \\ H \end{matrix}\right\} N$$

(5.)



$$\left.\begin{matrix} X'' \\ X' \end{matrix}\right\} N$$

(6.)

$$X'''\} N$$

Of these the first three represent either the amides, or the bases described by Wurtz and Hofmann; the last is a nitryle, and the others are substances at present scarcely known.

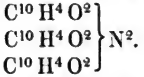
Now as regards the first three classes, it is manifest that they prove amides or bases, according to the properties of the radicals replacing the hydrogen; and we may fairly argue from analogy that the members of the last may be also either basic or non-basic. The nitryles at present known are all non-basic, but it is my belief that the most probable explanation of the constitution of the bases of the pyridine series is to suppose that they are true basic nitryles, and that, for instance, in pyridine itself, the tribasic radical $C^{10} H^{5}$ replaces three atoms of hydrogen in ammonia. The opinion thus expressed regarding the constitution of these bases, and even the possibility of such compounds existing, is speculative, but at the same time it is not altogether unsupported by facts, for though we have no bases in which a tribasic radical exists, there certainly are instances in which two atoms of hydrogen are replaced by a bibasic radical. A marked example is found in Gerhardt's platinamine, although there the replacing substance is not a compound but a simple radical. Its formula may be written thus:—

$$\left.\begin{matrix} Pt \\ H \end{matrix}\right\} N,$$

in which platinum is a bibasic radical replacing two equivalents of hydrogen. Diplatinamine may in the same manner be represented, with its formula written thus:—

$$\left.\begin{matrix}Pt\\H^4\end{matrix}\right\}N^2,$$

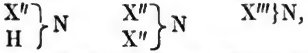
in which two equivalents of ammonia have been brought into play. Lastly, in furfurine we have a purely organic base, formed from two equivalents of ammonia by replacement of the whole of its hydrogen by three atoms of a compound radical, its formula being—



$$\left.\begin{matrix}C^{10}H^4O^2\\C^{10}H^4O^2\\C^{10}H^4O^2\end{matrix}\right\}N^2.$$

The view now expressed would make the constitution of the bases correspond very closely with that of the acids, as explained by Gerhardt. According to that chemist, a monobasic acid is formed from one atom of water (viewed as H^2O) by replacement of hydrogen by a monobasic radical, while a bibasic acid is formed from two atoms of water, by the replacement of two atoms of hydrogen by a bibasic radical.

I have been led into these observations by a desire to explain in a more satisfactory manner than our present knowledge of the bases will permit, the constitution of pyridine and its homologues; but I am now about to enter upon a series of experiments, with a view of obtaining some of the bases



$$\left.\begin{matrix}X''\\H\end{matrix}\right\}N \qquad \left.\begin{matrix}X''\\X''\end{matrix}\right\}N \qquad X'''\}N,$$

the probable existence of which I have now indicated on theoretical grounds, which may probably form the subject of a future communication.

I may further mention, that I have found that the platinum salts of pyridine and picoline undergo a peculiar decomposition when boiled, platinum bases of very remarkable constitution being formed. I am extending this investigation to the other bases, and hope that my experiments will, at no distant date, be sufficiently advanced for publication.

XXXII. *On a more convenient form of applying Gallic Acid as a Developing Agent in Photography. By* WILLIAM CROOKES, *Esq.*[*]

AMONG the more troublesome operations occurring in the practice of photography on paper, when pursued on any extensive scale, is the frequent preparation of large quantities of gallic-acid solution. I believe Mr. Spiller[†] was the first to suggest a means by which this difficulty may be practically overcome: he recommends the addition of a small proportion of alcohol or acetic acid to the aqueous solution, which will so arrest decomposition as to render it possible to keep in stock a quantity of this reagent, as we should of any other stable preparation.

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[*] Communicated by the Author.

[†] Journal of the Photographic Society, vol. ii. p. 56.

Having undertaken the arrangement of the department in this Observatory in which photography is applied to the registration of the various meteorological phænomena, and having for many reasons decided upon the use of the wax-paper process* as that best adapted to the purpose, I have naturally been anxious to avail myself of any improvements which may tend to diminish the labour or contribute to the general perfection of the result.

At first the solution of gallic acid for developing the latent image was prepared in large quantities at a time, and preserved from decomposition by the process just referred to; but the amount of liquid necessary to be prepared at once, in order to last any length of time, proved to be so bulky, that I was induced to try whether the method could not be so far modified as to admit of the water being entirely left out of the stock solution, and only added as required for use. This led to the employment of alcohol as a solvent for the gallic acid; and finding that under these conditions the character of the latter as a developing agent was greatly heightened, I resolved to make a few experiments on the subject, the result of which I beg leave to communicate, in the hope that the particulars will be found useful to others similarly situated to myself.

Two ounces of gallic acid are to be dissolved in 6 ounces of alcohol (60° over proof); to hasten solution, the flask may be conveniently heated by immersion in hot water; when cold, it should be filtered, mixed with half a drachm of glacial acetic acid, and preserved in a stoppered bottle for use; so prepared, it will keep unaltered for a considerable length of time. The gallic acid is not precipitated from this solution by the addition of water; consequently, if in any case desirable, the development of a picture may be effected with a much stronger bath than the one usually employed.

To obtain a solution of about the same strength as a saturated aqueous solution, half a drachm of the above would require to be added to 2 ounces of water; but for my particular purpose I prefer a weaker bath, which is prepared by mixing half a drachm with 10 ounces of water. In either case it will be found necessary to add solution of nitrate of silver in small quantities as the developing picture seems to require it.

Radcliffe Observatory, Oxford,
February 1855.

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* Notes and Queries, vol. vi. p. 443.

XXXIII. *On the Gold Fish of Franklin.*

IN a note at the foot of page 250 of the 37th volume of the Philosophical Magazine, a question of interest both in a mechanical and electrical point of view is suggested by the following statement of Prof. W. Thomson :—"*The phænomenon of a solid body,*" writes Mr. Thomson, "*hovering freely in the air, in stable equilibrium, without any external support, has never, I am convinced, been witnessed as the result of any electrical or magnetical experiment.*" An experiment described by Franklin, and redescribed by Prof. Srtsczek of Pesth*, without any knowledge of what Franklin had done previously, seems to furnish the precise fact alluded to by Prof. Thomson. If the knob of a charged Leyden jar be caused to approach a number of scraps of gold-leaf, the consequent jumping of the leaves is well known. If a certain shape be imparted to the gold-leaf, when the knob is approached the leaf is attracted and moves towards the knob; but, before it reaches the latter, it is arrested, and hovers in the air, like a fish in water. When the atmosphere is dry, the leaf can be preserved swimming for hours together "without any external support or constraint."

The following description of the experiment by Prof. Srtsczek is taken from Poggendorff's *Annalen,* vol. lxxxviii. p. 493.

Let a small Leyden jar which can be held conveniently in the hand be charged with, say positive, electricity. Let the knob be gradually brought near to several bits of gold-leaf lying upon clean paper, from 6 to 12 lines in length, and from a line to a line and a half in width: they may be rectangular, lozenge-shaped, trapezium-shaped or triangular.

A jumping of the leaves immediately commences, but soon the remarkable fact will be witnessed that one or more of the scraps will suddenly halt in their movement, and remain freely floating in the air; sometimes rotating round the knob of the jar, and showing a tendency to rotate round their own axes.

The knob must reach several inches above the insulated rim of the jar, so that scraps which hover at a considerable distance may not be attracted by the rim. At the conductor of a machine the swimming of the gold-leaf is also seen at a much greater distance (a foot for example), but the experiment does not succeed so quickly. The leaves must be smooth, and of an elongated shape.

With the same strength of charge, portions of gold-leaf, equally long, but of different shapes, hover at different distances from the knob.

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* And, we believe, exhibited in a modified form by Prof. Faraday in his public lectures.

With the same charge, and scraps tolerably alike in shape, the long ones hover at greater distances than the short ones. This is most plainly seen when the experimenter succeeds in causing several scraps to swim at the same time.

When the charge diminishes in intensity, the leaves slowly approach the knob of the jar; when, however, by means of the knob of a second jar, electricity is communicated to the former, the leaves again recede. In this manner, when the air is dry, a scrap of gold-leaf can be kept swimming for hours.

In some cases the rotation first begins when the floating leaf comes near to the jar, and the rotation is accelerated as the leaf approaches.

With scraps of a suitable shape, the equilibrium is so stable that the upright jar may be suddenly pulled downwards, without altering the hovering condition of the scrap of gold-leaf.

From the side of the leaf most distant from the jar electricity is given off of the same name as that wherewith the jar is charged; and it has the power of propagating itself to a considerable distance. When a gold-leaf electroscope is placed near the "fish," a permanent divergence is observed after the instrument has been removed.

When the swimming leaf is removed from the neighbourhood of the knob, by suddenly withdrawing the jar, it is found that those pieces which swim at about an inch distance are in a neutral condition; while those which swim at a less distance are negative. Only such as, while swimming, come very near the knob of the jar, show sometimes positive electricity.

In a note to this paper Prof. Poggendorff remarks:—"Although the more recent treatises on electricity do not mention the fact above described, still it is not new, the experiment having been made by Franklin in 1749 on the conductor of an electric machine (see Experimental Observations on Electricity, &c. London, 1769, p. 72). The free hovering of a piece of gold-leaf, in so stable a position, must always be regarded as a most remarkable fact, which is deserving of revival, particularly as very few appear to be acquainted with it."

In the following volume of the *Annalen*, p. 164, Prof. Riess makes the following remarks:—"A bit of gold-leaf, *differently* pointed at its ends, and with its blunter end towards the conductor of an electric machine, flies towards the latter, and remains floating at a distance from it, because the electric wind generated at the blunter end drives it back, while that generated at the sharper end and the electric attraction drive it towards the conductor. A point on the side of the strip of gold-leaf must cause an axial rotation. The same action may be observed with a sewing-needle which has its eye broken off. When it is suspended horizontally and the knob of a Leyden jar is carefully brought near it, at a certain distance from the knob the pointed end is strongly repelled, while the blunt end is attracted."

We may add to these remarks, that in some cases we have succeeded in causing a bit of gold-leaf to flutter so speedily as almost to obscure its shape, and give it the appearance of an insect busily plying its wings. When the hand is approached, the fluttering ceases, and the little swimmer floats tranquilly in the atmosphere. So strong is the electric wind proceeding from the floating scrap, that it can be distinctly felt, as a cold draft, at a distance of 5 or 6 inches.

XXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 156.]

Nov. 16, 1854.—Colonel Sabine, R.A., V.P. and Treas., in the Chair.

THE following communications were read:—

"Observations on the Respiratory Movements of Insects." By the late William Frederick Barlow, F.R.C.S. Arranged and communicated by James Paget, F.R.S.

This essay contains the greater part of a series of observations made between 1845 and 1850. The following are some of the conclusions which they plainly indicate:—

(1.) The respiratory movements of Dragon-flies (Libellulæ), and, probably, of other insects also, are naturally subject to considerable and frequent variations in force and rate, the causes of many of these variations being as yet unknown.

(2.) The respirations of these insects are always quickened by exercise, emotion, rise of temperature, galvanism, and mechanical irritation; and the last three agents quicken them in the decapitated, as well as in the perfect, insect.

(3.) The respiratory movements of each segment of the trunk are, in some measure, independent of those of the rest, although in the perfect insect they concur in all the segments. They continue to be performed, though feebly and slowly, in separated segments, provided their nervous cords and ganglia are entire: and they may be abolished in single and successive segments by the local action of chloroform.

(4.) The removal of the head, including the supra- and sub-œsophageal ganglia, does not, like the removal of the medulla oblongata of the vertebrate animal, put a stop to the respiratory movements of the insect; but it diminishes their frequency and force, and deprives them of all influence of the will and of mental emotions.

(5.) The shock inflicted by the sudden destruction of the head, or of the terminal part of the abdomen, generally stops all the respiratory movements of the insect for a time, and much enfeebles them during the remainder of its life.

(6.) The general tendency of the observations is to corroborate the opinion of the self-sufficiency of the several ganglia for the movements of their appropriate segments, and, thus far, to maintain the belief in their essential independence. At the same time, the observations on the diffused influence of shocks accord with those of the coordinate similar movements of all the segments, in proving their close mutual relations and mutual influence.

Nov. 23, 1854.—Thomas Bell, Esq., V.P., in the Chair.

"On the Theory of Definite Integrals." By W. H. L. Russell, Esq., B.A.

I propose in the following paper to investigate some new methods for summing various kinds of series, including almost all of the more important which are met with in analysis, by means of definite integrals, and to apply the same to the evaluation of a large number of definite integrals. In a paper which appeared in the Cambridge and Dublin Mathematical Journal for May 1854, I applied certain of these series to the integration of linear differential equations by means of definite integrals. Now Professor Boole has shown, in an admirable Memoir which appeared in the Philosophical Transactions for the year 1844, that the methods which he has invented for the integration of linear differential equations in finite terms, lead to the summation of numerous series of an exactly similar nature, whence it follows that the combination of his methods of summation with mine, leads to the evaluation of a large number of definite integrals, as will be shown in this paper. It is hence evident that the discovery of other modes of summing these series by means of definite integrals must in all cases lead to the evaluation of new groups of definite integrals, as will also be shown in the following pages. I then point out that these investigations are equivalent to finding all the more important definite integrals whose values can be obtained in finite terms by the solution of linear differential equations with variable coefficients. Again, there are certain algebraical equations which can be solved at once by Lagrange's series, and by common algebraical processes; the summation of the former by means of definite integrals affords us a new class of results, which I next consider. A continental mathematician, M. Smaasen, has given, in a recent volume of Crelle's Journal, certain methods of combining series together which give us the means of reducing various multiple integrals to single ones. The series hitherto considered are what have been denominated "factorial series;" but, lastly, I proceed to show that analogous processes extend to series of a very complicated nature and of an entirely different form, and for that purpose sum by means of definite integrals certain series, whose values are obtained in finite terms in the "Exercices des Mathématiques" by means of the Residual Calculus. The total result will be the evaluation of an enormous number of definite integrals on an entirely new type, and the application of definite integrals to the summation of many intricate series.

Dec. 7, 1854.—Colonel Sabine, V.P., in the Chair.

The following communications were read:—

"On the Attraction of the Himalaya Mountains, and of the elevated region beyond them, upon the Plumb-line in India." By the Venerable John Henry Pratt, M.A., Archdeacon of Calcutta.

The author commences by observing that it is now well known that the attraction of the Himalaya Mountains, and of the elevated region beyond them, has a sensible influence on the plumb-line in North India. This circumstance was brought to light during the progress of the great trigonometrical survey of that country. It was found by triangulation that the difference of latitude between the two extreme stations of the northern division of the arc, namely Kalianpur and Kaliana, is 5° 23′ 42″·294, whereas astronomical observations show a difference of 5° 23′ 37″·058, which is 5″·236 less than the former.

That the geodetic operations are not in fault, appears from this; that two bases, about 7 miles long, at the extremities of the arc having been measured with the utmost care, and also the length of the northern base having been computed from the measured length of the southern one, through a chain of triangles extending about 370 miles, the difference between the measured and the computed lengths was only 0·6 of a foot, which would produce, even if wholly lying in the meridian, a difference of only 0″·006 in the latitude.

The difference 5″·236 must therefore be attributed to some other cause. A very probable cause presents itself in the attraction of the superficial matter which lies in such abundance on the north of the Indian arc. It is easily seen that this disturbing force acts in the right direction, that is, it diminishes the difference of astronomical latitude between the two stations. Whether the cause here assigned will account for the error in the difference of latitude in *quantity* as well as in direction, is the question which the author proposes to discuss in the present paper.

It might seem at first sight that if mountain attraction were so influential as is here supposed, it would disturb the geodetic operations, since in observing the altitude or depression of one station as seen from another, the error in the plumb-line must come into calculation. The author shows, however, by mathematical calculation, that the effect of mountain attraction on the geodetic operations is perfectly insensible, so that it is clearly the astronomical operation of finding the difference of latitude that requires the correction. This is further apparent from the results obtained by Colonel Everest on attempting to determine the azimuths of the arc at seven stations *astronomically*.

To show the importance of considering mountain attraction in the delicate problem of the figure of the earth, the author investigates the effect of a small error in the difference of latitude of the extremities of an arc on the deduced value of the earth's ellipticity. As two unknown quantities occur in the determination of the spheroid of revolution which most nearly agrees with the earth, namely a the equatorial radius and ϵ the ellipticity, two arcs are required in order to determine them. The author selects the Russian arc, measured near North Cape, as the most advantageous with which to combine the northern portion of the Indian arc, and shows that an error of 5″·236 in defect in the amplitude of the latter would diminish the value of the ellipticity resulting from the two by about the $\frac{1}{25}$th part of the whole. If the effect of mountain attraction be as great as the author calculates it to be, (15''·885 in the northern portion of the Indian arc,) the ellipticity would be diminished by $\frac{1}{8}\epsilon$, and even by as much as $\frac{1}{6}\epsilon$ if the whole Indian arc from Kaliana to Damargida were employed.

The author then proceeds, first to develope his method of calculation, and then to reduce his formula to numbers, according to the best data which he was able to collect.

An expression is first investigated for the horizontal attraction of a prism of the earth's crust standing on a given small base, having a small height, and situated at a given angular distance (measured from the centre of the earth) from the station, A, at which the attraction is sought. In the cases to which this expression is employed it reduces itself without sensible error to

$$\frac{M}{a^2}\cos\left(\frac{1}{2}\theta\right),$$

where M is the attracting mass, a the chord joining its base with A, and θ the angle subtended by this chord at the earth's centre.

In applying this expression to the problem in hand, the author divides the earth's surface into lines, by vertical planes passing through at equal angular distances. These lines are further subdivided by small circles having A for their common pole, and in this manner cutting the whole surface into curvilinear quadrilaterals. He then investigates what the law of dissection must be, that is, according to what law the radii of the small circles must be taken to increase, in order that the horizontal attraction of the portion of the crust standing on one of the quadrilaterals may be equal to the product of its average height and density by a constant quantity, independent of the distance of the quadrilateral from A. If α and $\alpha+\phi$ be the angular radii of two consecutive small circles, there results

$$\frac{\phi\cos^2\left(\frac{1}{2}\alpha+\frac{1}{4}\phi\right)}{\sin\left(\frac{1}{2}\alpha+\frac{1}{4}\phi\right)}=\text{a constant quantity}=c.$$

To fix the value of this constant, the author assumes $\phi=\frac{1}{10}\alpha$ when ϕ and α are indefinitely small, which gives $c=\frac{4}{21}$. The above equation may then be solved numerically with sufficient approximation. In this manner a table is calculated of the radii of the successive small circles.

These distances should be laid down, and the circles drawn, on a map or globe, as well as the lines dividing the surface into lines. Nothing then remains to be done but to ascertain the average heights of the masses standing on the compartments thus drawn.

The author's paper was accompanied by a plate representing an outline of the continent of Asia. On this was laid down a polygonal figure DEFGHIJKL, (which for convenience the author calls the "enclosed space,") marking the boundary of an irregular mass, which is the only part of the earth's surface that appears to have a sensible effect on the plumb-line in India. The boundary of this space is thus defined:—

DEF is the Himalaya range, having a bend at E from north-west on the left, to east-by-south on the right. FG is a range running to the table-land of Yu-nan in lat. 25° and long. 103°. GH is the range of the Yun-Ling mountains, in which there are many peaks of perpetual snow. HI is the Inshan range. IJ is the Khing-khan range, very steep on the east side, not so on the west; the passes are said to be 5525 feet above the sea. JK is the Altai range, the highest peak of which is 10,800 feet; the average height is 6000: the range declines towards the east. KL was once thought to be a range of mountains, but is now found to be a line of broken country. LD is the Bolor range, rising to an elevation similar to that of the Hindoo Koosh. There are besides these two ranges of mountains running into the enclosed space, parallel to the Altai and Southern Himalayas, namely the Thian-Schan range, or Celestial Mountains, and the Kuen-Luen range, being a continuation of the Hindoo Koosh, which rises from an altitude of 2558 feet near Herat to about 20,000 where it meets the Bolor range. It is, however, with the elevation of the enclosed space itself that we are principally concerned, since ranges of mountains have not so important an influence, when distant, as table-lands of elevation.

Before describing the country within these limits, the author gives a general sketch of the parts which lie outside, from which it appears that the calculations may be confined to the enclosed space. He then describes in detail the nature of the country within the boundaries of the enclosed space, commencing with the Himalayas, which rise abruptly from the plains of India to 4000 feet and more, and cover an extensive broken space some 100 or 200 miles wide, rising to great heights; perhaps 200 summits exceed 18,000 feet; the highest reaches to more than 28,000. The general base on which these peaks rest rises gradually to 9000 or 10,000 feet, where it abuts on the great plateau north of the range. The character of the country to the south of this plateau is much better known than that to the north. If a circle with a radius 5°·046 (the value of one of the radii employed in the dissection) be drawn around Kaliana, it will pass over the highest part of this plateau. This circle divides the enclosed space into two portions, of which the southern is called by the author the "Known" and the northern the "Doubtful Region." The effects of the two portions are separated in the calculation by the introduction of an arbitrary factor.

After describing the doubtful region, as far as was possible from the data to which he had access, the author assumes, as the best general representation of the facts, that to the north of a line running through Leh and H'Lassa the doubtful region slopes gradually from 10,000 feet down to 2500 along a parallel line nearly in its centre, and then rises again at the same angle to the north, and that the portion to the south of the line first mentioned, and not included in the known region, slopes at four times that rate.

The author then proceeds to numerical summations replacing an integration to be extended over the whole of the enclosed space. The breadth of the lines employed in the calculation is taken at 30°, which is shown not to be too large to give good results. The following are the results obtained:—

	Arising from		
Station A, Kaliana.	Known region.	Doubtful region.	Total.
Deflexion of plumb-line in meridian	12″·972	14″·881	27″·853
Correction of same for every 100 feet of change in heights	0·312	0·260	
Deflexion of plumb-line in prime vertical	8·136	8·806	16·942
Station B, Kalianpur.			
Deflexion in meridian	3·219	8·749	11·968
Correction for 100 feet	0·059	0·158	
Deflexion in prime vertical	0·789	3·974	4·763
Station C, Damargida.			
Deflexion in meridian	1·336	5·573	6·909
Correction for 100 feet	0·022	0·100	
Deflexion in prime vertical	0·000	2·723	2·723

whence there results,

Total deflexion at A=32″·601, and in azimuth 31° 18′ East.
Total deflexion at B=12·880, and in azimuth 21 42 East.
Total deflexion at C= 7·426, and in azimuth 21 31 East.

Difference of meridian deflexions at A and B=15″·885.
Difference of meridian deflexions at A and C=20·944.
Difference of meridian deflexions at B and C= 5·059.

The first of these differences is considerably greater than 5″·236, the quantity brought to light by the Indian Survey.

The author then examines these values more minutely, and considers the effect of various hypotheses for reducing them.

In the first place, the density of the attracting mass may have been assumed too large. The density assumed is 2·75 that of distilled water, the value assumed as the mean density of the mountain Schehallien in the calculations of Maskelyne. This can hardly be too great, but at any rate no remarkable supposition relative to the density can reduce the attraction by more than a small fraction of the whole.

Next, the mass of the doubtful region may have been assumed too great. This hypothesis is then examined by the author, who concludes that even the extravagant supposition of the non-existence of that region will not reduce the difference of meridian deflexions at A and B lower than to 9″·753.

A third means of reduction may be looked for in the known region. A large part of the attraction belonging to this region arises from the Great Plateau. It would be necessary to cut down this plateau as much as 6000 feet to reduce the deflexions at A and B to $5''\cdot236$, even were the whole mass on the doubtful region non-existent; so that it appears to be quite hopeless, by any admissible hypothesis relative to heights, densities, &c., to reduce the calculated deflexion so as to make it tally with the error brought to light by the survey.

After entering into some elaborate calculations confirmatory of the previous results, the author concludes by calculating the form of the Indian arc, that is, by determining what spheroid of revolution,—the axis of revolution being the earth's axis,—would most nearly coincide with that arc without reference to the rest of the earth, the data employed being the lengths and amplitudes of the northern and southern portions of the arc, and of course their sum, and likewise the latitudes, or at least approximate latitudes, of the middle points of the arcs. By using the amplitudes uncorrected for mountain attraction, the author obtains for the value of the ellipticity deduced from the Indian arc alone $\frac{1}{196\cdot3}$, nearly agreeing with $\frac{1}{191\cdot6}$, which is Col. Everest's result; but by using the amplitudes corrected for mountain attraction according to the author's calculation, the ellipticity is reduced to $\frac{1}{426\cdot2}$. He concludes that the arc is more curved than it would be if it had the mean ellipticity of the earth, and regards the supposition of a general deviation of the earth's surface in that region from the mean spheroidal form as the most satisfactory mode of accounting for the discrepancy.

"On the Value of Steam in the Decomposition of neutral Fatty Bodies." By George Wilson, Esq.

In the course of a long series of experiments conducted on a large scale, the author has observed that the so-called neutral fatty bodies may be resolved, without danger of injurious decomposition, into glycerine and fatty acids, provided the still is maintained at a uniform high temperature, and that a continuous current of steam is admitted into it.

The temperature required to effect the splitting of the fats into their proximate elements varies with the nature of the body itself, but all hitherto tried may be resolved into glycerine and fatty acid at a temperature of 560° Fahr., many at much below that temperature. At a further period it is the author's intention to lay before the Society a detailed account of his experiments, with the confirmatory analyses, but in the mean time he states that palm oil, cocoa-nut oil, fish oil, animal tallow, Bornean vegetable tallow, "Japan vegetable wax" (more properly tallow), and several others have yielded satisfactory results, the fatty acid and glycerine distilling over together, but no longer in combination, and separating in the receiving vessel.

Dec. 14, 1854.—The Lord Wrottesley, President, in the Chair.

"On the Structure of some Limestone Nodules enclosed in Seams of Bituminous Coal, with a Description of some Trigonocarpons contained in them." By J. D. Hooker, M.D., F.R.S., and E. Binney, Esq.

The authors first describe the occurrence of the limestone nodules, which form a continuous bed in the centre of a thin seam of bituminous coal in the lower part of the Lancashire coal-field. The nodules were of various sizes, some weighing many pounds, and caused the coal to bulge out both above and below them, and they were found to be entirely composed of vegetable tissues converted into carbonate of lime and magnesia. Their formation is supposed by the authors to be due to infiltration of water through the superincumbent shales, which were full of fossil shells supposed to be of marine origin, and the aggregation of the mineral matter round centres of vegetable remains. The chemical constituents of the nodules were found to be carbonates of lime and magnesia, sesquioxide and sulphate of iron, with a little carbonaceous matter.

The probability of these nodules representing an average sample of the vegetable constituents of the surrounding coal is then discussed, and attention is drawn to the very great interest and importance that would attach to them were such a view substantiated, as showing the exact nature of the association of plants which is capable of conversion into bituminous coal.

All the plants contained in the nodules were common in other parts of the coal formation, viz. *Calamodendron*, *Halonia*, *Sigillaria*, *Lepidodendron*, *Stigmaria*, *Trigonocarpon*, *Anabothra*, and others; of these the first-named genus occurred in the greatest abundance and as large fragments of fossil wood. Very many of the specimens were sliced, and being reduced to very thin transparent sections, were examined with the view of determining the botanical character of their contents, and the intimate structure of the masses of more or less homogeneous aspect to which they were reduced by decomposition, previous to or during the operation of calcification. The results were very satisfactory, and seemed to indicate that all traces of vegetable structure may be completely obliterated in the substance of highly bituminized coal, which may nevertheless also contain fragments of wood with their tissues preserved.

An account is then given of the examination of the details of structure of *Trigonocarpon*, and this, as well as the comparison of *Trigonocarpon* with the modern genus *Salisburia*, is illustrated by drawings and analyses.

The authors are still engaged with the study of these nodules, with the view of showing the relationship between *Calamodendron*, *Calamites*, *Sigillaria* and *Anabothra*, and the details are preparing for publication.

XXXV. *Intelligence and Miscellaneous Articles.*

OBJECT OF SALT IN THE SEA.

At a recent Meeting of the Canadian Institute, a very interesting paper was read by Professor Chapman, of University College, Toronto, on the "Object of the salt condition of the Sea." Professor Chapman began by assuming that the sea was created salt from the beginning, and for some beneficent purpose; and then proceeded to discuss the views hitherto advanced in elucidation of this object. The suggestion, that the sea is salt in order to preserve it in a state of purity, was considered to be untenable for several reasons; mainly, however, from the fact, that organic impurities when diffused through a vast body of moving water, whether fresh or salt, become altogether, and very rapidly lost; so much so, indeed, as apparently to have called forth a special agency to arrest the total annihilation of organized matter in its final oscillation between the organic and inorganic worlds. The author alluded to the countless hosts of microscopic creatures which swarm in most waters, and whose principal function has been ably surmised by the great anatomist, Professor Owen, to be that of feeding upon, and thus restoring to the living chain, the almost unorganized matter diffused through their various zones of habitation. These creatures preying upon one another, and being preyed upon by others in their turn, the circulation of organic matter is kept up, and carried through its appointed rounds. If we do not adopt this view, we must at least look upon the Infusoria, the Foraminifera, and many of the higher types, as scavenger agents appointed to prevent an undue accumulation of decaying matter; and thus, as before, the salt condition of the sea does not become a necessity. It was shown also, that, under many circumstances, the amount of saline matter in the sea is not sufficient to arrest decomposition. Other suggestions, to the effect that the sea is salt in order to render it of greater density, and by lowering its freezing-point to preserve it from congelation to within a shorter distance of the poles, were then discussed in their more prominent relations. The value of these suggestions in a secondary point of view was fully admitted, but shown, at the same time, to be scarcely adequate to meet the entire solution of so vast and grand a problem as that which is manifestly involved in the salt condition of the sea. The freezing-point of sea water, for instance, is only 3½° F. lower than that of fresh water; and hence with the present distribution of land and sea,—and still less, probably, with that which obtained in former geological epochs—no very important effects would have resulted had the ocean been fresh instead of salt. So far as regards the habitable portions of the world, the present difference would be next to nothing. Professor Chapman here submitted to the Institute a suggestion which he believed to be original, in reference to the question under discussion. He considered the salt condition of the sea as mainly intended to *regulate evaporation*, and to prevent an undue excess of that phænomenon under the influence of any disturbing causes that might from time to time arise. It is well known that under the same atmospheric pressure different liquids have very different boiling-points; and in like manner, saturated solutions evaporate more slowly than weak ones, and these latter more slowly again than pure water. In sea water we have on an average about 3½ per cent. of solid matter, of which about 2·6 consists of chloride of sodium or common salt. The results of certain experiments by the author in reference to evaporation on weighed quantities of ordinary rain-water, and water holding in solution 2·6 per cent. of salt, were then given. The excess of loss of the rain-water over the water of the salt solution was, for the first twenty-four hours, 0·54 per cent.; at the close of forty-eight hours, 1·04 per cent.; after seventy-two hours, 1·46 per cent., and so on, always in an increasing ratio; the experiments in each case being carried on for six days.

Here, then, we have a self-adjusting phænomenon, one of those admirable contrivances in the balance of forces, which an attentive study of nature reveals to us in every direction. If, other conditions being equal, any temporary cause render the amount of saline matter in the sea above its normal value, evaporation goes on the more and more slowly; and, on the other hand, if this value be depreciated by the addition of fresh water in undue excess, the evaporating power is the more and more increased—thus aiding time, in either instance, to restore the balance. In conclusion, the author pointed out that the consideration of this principle might shed some further light on the geographical distribution of fresh and salt water lakes on the present surface of the globe.

ON AN APPEARANCE SEEN IN THE MOON.

BY ROBERT HART, ESQ.

(Letter to the Astronomer Royal.)

"On the night of the 27th December, 1854, between 6 and 7 P.M., the moon was very bright. I had brought my 10-inch reflector to bear upon the moon; on the shaded side of the disc I observed a white spot, where I have marked it on the sketch. As it was of the colour of the light of the moon, and not like starlight, I thought it part of the moon; but as it disappeared in less than a minute after I first observed it, I concluded it was a star eclipsed by the moon*. I now turned my attention to the light part of the disc, and my eye was at once attracted by an appearance I had never seen before on the surface of the moon, although I have observed her often during these last forty years. She was 8^d 4^h old at the time, and just on the edge of the light, where I have marked on the sketch, there were *two luminous spots*, one on either side of a small ridge, *which ridge was in the light*, and of the same colour as the moon; but these spots were of *a yellow flame colour*, while all the rest of the enlightened part was of a snowy white, and the mountain-tops that were coming into the light, *and just on the shadow side of these spots, were of the same colour as the moon*. The lights of these spots were *like the light of the setting sun reflected from a window a mile or two off*. I observed it for five hours. I thought them rather less bright than as first seen, *but very little less*; so bright were they, when the instrument was the least thing out of focus, they showed *rays around them as a star would do*.

"As I live about two miles out of Glasgow, I had no scientific friend with me at the time, but I called the attention of three gentlemen, my neighbours, and my own household, and they all described the appearance as I saw it myself, and have given above.

"I would have followed it longer had I been able, but the wind was very cold, 20°·5, and I had no shelter, as I take my telescope outside. I left off about 12 P.M.; and from the cloudy weather, I never saw the moon until she was nearly full, then only a few minutes, and again on the 10th January, at 4 A.M.; but the haze was too thick. I thought I might see them in the shade, but I could not: I have never seen her since.

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* This was, no doubt, the occultation of μ Piscium, which, according to the Nautical Almanac, was in conjunction with the moon at 6^h 54^m 10^s, on the evening of December 27, 1854.

"May I ask the favour of your laying this before the Astronomical Society, that we might learn if any of the members have observed this appearance in the moon, or have heard that it was seen by others. I shall be happy to learn if it has been taken notice of by any other observer, and their opinion of it.

"It appeared to me, from the brightness of the light and the contrast of colour, *to be two active volcanos or two mouths of one in action*.

"If there was no star eclipsed by the moon at or about 7 P.M. that night, the white spot that was more like one of the mountain-tops tipped with light than a star, may have had some connexion with it."—*Proc. Roy. Astron. Soc.* 1855, p. 89.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1855.

Chiswick.—Jan. 1. Cloudy: boisterous. 2. Densely clouded: slight rain: overcast and mild. 3. Very fine. 4. Overcast. 5. Very fine. 6. Overcast. 7. Densely clouded. 8. Overcast. 9. Fine: slight rain. 10. Foggy: very dense fog at night. 11. Foggy. 12. Foggy: hazy clouds. 13. Overcast: fine: cloudy: clear and frosty. After unusually fine dry weather, a most severe period now commences. 14. Fine: cloudy: clear with sharp frost. 15. Hazy: frosty. 16. Slight rain: frosty. 17. Slight snow: clear and frosty. 18. Frosty: clear: snow. 19. Sharp frost: uniformly overcast: clear and frosty. 20. Cloudy: snowing. 21. Sharp frost: frosty haze. 22. Snowing: hazy. 23. Snowing: overcast. 24. Overcast: frosty. 25. Fine: frosty. 26. Hazy. 27. Foggy: fine: frosty. 28. Overcast. 29. Snow showers. 30. Snowing. 31. Overcast: windy: drifting snow.

Mean temperature of the month	33°·45
Mean temperature of Jan. 1854	37 ·88
Mean temperature of Jan. for the last twenty-nine years ...	37 ·06
Average amount of rain in Jan.	1·74 inch.

Boston.—Jan. 1. Cloudy: stormy. 2. Cloudy. 3. Fine. 4—9. Cloudy. 10. Fine. 11, 12. Cloudy. 13. Cloudy: rain P.M. 14—16. Cloudy. 17. Cloudy: snow A.M. 18—20. Cloudy. 21, 22. Cloudy: snow A.M. and P.M. 23. Cloudy: snow A.M. 24—26. Cloudy. 27. Fine. 28. Cloudy. 29. Fine: snow P.M. 30, 31. Cloudy.

Sandwick Manse, Orkney.—Jan. 1. Sleet-showers A.M.: snow-showers, frost P.M. 2. Cloudy A.M.: fog P.M. 3. Drizzle A.M.: drizzle, showers P.M. 4. Bright A.M.: rain P.M. 5. Showers A.M.: showers, thunder and lightning P.M. 6. Showers, bright A.M.: showers, thunder and lightning P.M. 7. Drops A.M.: drizzle P.M. 8. Rain A.M. and P.M. 9. Clear A.M. and P.M. 10. Clear, frost A.M.: rain P.M. 11. Drizzle A.M.: cloudy P.M. 12. Cloudy A.M. and P.M. 13. Cloudy, fine A.M.: fog P.M. 14, 15. Cloudy A.M. and P.M. 16. Showers, bright A.M.: cloudy P.M. 17. Clear, frost A.M.: fine, rain P.M. 18. Frost, showers A.M.: damp P.M. 19. Drizzle A.M.: drizzle, showers P.M. 20. Damp, fine A.M.: fine, clear P.M. 21. Cloudy A.M.: fine, cloudy P.M. 22. Showers A.M.: sleet P.M. 23. Bright, frost A.M.: clear, frost P.M. 24. Bright, frost A.M.: fine, frost P.M. 25. Cloudy A.M.: rain P.M. 26. Cloudy A.M.: snow-showers P.M. 27. Bright, frost A.M.: snow-showers P.M. 28. Snow-showers A.M. and P.M. 29—31. Snow, clear A.M.: frost, clear P.M.

Mean temperature of Jan. for twenty-eight previous years ...	38°·38
Mean temperature of this month	38 ·16
Mean temperature of Jan. 1854	36 ·46
Average quantity of rain in Jan. for fourteen previous years .	4·38 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1835. Jan.	Max.	Min.		9½ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·951	29·919	29·44	29·31	29·73	53	46	52	36½	34½	w.	wnw.	nw.	·02	....	·37
2.	30·069	30·028	29·72	29·88	29·88	51	44	43	36	39	nw.	nw.	sw.	·01	....	·02
○ 3.	30·170	30·139	29·74	29·83	29·79	52	39	44	46	46½	nw.	wnw.	sw.	....	....	·11
4.	30·218	30·164	29·72	29·65	29·40	47	38	37	48	48½	sw.	w.	sw.	....	....	....
5.	30·132	30·062	29·66	29·52	29·35	51	40	46	40½	42	sw.	sw.	w.	....	....	·40
6.	30·361	30·240	29·81	29·88	30·01	50	44	48	40½	43	sw.	calm	s.	....	....	·27
7.	30·530	30·428	30·00	29·86	29·99	49	42	45	48	46½	sw.	calm.	sw.	....	....	·20
8.	30·464	30·303	30·00	29·77	29·78	48	41	45	47½	41½	sw.	w.	nw.	....	....	·48
9.	30·405	30·283	29·83	30·15	30·38	50	28	45	37	38	w.	wnw.	w.	·02	....	·36
10.	30·525	30·495	30·20	30·36	30·28	39	27	28	36	39½	ne.	wnw.	s.	....	....	....
☾ 11.	30·525	30·457	30·16	30·19	30·44	38	29	35	48	41½	ne.	sw.	nw.	....	....	·09
12.	30·543	30·519	30·22	30·55	30·58	41	32	32	41½	41½	ne.	nw.	wnw.	....	....	....
13.	30·536	30·432	30·22	30·54	30·51	42	26	34	43½	34	ne.	nw.	e.	....	....	....
14.	30·468	30·434	30·15	30·47	30·47	41	19	33	43	43	ne.	n.	w.	....	·03	....
15.	30·456	30·223	30·08	30·34	30·31	35	21	34	44	38	w.	nw.	n.	....	....	·21
16.	30·081	30·017	29·77	30·28	30·35	40	22	35	37	38	n.	nw.	ne.	·01	....	·08
17.	30·077	30·011	29·90	30·44	30·36	32	20	28	35	38	ne.	ene.	nw.	....	....	....
● 18.	30·118	29·955	29·87	30·23	30·19	34	18	28·5	39	44	ne.	nnw.	n.	....	·03	·06
19.	29·984	29·879	29·73	30·15	30·14	28	13	28	42	40	se.	calm	nne.	....	....	·04
20.	29·839	29·702	29·70	30·17	30·16	31	18	30	38½	37	ne.	ene.	e.	....	....	·02
21.	29·841	29·724	29·65	30·12	30·11	34	17	29	37	37	ne.	s.	e.	....	....	....
22.	29·803	29·762	29·50	29·97	29·91	35	18	30	38	35	sw.	nw.	se.	....	·20	·10
23.	29·868	29·807	29·64	29·95	30·15	34	24	31	36	34	n.	n.	s.	....	·08	·06
24.	30·003	30·000	29·77	30·23	30·26	37	26	34	35	34	ne.	n.	se.	....	·05	....
☽ 25.	30·023	29·991	29·77	30·18	29·98	38	23	33	38	37	nw.	n.	sw.	·04	....	....
26.	30·003	29·871	29·66	30·02	30·12	37	17	32	33	34	sw.	nw.	ene.	....	....	·15
27.	29·972	29·946	29·70	30·22	30·12	34	15	23·5	36	36	ne.	nw.	nw.	....	....	·02
28.	29·934	29·847	29·66	29·88	29·85	36	26	32	33	35	nw.	nw.	calm	....	....	·09
29.	29·730	29·597	29·45	29·84	29·83	35	17	23	28	29	nw.	n.	ne.	....	....	·07
30.	29·718	29·669	29·54	29·85	29·90	33	25	30	29	30	ne.	e.	ese.	....	·04	·06
31.	29·498	29·425	29·40	29·92	29·97	31	23	28·5	28	31	ne.	e.	w.	....	....	....

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

APRIL 1855.

XXXVI. *Elementary Demonstrations of Propositions in the Theory of Magnetic Force. By* Professor W. Thomson*.

Def. 1. THE lines of force due to any magnet or electro-magnet, or combination of magnets of any kind, are the lines that would be traced by placing the centre of gravity of a very small steel needle, perfectly free to turn about this point, in any position in their neighbourhood, and then carrying it always in the direction pointed by the magnetic axis of the needle.

Remark. Except in the cases of symmetrical magnets, the lines of force will generally be lines of double curvature, and no set of them will lie in one plane.

Def. 2. The lines of component force in any plane are the lines traced by placing the centre of gravity of a steel needle anywhere in this plane, and carrying it always in this plane in the nearest direction to that pointed by its magnetic axis; that is, the direction of the orthogonal projection of the magnetic axis on the plane; or the direction that the steel needle would point with its magnetic axis if placed with it in the plane, and free to turn about an axis through its centre of gravity perpendicular to the plane.

Prop. I. If the line of component magnetic force through any point in a plane be curved at this point, the force will vary in a line perpendicular to the line of force in its plane, *increasing* in the direction *towards* the centre of curvature.

Let EABF be a line of component force in the plane of the diagram, and let GCDH be another near it, each and all between them being curved in the same direction, the arrow head on each indicating the way a north pole would be urged. Let AC, BD be lines drawn perpendicular to all the lines of component force between these two. Because of the curvature of these lines, the lines AC and BD (whether straight or curved) must be so inclined to one another that the portion CD cut off from the last shall be less than the portion AB cut off from the first. Let a north pole of an infinitely thin, uniformly and longitudinally magnetized bar, of which the south pole is at a great distance from the magnets, be carried from D to C along the line of component force through these points, from C to A perpendicular to all the lines of force traversed, from A to B again along a line of force, and lastly, from B to D perpendicular to the lines of force. Work must be spent on it in carrying it from C to D, and work is gained in passing it from A to B. Then, because no work is either gained or spent in carrying it from C to A or from B to D, the work gained in moving along AB cannot exceed the work spent in the first part of the motion, or else we should have a perpetual development of energy from no source*, by simply letting the cycle of motion be repeated over and over

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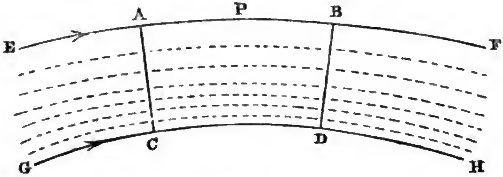
* Communicated by the Author.



again: and the work spent along DC cannot exceed that gained from A to B, or else we might have a perpetual development of energy from no source, merely by reversing the motion described, and so repeating. The work spent and gained in the motions along DC and AB respectively must therefore be exactly equal. Hence the mean intensity of the force along CD, which is the shorter of the two paths, must exceed the mean intensity of the force along the other; and therefore the intensity of the force increases from P in the perpendicular direction towards which the concavity of the line through it is turned.

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* [Note added March 26, 1855.]—It might be objected, that perhaps the magnet, in the motion carried on as described, would absorb heat, and convert it into mechanical effect, and therefore that there would be no absurdity in admitting the hypothesis of a continued development of energy. This objection, which has occurred to me since the present paper was written, is perfectly valid against the reason assigned in the text for rejecting that hypothesis; but the second law of the dynamical theory of heat (the principle discovered by Carnot, and introduced by Clausius and myself into the dynamical theory, of which, after Joule's law, it completes the foundation) shows the true reason for rejecting it, and establishes the validity of the remainder of the reasoning in the text. In fact, the only absurdity that would be involved in admitting the hypothesis that there is either more or less work spent in one part of the motion than lost in the

Prop. II. The augmentation of the component force in any plane at an infinitely small distance from any point, towards the centre of curvature of the line of the component force through it, bears to the whole intensity at this point the ratio of the infinitely small distance considered, to the radius of curvature.

If, in the diagram for the preceding proposition, we suppose AB and CD to be infinitely near one another, and each infinitely short, they will be infinitely nearly arcs of circles with infinitely nearly equal radii. Hence the difference of their lengths must bear to either of them the ratio of the distance between them to the radius of curvature. But the mean intensities along these lines must, according to the preceding demonstration, be inversely as their lengths, and hence the excess of the mean intensity in CD above the mean intensity in AB must bear to the latter the ratio of the excess of the length of AB above that of CD to the latter length; that is, as has been shown, the ratio of the distance between AB and CD to the radius of curvature.

Prop. III. The total intensity does not vary from any point in a magnetic field to a point infinitely near it in a direction perpendicular to the plane of curvature of the line of force through it.

Prop. IV. The total intensity increases from any point to a point infinitely near it in a direction towards the centre of curvature of the line of force through it, by an amount which bears to the total intensity itself, the ratio of the distance between these two points to the radius of curvature.

These two propositions follow from the two that precede them by obvious geometrical considerations.

[They are equivalent to asserting, that if X, Y, Z denote the components, parallel to fixed rectangular axes, of the force at any

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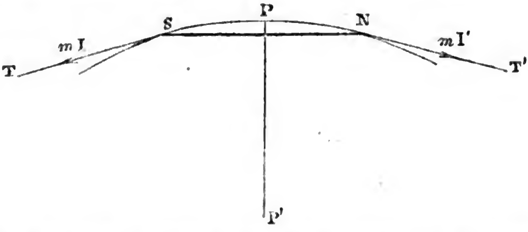
other, would be the supposition that a thermo-dynamic engine could absorb heat from matter in its neighbourhood, and either convert it wholly into mechanical effect, or convert a part into mechanical effect and emit the remainder into a body at a higher temperature than that from which the supply is drawn. The investigation of a new branch of thermo-dynamics, which I intend shortly to communicate to the Royal Society of Edinburgh, shows that the magnet (if of magnetized steel) does really experience a cooling effect when its pole is carried from A to B, and would experience a heating effect if carried in the reverse direction. But the same investigation also shows that the magnet must absorb just as much heat to keep up its temperature during the motion of its pole *with* the force along AB, as it must emit to keep from rising in temperature when its pole is carried *against* the force, along DC.

point whose coordinates are (x,y,z), the expression $Xdx + Ydy + Zdz$ must be the differential of a function of three independent variables.]

Examination of the Action experienced by an infinitely thin uniformly and longitudinally magnetized bar, placed in a non-uniform Field of Force, with its length direct along a line of force.

Let SN be the magnetized bar, and ST, NT′ straight lines touching the line of force in which, by hypothesis, its extremities lie, and P a point on it, midway between them. The resultant force on the bar will be the resultant of two forces pulling its ends in the lines ST, NT′. If these two forces were equal (as they would be if the intensity of the field did not vary at all along a line of force, as for instance when the lines of force are concentric circles, as they are when simply due to a current of electricity passing along a straight conductor; or if P were in a situation between two dissimilar poles symmetrically placed on each side of it), the resultant force would clearly bisect the angle between the lines TS, T′N, and would therefore be perpendicular to the bar and to the lines of force in the direction towards which they are curved; that is (Prop. IV.), would be from places of weaker to places of stronger force, perpendicularly across the lines of force. On the other hand, if the line of force through P has no curvature at this point, or no sensible curvature as far from it as N and S, the lines NT and ST′ will be in the same straight line, and the resultant force on the bar will be simply the excess of the force on one end above that on the other acting in the direction of the greater; and since in this case (Prop. IV.) there is no variation of the intensity of the



force in the field in a direction perpendicular to the lines of force, the resultant force experienced by the bar is still simply in the direction in which the intensity of the field increases, although this is now a direction coincident with a line of force. Lastly, if the intensity increases most rapidly in an oblique direction in the field, from P in some direction between PS and

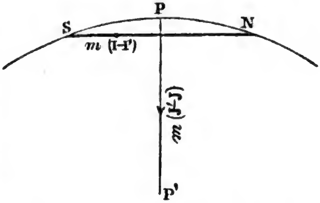
PP′, there must clearly be an augmentation (a "component" augmentation) from P towards P′; and therefore (Prop. IV.) the line through P must be curved, with its concavity towards P′, and also a "component" augmentation from N towards S, and therefore the end S must experience a greater force than the end N. It follows that the magnet will experience a resultant force along some line in the angle SNP′, that is, on the whole from places of weaker towards places of stronger force, obliquely across the lines of force.

Prop V. (*Mechanical Lemma.*) Two forces infinitely nearly equal to one another, acting tangentially in opposed directions on the extremities of an infinitely small chord of a circle, are equivalent to two forces respectively along the chord and perpendicular to it through its point of bisection, of which the former is equal to the difference between the two given forces and acts on the side of the greater; and the latter, acting towards the centre of the circle, bears to either of the given forces the ratio of the length of the arc to the radius.

The truth of this proposition is so obvious a consequence of "the parallelogram of forces," that it is not necessary to give a formal demonstration of it here.

Prop. VI. A very short, infinitely thin, uniformly and longitudinally magnetized needle, placed with its two ends in one line of force in any part of a magnetic field, experiences a force which is the resultant of a longitudinal force equal to the difference of the forces experienced by its ends, and another force perpendicular to it through its middle point equal to the difference between the force actually experienced by either end, and that which it would experience if removed, in the plane of curvature of the line of force, to a distance equal to the length of the needle, on one side or the other of its given position.

NS being the bar as before, let I denote the intensity of the force in the field at the point occupied by N, I′ the intensity at S, J the intensity at P on the line of force midway between S and N, and J′ the intensity at a point P′, at a distance PP′ equal to the length of the bar, in a direction perpendicular to the line of force. Then if m denote the strength of magnetism of the bar, mI and mI′ will be the forces on its two extremities respectively. Hence by the mechanical lemma, the resultant of these forces will be the same as the resultant of a force $m(\mathrm{I}-\mathrm{I}')$ acting along the bar in the



direction SN, and a force perpendicular to it towards the centre of curvature, bearing the same ratio to either mI or mI′, or to mJ (which is their mean, and is infinitely nearly equal to each of them), as NS to the radius of curvature, or (by Prop. II.) the ratio of the excess of the intensity at P′ above that at P to the intensity at either, that is the ratio of J′−J to J, and therefore itself equal to m(J′−J). The bar therefore experiences a force the same as the resultant of m(I−I′) acting along it from S towards N, and m(J′−J) perpendicularly across it towards P′, through its middle point.

Cor. The direction of the resultant force on the bar is that in which the total intensity of the field increases most rapidly; or, which is the same, it is perpendicular to the surface of no variation of the total intensity.

Prop. VII. The resultant force on an infinitely small magnet of any kind placed in a magnetic field, with its magnetic axis along the lines of force, is in the line of most rapid variation of the total intensity of the field, and is equal to the magnetic moment of the magnet multiplied by the rate of variation of the total intensity per unit of distance; being in the direction in which the force *increases* when the magnetic axis is "direct," (that is, in the position it would rest in if the magnet were free to turn about its centre of gravity).

Cor. 1. The resultant force experienced by the magnet will be in the contrary direction, that is, the direction in which the total intensity of the field diminishes most rapidly, when it is held with its magnetic axis reverse along the lines of force of the field.

Cor. 2. A ball of soft iron, or of any non-crystalline paramagnetic substance, held anyhow in a non-uniform magnetic field, or a ball or small fragment of any shape, of any kind of paramagnetic substance whether crystalline or not, left free to turn about its centre of gravity, will experience a resultant force in the direction in which the total intensity of the field increases most rapidly, and in magnitude equal to the magnetic moment of the magnetization induced in the mass multiplied by the rate of variation of the total intensity per unit distance in the line of greatest variation in the field. For such a body in such a position is known to be a magnet by induction, with its magnetic axis direct along the lines of force.

Cor. 3. A ball of non-crystalline diamagnetic substance held anyhow in a magnetic field, or a small bar or fragment of any shape of any kind of diamagnetic substance, crystalline or non-crystalline, held by its centre of gravity, but left free to turn about this point, experiences the same resultant force as a small steel or other permanent magnet substituted for it, and held with its magnetic axis reverse along the lines of force. For Faraday has discovered, that a large class of natural substances in the stated conditions experience no other action than *a tendency from places of stronger towards places of weaker force, quite irrespective of the directions the lines of force may have*, and he has called such substances diamagnetics.

Cor. 4. A diamagnetic, held by its centre of gravity but free to turn about this point, must react upon other magnets with the same forces as a steel or other magnet substituted in its place, and held with its magnetic axis reverse along the lines of force due to all the magnets in its neighbourhood.

Cor. 5. Any one of a row of balls or cubes of diamagnetic substance held in a magnetic field with the line joining their centres along a line of force, is in a locality of less intense force than it would be if the others were removed; but any one ball or cube of the row, if held with the line joining their centres perpendicularly across the line of force, is in a locality of more intense force than it would be if the others were removed.

Cor. 6. When a row of balls or cubes, or a bar, of perfectly non-crystalline diamagnetic substance, is held obliquely across the lines of force in a magnetic field, the magnetic axis of each ball or cube, or of every small part of the substance, is nearly in the direction of the lines of force, but slightly inclined from this direction towards the direction perpendicular to the length of the row or bar. Hence, since the magnetic axis of every part differs only a little from being exactly *reverse along the lines of force*, the direction of the resultant of the couples with which the magnets, to which the field is due, act on the parts of the row or bar must be such as to turn its length along the lines of force.

Cor. 7. The positions of equilibrium of a row of balls or cubes rigidly connected, or of a bar of perfectly non-crystalline diamagnetic substance, free to move about its centre of gravity in a perfectly uniform field of force, are either with the length along or with the length perpendicularly across the lines of force: positions with the length along the lines of force are stable; positions with the length perpendicularly across the lines of force are unstable.

Cor. 8. The mutual influence and its effects, referred to in Cors. 5, 6, 7, is so excessively minute, that it cannot possibly have been sensibly concerned in any phænomena that have yet been observed; and it is probable that it may always remain insensible, even to experiments especially directed to test it. For the influence of the most powerful electro-magnets induces the peculiar magnetic condition of which diamagnetics are capable, to so slight a degree as to give rise to only very feeble, scarcely sensible, mutual force between the diamagnetic and the magnet; and therefore the magnetizing influence of a neighbouring diamagnetic, which could scarcely, if at all, be observed on a piece of soft iron, must be inappreciably small on another diamagnetic.

Cor. 9. All phænomena of motion that have been observed as produced in a diamagnetic body of any form or substance by the action of fixed magnets or electro-magnets, are due to the resultant of forces urging all parts of it, and couples tending to turn them; the force and couple acting on each small part being the same as it would be if all the other parts were removed.

Cor. 10. The deflecting power (observed and measured by Weber) with which a bar of non-crystalline bismuth, placed vertically as core in a cylinder electro-magnet (a helix conveying an electric current), urges a magnetized needle on a level with either of its ends, is the reaction of a tendency of all parts of the bar itself from places of stronger towards places of weaker force in its actual field.

The preceding investigation, leading to Props. VI. and VII., is the same (only expressed in non-analytical language) as one which was first published in the Cambridge and Dublin Mathematical Journal, May 1846. The chief conclusions now drawn from it, with particulars not repeated, were stated in a paper entitled 'Remarks on the Forces experienced by inductively magnetized Ferromagnetic or Diamagnetic Substances,' in the Philosophical Magazine for October 1850.

Glasgow College,
March 15, 1855.

XXXVII. *On British Pectolites.*

By Dr. M. Forster Heddle, *and* R. P. Greg, *Esq.*[*]

KOBELL'S mineral *pectolite*, found in the amygdaloid of Monte Baldo in the Southern Tyrol, is introduced into Dr. Thomson's 'Mineralogy' with the original analysis (No. 16) misquoted, the quantity of water being given as 8·89 instead of 3·89. This blunder explains the fact, that in another part of his work, Thomson describes, under the name of *Wollastonite*, a mineral found in greenstone near Kilsyth, which, however, is evidently pectolite, the analysis (No. 2) also agreeing.

Dr. Thomson states that the mineral he analysed was also afterwards found by Lord Greenock in the neighbourhood of Edinburgh, and Mr. Rose informs us that the precise locality was the Costorphine Hill. Now Walker has given an analysis (No. 3) of a mineral from the Costorphine Hill which is evidently also *pectolite*; it is included in mineralogical works under the head of table spar, but containing soda, it must be *pectolite*, which differs from *table spar* merely in silicate of soda replacing silicate of lime. Walker's mineral then in all probability is identical with the mineral discovered by Lord Greenock; we are not, however, aware that it has been lately found at this place.

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[*] Communicated by the Authors.

Again, Thomson describes among the zeolites a mineral to which he has given the name of "*stellite*," found in greenstone at Kilsyth; the original specimen analysed by Dr. Thomson we have lately seen, and do not hesitate in saying it is ordinary *pectolite*, as indeed its analysis (made by Dr. Thomson evidently on an impure portion) had before led us to suppose.

Mr. Rose of Edinburgh states, that Lord Greenock obtained at Loch End a weathered specimen of this mineral, but we have no analysis of this specimen.

Then we have the so-called "*Wollastonite*" of the Castle Rock of Edinburgh*, the analysis of which, according to Kennedy, is given as No. 4 in the Table.

Dr. Thomson sets aside this analysis, and states that the *Wollastonite* of the Edinburgh mineralogists is *prehnite*. White *prehnite* does occur along with the Castle Rock mineral, and as our analysis agrees with the ordinary ones of *pectolite*, the conclusion is that Thomson's specimen contained *prehnite*, but none of the substance in question.

Our analyses, made on 25 grains, afforded,—

	a.	*b.*
Silica	55·12	53·06
Alumina . . .	·30	46
Oxide of iron . .	·52	...
Lime	33·36	33·48
Soda	7·85	9·98
Potash	·36	·29
Water	3·13	*not det.*

The second specimen was evidently the purest.

Next we have to notice the specimens found by Mr. Forrest in greenstone at Ratho, near Edinburgh. Here it occurs of a pale green colour, and in orbicular masses with a fibrous and radiating structure, at a depth of 90 feet, associated with *steatite, calcite, heavy spar, specular iron*; also in pseudomorphs after *analcime*, although analcime has not itself yet been found; and is found (though rarely) in distinct crystals. The fibrous variety is extremely tough when first obtained, but soon weathers, becoming asbestus-like on exposure.

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* Occurring in orbicular masses in trap, translucent and highly crystalline, with a radiating structure.

The specific gravity of the fibrous variety is 2·881, and our analysis on 25 grains is given under *c*; that of the crystals taken on 5 grains is given under *d*.

	c.	*d.*
Silica . .	52·525	52·580
Alumina .	·884	1·456
Lime . .	32·794	33·750
Magnesia .	trace	
Soda . . .	9·752 (with some potash)	9·258
Water . .	3·040	2·800
	98·995	99·844

About three years ago Mr. M'Gibbon found a mineral at Knockdolian Hill, near Ballantrae in Ayrshire, the matrix of which was limestone.

Its specific gravity is 2·778, and our analysis, made on 25 grains, is given in the Table, No. 8; and this mineral is therefore *pectolite*. At this locality it also occurs in needle crystals.

Some time ago we got this mineral from Talisker in Skye associated with *mesotype* and *stilbite*; the analysis is No. 9. Dr. Scott of Edinburgh has analysed this mineral, with results differing in the soda and the water, the quantity of the former being 7·670, and of the latter 5·058 (see No. 10).

Our last locality is on the shore near Girvan in Ayrshire, where the mineral occurs in quantity, and of considerable beauty, hitherto known to collectors as white *tremolite*; here it varies from highly translucent and fibro-crystalline to opake, being sometimes, however, as tough as the *okenite* from Faroe.

Our analysis on 25 grains afforded,—

Silica	53·480
Alumina . . .	·055
Oxide of iron . .	·005
Lime	34·384
Magnesia . . .	traces
Soda	9·877
Potash	·355
Water	3·262

The mineral is apparently purer at this locality than at any other.

We have *pectolite* occurring in Scotland at the following localities: Kilsyth, Costorphine Hill, Loch End, Edinburgh Castle, Ratho, Knockdolian Hill and Girvan in Ayrshire, and at Talisker in Skye: and we now have analyses of specimens from all these localities, with the single exception of Loch End*. Specimens from the Morne Mountains, said to be this *mineral*, have by our own analysis proved to be table spar, and consequently the only locality we have as yet of this mineral in the British Islands.

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* According to Prof. R. D. Thomson, pectolite has occurred also at Bishoptown, and near Kilpatrick.

Frankenheim argues that *pectolite* is anhydrous, chiefly because he wishes to prove it to be a *hornblende*. This is quite inadmissible; from not one locality has it been found anhydrous, and the per-centages from the different localities do not vary much, averaging about 3 per cent.; and although the proportion of water does vary *half a per cent.* when the powdered mineral has been dried at 212°, yet when this necessary precaution has been taken, the proportion is in each specimen invariably the same; any previous variation must therefore have been due to hygrometric moisture. In addition to this, we find the mineral to be very slightly absorbent; that from Ratho absorbs ·53 per cent. of moisture, that from the Castle Rock ·29 per cent., and that from Ayrshire ·35 per cent.; again, it parts with the water it does contain only after the most intense heating. The Castle Rock mineral, after about ten minutes' exposure to a red heat, gave only 1·2 per cent. of water; a loss in the analysis led us to redetermine the water, when, after exposing the mineral to a heat approaching to whiteness for thirty minutes, we got 3·13 per cent. Seeing, then, that the mineral in powder is almost *non*-absorbent, and that it retains the water it does contain with such tenacity, it appears to us to be an extraordinary conclusion to arrive at to say that water is unessential; and to exclude a constantly present constituent merely to suit a theory, is absurd.

There is a difference of opinion with regard to the formula of this mineral, and its place in the system. Dr. Scott rejects the formula given by Berzelius, and prefers one of his own, which agrees admirably with his analysis; but as the quantity of water as therein stated is much greater than elsewhere found, his formula of course does not suit the analyses of others.

Frankenheim also rejects Berzelius's formula, and offers instead,—

$$4(4CaO, 3SiO^3) + (4NaO, 3SiO^3) + 5HO,$$

the calculated per-centages of which are,—

Silica 15 atoms = 52·76
Lime 16 atoms = 34·29
Soda 4 atoms = 9·53
Water 5 atoms = 3·42

This, it must be allowed, agrees well with the analytical results; but the question is, does it agree more closely than Berzelius's; for Frankenheim substituted this formula in order that he might, by writing it more simply $4(CaO, NaO)\,3SiO^3 + HO$ (where the

CaO is to the NaO in the proportion of 4 to 1), and rejecting the HO, make the mineral come under the hornblende formula, viz.—

$$4RO, 3SiO^3,$$

RO standing for CaO, MgO, FeO^3, Al^2O^3, NaO.

We have, in calculating the formula from our analyses, chosen the crystals from Ratho as in all probability the purest:—

		Oxygen.			Atoms.
SiO^3,	52·580 =	26·209 =	11·03 =	11	11
CaO,	35·206 =	10·016 =	4·25 =	4	12
NaO,	9·258 =	2·376 =	1 =	1	3
HO,	2·800 =	2·489 =	1·05 =	1	3

giving the formula—

$$3(NaO, SiO^3) + 4(3CaO, 2SiO^3) + 3HO;$$

the calculated per-centages of which are,—

Silica	11 atoms =	508409 =	52·574
Lime	12 atoms =	337956 =	34·947
Soda	3 atoms =	93629 =	9·683
Water	3 atoms =	27039 =	2·796
		937043	100·000

This, it will be allowed, is a still closer approximation than the per-centages of the hornblendic formula, and this also is the formula given by Berzelius.

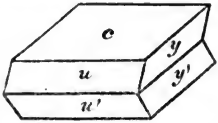
Now this formula points, not to *hornblende*, but to *augite*, for it will be seen that it is the same as that of *table spar*, 3CaO, $2SiO^3$; only neutral silicate of soda replaces silicate of lime, and *table spar* is an *augite*, the formula of which is 3RO, $2SiO^3$ (where RO may stand for CaO, MgO, NaO, Fe^2O^3, Al^2O^3, &c.); for if it is allowable to disregard for the time being the HO, in order to bring the mineral among the *hornblendes*, the same course cannot be objected to, to bring it among the *augites*.

The crystalline form of *pectolite*, as we have ascertained from the Ratho crystals, is the same as *Wollastonite*, with which it is therefore isomorphous. The accompanying figure represents the usual form of these crystals.

$c : u = 95^\circ\ 23'$

$c : y = 54\ 05\ (125^\circ\ 55')$

$y : u = 93\ 30$



Cleavage highly perfect, parallel to *c* and *u*. Twin face *c*. The face *y* coming in the zone *z*, *e*, (*y*), *x* of Brooke and Miller, appears to be hemihedral in the Ratho crystals, and is a plane not given in their edition of Phillips, though probably identical with the *h*1 of Allan's 'Phillips,' p. 47, where P : *h*1 = 126°, and M : T = 95° 20′.

We have also obtained cleavages of about 95° 30′ with highly crystalline specimens of *pectolite* from Monte Baldo in the Tyrol, and the Castle Rock, Edinburgh.

Pectolite, which as a characteristic species has not received as much attention as it deserves, is rather an abundant mineral, especially in Scotland, and is identical with the *Wollastonite* and *stellite* of Thomson, with the *stellite* of Sheppard, and with the *osmelite* of Breithaupt.

Table of Analyses.

No.	Locality.	$\dddot{\mathrm{Si}}$.	$\dot{\mathrm{Ca}}$.	$\dot{\mathrm{Na}}$.	H.	$\dddot{\mathrm{Al}}$, $\dddot{\mathrm{Fe}}$, $\dot{\mathrm{Mg}}$, $\dot{\mathrm{Ka}}$.	Analyser.
1.	Monte Baldo, Tyrol ...	52·34	35·20	9·60	2·80	traces	Kobell, 2nd analysis.
2.	Kilsyth, Scotland ("*Wollastonite*")	52·74	31·68	9·60	2·00	3·39	Thomson.
3.	Costorphine Hills	54·00	30·79	5·55	5·43	2·59 ($\dot{\mathrm{Mg}}$)	Walker.
4.	Castle Rock, Edinburgh	51·50	32·00	8·50	5·00	1·00	Kennedy.
5.	Castle Rock, Edinburgh	53·06	33·48	9·98	3·13	0·75	Dr. Heddle.
6.	Ratho ("*fibrous*")	52·525	32·794	9·752	3·040	0·884	Dr. Heddle.
7.	Ratho ("*crystallized*")	52·580	33·750	9·258	2·800	1·456	Dr. Heddle.
8.	Knockdolian Hill, Ayreshire............	53·240	32·220	9·570	3·600	1·002	Dr. Heddle.
9.	Talisker, Skye............	53·820	29·880	9·551	3·760	2·728	Dr. Heddle.
10.	Talisker, Skye............	52·000	32·850	7·670	5·058	1·820	Dr. Scott.
11.	Girvan, Ayrshire.........	53·48	34·384	9·877	3·262	0·415	Dr. Heddle.
12.	Bishoptown, Renfrewsh.	52·07	32·80	9·60	2·00	4·20	Dr. Thomson.
13.	Bavaria ("*osmelite*")...	52·91	32·96	$\dot{\mathrm{Na}}$ 6·10 $\dot{\mathrm{Ka}}$ 2·79	4·01	0·86	Adam.
14.	Bergen Hill, New Jersey ("*stellite*") ...	54·00	32·10	8·89	2·96	1·90	Kendall.
15.	Isle Royale, Lake Superior	55·00	32·53	9·72	2·75	1·10	Dickenson.
16.	Monte Baldo, Tyrol ...	51·30	33·77	8·26	3·89	2·47	Kobell, 1st analysis.

XXXVIII. *Magnetic Remarks.*

By Professor Faraday, *D.C.L., F.R.S. &c.*

My dear Tyndall,

In relation to your letter of last month*, I write, not for the purpose of giving what might be taken as an answer, but to say that it seems to me expedient and proper to wait and allow the thoughts that my papers may raise, to be considered and judged of at their leisure by those who are inclined to review and advance the subject. Perhaps, after a respectful interval, I may be induced to put forth such explanations, acknowledgements, or conclusions, as the state of the subject may then seem to render necessary or useful.

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* Phil. Mag. 1855, vol. ix. p. 205.

In the mean time, the more we can enlarge the number of anomalous facts and consequences the better it will be for the subject; for they can only remain anomalous to us whilst we continue in error. I may say, however, that the idea you suggest presents no difficulty to me; for having on former occasions (Exp. Res. 2501.) had to consider the magnecrystallic phænomena presented by the same body in different media, and having found the magnecrystallic difference unchanged in the media, I have no difficulty in conceiving that a body (as bismuth), which in the amorphous state is of the same magnetic character as the medium around it, shall, when employed as a crystal, be paramagnetic in one direction and diamagnetic in another (3157.). What happens in a medium may, according to my knowledge of the facts, happen in space; and is in full accordance with Thomson's clear paper on the theory of magnetic induction in crystalline bodies*.

In respect of the effects of pressure, to which you refer in your letter, we cannot easily draw conclusions on either side until we know better what pressure does. I am not aware whether you consider that pressure on bismuth, whilst it makes the metal more diamagnetic in one direction than another, also makes it more diamagnetic as a whole than before; or whether you suppose it *less* diamagnetic in the transverse direction of the pressure than at first. Gmelin says, on the authority of Marchand and Scheerer (vol. iv. p. 428), that the density of bismuth is diminished as pressure upon it is increased, and extraordinary as the fact seems, gives densities of the following degree for increasing pressures, 9·783, 9·779, 9·655, 9·556; a change in texture at the same time occurring. If the statement be true, then the line of pressure in your beautiful experiments may be the line of *least density* or of *least approximation*, though I hardly know how to think so; still it becomes difficult for us to draw reasons from the constitution of a compressed body, until we know what happens during the compression, although no difficulty arises in considering it, after compression in one direction, like to a magnecrystallic substance.

You are aware (and I hope others will remember) that I give the lines of force† only as *representations* of the magnetic power, and do not profess to say to what physical idea they may hereafter point, or into what they will resolve themselves. Advancing no principle, I say, that the hypothetical fundamental ideas already advanced, when taken in relation to the body of facts now known, are self-contradictory and inapplicable. The following points, namely,—that the *direction* and *polarity* of lines of magnetic force are always shown truly by the electric current induced in metal moving within their influence;—that the dualities of electricity and magnetism are always respectively and essentially related;—that the dualities of an isolated magnet are not related back in straight lines through the magnet;—are to my mind not hypothetical in character, but easily proveable by experiment:—and they, with the considerations arising from the principle of the conservation of force, seem to me to be left unexplained by, and in opposition to, the usual hypotheses. No difference arises about the laws of magnetic action and their mathematical development; and that, simply because they are as yet applied only partially, and thus far are in accordance with *all* the views taken, including mine. When the attempt is made to apply them so as to include at once *paramagnetic, diamagnetic*, and *electro-magnetic* phænomena, and at the same time to deduce them from *one* hypothetical cause, then they may become so large and yet precise as to enable us to distinguish between true and false assumptions. On my part I endeavour not to assume anything, but only to draw such conclusions from the assumptions already made, and the phænomena now discovered, as seem subject to experiment and tangible by facts.

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* Phil. Mag. 1851, vol. i. p. 177.

† It is nearly twenty-four years since I first called attention to these lines; Exp. Res. 114, note.

Some persons may feel surprised that I dwell upon points which are perfectly and mathematically explained by the hypothesis of two magnetic fluids, as, for instance, places of little or no action (3341. &c.). My reason is, that being satisfied by the phænomena of diamagnetism, &c. that that hypothesis cannot be true, all these and such like phænomena acquire a new character and a high importance which they had not before, and amongst other philosophical uses, point most emphatically to the essential relation of the dualities and their equivalency in power. They do not contradict the old hypothesis when that is partially applied, but they are not the less strong and striking as evidence in favour of the view of lines of force.

I am, my dear Tyndall,
Yours very faithfully,
M. Faraday.

Royal Institution,
March 14, 1855.

XXXIX. *On the Benzole Series.—Determination of Boiling-points.* *By* ARTHUR H. CHURCH, *Esq.*[*]

SINCE 1825, when Faraday discovered its first member, our knowledge of the hydrocarbons of the benzole series has remarkably extended: it is interesting to see from what various sources these hydrocarbons have been derived; and although doubts still remain as to the relations of these bodies to one another, yet their composition has been ascertained with certainty. Without taking notice of all these sources, it may suffice to mention that the five known members of the series occur in coal-naphtha, and some of them in the oil separated by the addition of water to crude wood-spirit; benzole having also been obtained from oil-gas and from benzoic acid; toluole from resin-gas, balsam of Tolu, dragon's blood and toluylic acid; while cumole has been procured from phorone and from cuminic acid, cymole from camphor and from cumin oil.

In experiments made for the purpose of obtaining a liquid appropriate for the preservation of highly oxidizable metals, I was surprised to find (oxygen being excluded) that on the application of heat, sodium immersed in toluole perfectly free from moisture soon became tarnished and evolved gas. This evolution ceased after a time, and the metallic lustre of pieces of sodium placed in the liquid remained unimpaired.

After many attempts to ascertain the origin of these phænomena, and after having satisfied myself experimentally of the absence of water from the toluole under examination, the observation was made that toluole, which exerted no further action on sodium, tarnishes it after redistillation in the ordinary manner, and it was ultimately found that the occasion of this was a product of oxidation. This oxidation may be prevented, if care be taken that the upper part of the retort do not reach a higher temperature than that at which the liquid boils. Toluole purified by sodium and distilled from a retort immersed in a bath of dilute chloride of calcium solution exerts no further action on sodium, and comes over to the last drop quite colourless and leaving no residue. Toluole, which, in addition to the ordinary methods of purification, had been treated with oil of vitriol and with solution of chromic acid, behaved in the same manner.

I was anxious to ascertain whether the boiling-point of toluole was altered by the treatment with sodium. I employed in the experiments several ounces of toluole from coal-naphtha, which, purified in the ordinary manner, had distilled over between 108° and 109° C.; I also made use of toluole from toluylic acid. The thermometers employed (three) were finely divided, and had been accurately tested, the errors being known up to 200° C. Into a small retort of symmetrical shape was introduced a coil of platinum wire, and about 100 grammes of toluole repeatedly purified with sodium. Before a perfectly trustworthy result could be arrived at, there were several precautions to be taken. An allowance had to be ascertained and made for irregularities introduced by the partial immersion of the thermometer-stem in the vapour; and it was found that when the spheroids which form upon the surface of the liquid touch the bulb of the thermometer (immersed in the vapour), they cause variations in the height of the mercury, thus interfering with the constancy of the boiling-point. Remembering these things, and taking care to place the bulb of the retort in a chloride of calcium bath, I observed the boiling-point of purified toluole, the bulb of the thermometer just touching the surface of the liquid. The temperature of the liquid rose gradually to 97° C.; from this point to 103° the rise was rapid. Between 103° and 104° eight-tenths of the toluole distilled over; and on redistillation, the boiling-point remained constant at 103°·6, the barometric pressure being $0^{m}\cdot757$. With toluole (purified by sodium) prepared from toluylic acid, exactly the same phænomena were observed.

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[*] Communicated by the Author.

Having now given the more important details of my procedure for the preparation, &c. of pure toluole, I may mention that I subjected benzole (from benzoic acid) to the same treatment: the boiling-point of benzole thus obtained, and that (from coal tar) purified by repeated crystallizations, is, however, identical. Of the xylole (obtained from coal-naphtha and wood-spirit), I collected for experiment that which came over at about 128°. After purification with potash, sulphuric acid, solution of chromic acid, and with caustic baryta and sodium, the boiling-point was but slightly reduced. The cumole (from cuminic acid and coal-naphtha) was purified in the same way, as was also the cymole (from oil of cumin and coal-naphtha).

I give here the (corrected) boiling-points of the liquids thus purified at a uniform barometric pressure of $0^{m}\cdot760$.

	Formula.	Boiling-point.	Difference.
Benzole . .	$C^{12}H^{6} = C^{6}3(C^{2}H^{2})$	80°·8	22°·9
Toluole . .	$C^{14}H^{8} = C^{6}4(C^{2}H^{2})$	103·7	22·5
Xylole . .	$C^{16}H^{10} = C^{6}5(C^{2}H^{2})$	126·2	22·2
Cumole . .	$C^{18}H^{12} = C^{6}6(C^{2}H^{2})$	148·4	22·3
Cymole . .	$C^{20}H^{14} = C^{6}7(C^{2}H^{2})$	170·7	

In the following table will be found the observations of other experimentalists compared with my own.

Benzole.	Toluole.	Xylole.	Cumole.	Cymole.
85°·5 **Faraday.**	114° **Gerhardt.**	129° **Cahours.**	144° 151°·4 **Gerhardt and Cahours.**	171° **Mansfield.**
86° **Mitscherlich.**	113° **Mansfield.**	126°·2 **Church.**	148° **F. Abel.**	171°·5 **Noad.**
80°·4 **Kopp.**	108° **Deville, Pelletier, and Walter.**		148°·4 **Church.**	175° **Gerhardt.**
80°·5 **Mansfield.**	109°·5 **Noad.**			170°·7 **Church.**
80°·8 **Church.**	110° **Wilson.**			
	106° **Glénard and Boudault.**			
	103°·7 **Church.**			

My own results are, I believe, strictly comparable with one another, having been obtained by the employment of the same apparatus in similar circumstances. The differences in the results of other observers may arise from very many causes, and in most cases no particular accuracy was aimed at. Kopp found that with benzole from benzoic acid (not purified by distillation from potassium, &c.), the temperature indicated by a thermometer with the bulb immersed in the vapour, differed from that indicated when the bulb was in the liquid by 0°·9 C.; with perfectly pure benzole, and if care be taken to adopt all the precautions I have mentioned, the difference is greatly diminished.

After obtaining the results which have been given in the present communication, I examined some of the distinguishing reactions of the bodies operated upon, in order to satisfy myself of their identity with those described under their respective names. As the boiling-point of toluole observed by me differed so considerably from those observations previously announced, I made analyses of the purified toluole employed in my experiments. These were perfectly satisfactory, as were also determinations of the sulphate of baryta in sulphotoluolate of baryta, $C^{14}\left\{\begin{matrix}H^{7}\\ SO^{2}\end{matrix}\right\}BaSO^{4}$, and in nitrosulphotoluolate of baryta, $C^{14}\left\{\begin{matrix}H^{6}\\ SO^{2}\\ NO^{4}\end{matrix}\right\}Ba\,SO^{4}$: the details and numerical results of these analyses, and of many others which the present inquiry necessitated, the limits and special object of the present paper do not admit of my giving here. The last-mentioned acid may be obtained most readily by a method which suggested itself to me when working upon the corresponding term of the benzole series; by it I have obtained nitrosulphoxylolic and -cumolic acids. As benzole with fuming sulphuric acid yields sulphobenzolic acid, I imagined that nitrobenzole would yield, when similarly treated, nitrosulphobenzolic acid; and this supposition experiment proved to be correct. It is not possible to obtain in this way sulphocymolic acid, for the compound $C^{20}\left\{\begin{matrix}H^{13}\\ NO^{4}\end{matrix}\right\}$ is not yet known; this acid may, however, be obtained easily by dissolving sulphocymolic acid in fuming nitric acid. The salts of these nitrosulpho-acids deflagrate when heated; in the case of the barium compounds, the residual sulphate of baryta assuming the form of long, narrow ribands.

Bases prepared from impure hydrocarbons of the benzole series give, on analysis, results tolerably accordant with theory: this circumstance, if not entirely accounted for by the numerous processes of purification which these bases undergo, is explained when we recollect the recent researches of C. G. Williams* 'On the Fractional Crystallization of Platinum Salts†.'

Many subjects for further investigation have suggested themselves to me during the progress of the present inquiry; among these the subject of isomers is of great interest. The important observations, too, of Regnault and others as to the alterations in their boiling-points which many liquids undergo when heated under pressure, I have found to refer to benzole, &c.; on this account, in purifying these liquids, I was obliged to desist from digesting them in sealed tubes. A digestion of toluole with sodium in a closed vessel for a fortnight causes the elimination of two bodies, the boiling-point (97°) of one being intermediate between that of benzole and toluole, the boiling-point of the second (112°) being between those of toluole and xylole. And I have lately found, that among the products of the distillation of eugenic acid with an excess of baryta, a liquid occurs for which my experiments (not made with a perfectly pure substance) indicate a formula $C^{18}H^{12}$; for $C^{20}H^{12}O^{4}-C^{2}O^{4}=C^{18}H^{12}$.

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* Phil. Mag. Sept. 1854.

† Anderson's observations on the boiling-points of certain bases (Phil. Mag. Feb. 1855) are also exceedingly curious

From this hydrocarbon, nitro-substitution products may be obtained, and I have very little doubt that it is an isomer of cumole. It boils at about 142°. It would seem, therefore, that there are three bodies having the formula $C^{18}H^{12}$; cumole boiling at 148°·4, mesitilole boiling at 155°, and the hydrocarbon from eugenic acid boiling at 142°. May not these bodies, and those obtained from toluole, stand in similar relations to one another? The same differences between their boiling-points are observable. I am still investigating this question with reference to the other members of the benzole series, and endeavouring by the differences of specific gravity, vapour density, refractive power, &c. of these liquids, to discover their true relations to one another.

By referring to the table given before, it will be found that, according to my experiments, the increment in the boiling-point in the benzole series for an addition of C^2H^2 varies between 22°·9 and 22°·2. By determining the boiling-points of the consecutive members of several series, in which not only carbon and hydrogen enter, but also oxygen and nitrogen, we shall be able to learn whether such formulæ[*] as those of Schröder will prove to any extent generally applicable. The substances in such experiments must of course be absolutely pure. The present contribution, having reference to a simple series containing only carbon and hydrogen, may be considered introductory. Schröder remarks, that between those boiling-points which are theoretically determinable from his formulæ and those experimentally observed, a difference of ±7°·2, ±14°·4 may be found. Since the remarks upon the isomers of cumole and toluole were written, I have noticed the close approximation of the differences between the boiling-points of these isomers to this difference of 7°·2.

February 1855.

XL. *On a New Single Fluid Galvanic Battery, more powerful, and less expensive in construction and use, than any of the Nitric Acid Batteries. By the* Rev. N. J. Callan, *Professor of Natural Philosophy in the Roman Catholic College, Maynooth*[†].

IN a paper published in the Philosophical Magazine for February 1854, I stated that sheet iron coated with an alloy of lead and tin, in which the quantity of lead is five or six times as great as that of tin, and afterwards platinized, might be substituted with advantage for the platinized silver used in Smee's battery. Being under a necessity of exhibiting occasionally during the academical year, optical experiments which required a very strong light, I was very anxious to have a galvanic battery in which there would be no need of porous cells or nitric acid, and which, with a moderate number of plates, would produce a brilliant coke light or lime light. From experiments made with our large galvanometer on the voltaic power of a pair of zinc and platinized coated iron plates excited by dilute sulphuric acid, I inferred that a battery containing a hundred, or one hundred and fifty such plates, would produce the light which I required. Having more than twenty Wedgwood troughs, each containing ten cells, I saw that the cost of such a battery would be very moderate: I therefore resolved to make one of 200 pairs of plates. Whilst the battery was being made, I occasionally tried the power of one trough or ten cells. I first used a solution of sulphuric acid consisting of one part of strong acid and about seven of water. I then tried one part of the same acid mixed with about six times its bulk of a pretty strong solution of common salt. The addition of the salt produced a considerable increase of voltaic power. I next employed an exciting mixture consisting of one part of nitric, six of sulphuric acid, and about fifty of water. With this mixture, ten pairs of plates, each about 6 inches square, ignited a pair of coke-points, which gave a small but brilliant star of light. But the nitric acid gradually dissolved the alloy of lead and tin with which the iron plates were coated, and the platina powder fell off. I then tried one part of nitric acid, twelve of sulphuric, and about a hundred of water. This mixture also acted on the coating of the iron plates. I was therefore obliged to give it up, and to return to the solution of sulphuric acid and common salt.

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[*] Not, however, based on arbitrary numbers.

[†] Communicated by the Author.

Before the entire battery was finished, I tried the power of 100 cells in igniting a pair of coke points. I was greatly disappointed by the feeble illuminating power of the coke light, and began to examine whether any of the cheap negative metals could be advantageously used with an exciting fluid which would contain a small portion of nitric acid. One of the first which occurred to me was cast iron, which acted so well as the negative element of the nitric acid battery. I prepared a cast-iron cell of the nitric acid battery, and a zinc plate, which I separated from the iron by pieces of wood. The cell was filled with one part of nitric, six of sulphuric acid, and about fifty of water. The galvanic action was very powerful, but the zinc plate was soon covered with a deposit of nitrate of iron. It was evident, then, that cast iron and zinc could not be excited with any fluid containing nitric acid unless the two metals were separated by a porous cell. I then tried the same cast-iron cell and zinc plate excited with one part of sulphuric acid and seven of water. The galvanometer showed that the voltaic current was very feeble. I therefore gave up all hope of making a battery such as I wanted, by exciting zinc and cast iron with dilute sulphuric acid.

During the evening of the day on which I tried the cast iron cell excited with dilute sulphuric acid, it struck me that, on having increased the strength of the acid solution by pouring a little acid into the cell, a strong impulse was given to the needle of the galvanometer, and that perhaps, by increasing very much the strength of the solution, I might get a very powerful galvanic current. I then resolved to try concentrated sulphuric acid and dilute acid of the various degrees of strength between concentrated acid and one part of acid mixed with eight of water.

The sulphuric acid which I used was very strong and pure, being made, not from pyrites, but from sulphur. In comparing the galvanic powers of the various batteries which I tried, I always used our large galvanometer, and sent the voltaic current only through the outermost coil, the diameter of which is about 25 inches. The coil is made of copper wire $\frac{3}{8}$ths of an inch thick. The outside coil is nearly 7 feet long. The galvanometer may be used as a sine or tangent galvanometer.

I began by exciting the cast-iron cell and zinc plate with concentrated sulphuric acid: the galvanic current produced was exceedingly feeble. I then mixed a small quantity of water with the acid: the deflection of the needle was greatly increased. I continued to pour water gradually into the cast-iron cell, and found that the deflection of the needle increased until the acid was diluted to a certain degree, and that when it was diluted still further, the deflection was diminished, and became constantly less as the quantity of water was increased. After five or six hours spent in frequently charging the cell with dilute sulphuric acid of various degrees of strength, I found that the acid which I used, diluted with twice its bulk of water, produced the most powerful galvanic current; and that when the acid was diluted with more or less than twice its bulk of water, the voltaic power of the cell was diminished. I afterwards found that acid which was still stronger than the very strong acid which I first used, and which was also made from sulphur, and not from pyrites, required to be diluted with three times its bulk of water in order to produce the maximum of galvanic power, or to produce a current equal in power to that which was produced by one part of the first acid diluted with twice its bulk of water. Either of the two diluted acids which I have described act very little on amalgamated zinc. I am inclined to think that the common sulphuric acid, which is generally made from pyrites, and is not so pure as that which is made from sulphur, nor so strong as that which I first used, should not be diluted with more than about once and a half its own bulk of water.

I subsequently tried the effect of sulphuric acid mixed with a solution of common salt, of dilute sulphuric and muriatic acid together, of undiluted muriatic acid, and muriatic acid diluted with a small quantity of water, and found by the galvanometer that a galvanic current of the same power was produced by the same cast-iron cell and zinc plate, whether they were excited by one part of the sulphuric acid which I first used, diluted with two of water, or with one part of the strongest sulphuric acid diluted with three of water, or with the first sulphuric acid mixed with three times its bulk of a strong solution of common salt, or with the strongest acid mixed with $3\frac{3}{4}$ times its bulk of the same solution, or with sulphuric and muriatic acid together diluted with a volume of water about twice as great as that of the sulphuric acid, or with undiluted muriatic acid, or muriatic acid diluted with a small quantity of water. Cast iron and zinc appear to be equally excited by the seven fluids just described.

Seeing, by the indications of the galvanometer, that the galvanic current produced by a cast-iron cell excited by any of the preceding fluids was exceedingly powerful, I resolved to compare it with the current produced by a nitric acid cell of the same size, and found that nearly the same deflection of the needle was produced by the two currents. I used a cell of the cast-iron nitric acid battery rather than one of Grove's or Bunsen's, because I found it more convenient than either, and not inferior in power.

The two cast-iron cells which I used were of the same size and shape; both were of a rectangular form. They were made for holding porous cells which contained zinc plates 4 inches square; their inside width was about $\frac{7}{8}$ths of an inch. For the nitric acid battery their width could not be less: were it less, the porous cells would not fit in the iron cells. The thickness of the zinc plate was about $\frac{1}{8}$th of an inch. Therefore the distance between each side of the zinc and cast iron was $\frac{3}{8}$ths of an inch. This distance cannot be diminished in the cells of the nitric acid battery; but in the battery excited with any of the above-mentioned fluids, it may be diminished without limit, and thus the galvanic power of the battery may be greatly increased.

When the distance between the zinc and cast iron of a cell excited by one part of sulphuric acid and two of water, or by any of the fluids which have been described, does not exceed the $\frac{1}{16}$th of an inch, the voltaic power of the cell is considerably superior to that of a cell of equal size of the nitric acid battery. I prepared a 4-inch cell and zinc plate of the nitric acid battery, and a zinc plate and cast-iron cell of the same size, in which the distance between the zinc and cast iron was about the $\frac{1}{16}$th of an inch. The opposite ends of these two cells were connected with the opposite ends of the coil of the galvanometer, in such a way, that, as soon as the two cells should be filled, the galvanic currents from them would flow simultaneously through the coil in opposite directions. The cell of the nitric acid battery was first filled: it was in the best working order, and produced a steady deflection of 82°. The other cell was then filled with one part of sulphuric acid and two of water, and the voltaic currents from the two cells flowed simultaneously through the coil in opposite directions. The current from the cell excited by dilute sulphuric acid destroyed the entire deflection produced by the nitric acid cell, and produced an opposite and steady deflection of 72°. On another occasion I made a similar experiment with a nitric acid cell and one of the same size, in which the distance between the zinc and cast iron was about the $\frac{1}{50}$th of an inch, and which was excited with one of the fluids already described. In this experiment the sine galvanometer was used, and the compass-box containing the magnetic needle was slid $21\frac{1}{2}$ inches from the centre of the coil in the direction of its axis. The voltaic current was first sent through the coil from the nitric acid cell, and a deviation of 38° was produced. The single fluid cell was then filled, and the galvanic currents flowed simultaneously from the two cells in opposite directions through the coil. The current from the single fluid cell destroyed the entire deviation of 38°, and produced a deviation of 42° on the opposite side of the magnetic meridian, thus showing that its power was more than twice as great as that of the current produced by the nitric acid cell. The porous cell employed in this experiment was of the best quality; it was perfectly saturated on the outside with nitro-sulphuric acid, and on the inside with dilute sulphuric acid, consisting of one part of very strong and pure sulphuric acid and five of water.

From the experiments and results which have been described, it is evident that a single fluid battery more powerful than any nitric acid battery of the same size, may be made by exciting a suitable arrangement of cast-iron cells or plates and amalgamated zinc with any of the following fluids. First, undiluted muriatic acid, or muriatic acid diluted with a small quantity of water; secondly, muriatic and sulphuric acid together diluted with a quantity of water a little more than twice as great by measure as that of the sulphuric acid; thirdly, sulphuric acid diluted with about twice its bulk of water, or the strongest sulphuric acid made from sulphur diluted with three times its bulk of water; fourthly, sulphuric acid mixed with three times its bulk of a strong solution of common salt, or the strongest sulphuric acid made from sulphur mixed with about $3\frac{3}{4}$ times its bulk of the same solution. The solution may be made by dissolving nearly two pounds of salt in each gallon of water.

It will be very useful to dissolve some carbonate or sulphate of soda in each of these exciting fluids, except the last. I recommend the last in preference to any of the others: first, because it is the cheapest of all; secondly, because I think the amalgamated surface of the zinc is better preserved, and kept cleaner by the last than by any of the others; thirdly, because it appears to act less on amalgamated zinc than any of the others.

A very powerful battery may be made by exciting amalgamated zinc, and iron, copper, platina, or a platinized metal with any of these fluids. But they produce the greatest galvanic power when cast iron is the negative element.

Muriatic acid preserves the amalgam on the zinc, but it soon covers it with a dark scum, which I fear would, after some time, diminish the action of the battery. Besides, it acts more than dilute sulphuric acid on amalgamated zinc. One would expect that sulphuric acid mixed with a solution of common salt would have the same effect as muriatic acid on the zinc; but such is not the case.

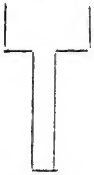
On account of its powerful galvanic action, a cast-iron battery charged with any of the above-mentioned fluids generates a large quantity of hydrogen, which produces considerable effervescence at the top of the cells. To obviate this inconvenience, I mixed with the exciting fluids various metallic and alkaline salts, hoping that the oxygen set free might combine with the liberated hydrogen, as it does in Daniell's and in the nitric acid batteries; and that some of them which contained the same proportion of oxygen as the nitrates do, might, like them when dissolved in sulphuric acid, give an increase of voltaic power. But my hopes were disappointed. The salts which I used principally were sulphate of iron and copper, acetate of lead, phosphate of soda, chlorate, arseniate and permanganate of potash. The effervescence appeared to be somewhat diminished by acetate of lead and phosphate of soda. Sulphate of copper and arseniate of potash produced a slight increase of voltaic power; but when sulphate of copper was used, copper was soon deposited on the zinc plate. Chlorate of potash, as might be expected, caused a series of explosions in the cell. There was no beneficial result from the use of permanganate of potash or peroxide of lead. On the whole, the effect produced by these substances would not be worth their cost.

In making a cast-iron battery to be excited by any of the fluids which I have described, care must be taken first to have the cast iron and zinc very near to each other; and secondly, to protect the inactive part of the cast iron against the action of the exciting fluid, and when the battery is intended not for constant work, but for experiments which may be occasionally interrupted, to make provision for taking the metallic plates out of the exciting fluid whenever it may be necessary to suspend the experiments.

First, the greater the proximity of the metallic elements, the more powerful will be the galvanic action, provided the hydrogen escapes with sufficient freedom to allow the exciting fluid to be in continual contact with the metals. If the hydrogen prevents the continual contact of the exciting fluid and metals, there will be a series of voltaic currents instead of one constant uniform current. I have found that when the zinc plate is not more than 4 inches square, the hydrogen does not interfere with the constant action of the battery, even though the distance between the zinc and cast iron should not exceed the $\frac{1}{30}$th of an inch. Even in that case a perfectly steady deflection of the magnetic needle is produced by the battery, and therefore the voltaic current must be constant and steady. I have not yet tried plates larger than 4 inches square, but I am inclined to think that plates a foot square or more need not be more than $\frac{1}{16}$th of an inch asunder.

Secondly, the part of the cast iron which has little or no effect in producing the voltaic current, must be protected against the action of the exciting fluid. Each of the exciting fluids that have been described acts a good deal on cast iron; and were the inactive part of the iron unprotected, there would be a considerable and useless consumption of cast iron, and of the strength of the exciting fluid. The iron may be sufficiently protected by covering it with vulcanized india-rubber, wood, or any other substance on which the exciting fluid acts but little or not at all.

The cast iron may have the form of cells which hold the exciting fluid, or of plates, each pair being connected together at the top, but separated from each other elsewhere so as to admit a zinc plate between them. The distance between each cast-iron plate and the one connected with it at the top may be a quarter or a fifth of an inch, so that a zinc plate an eighth of an inch thick may be put between the two, and that each side of the zinc will be a sixteenth or twentieth of an inch from the cast iron. The contact between the zinc and cast iron may be prevented by a thin wedge of wood at each corner of the plate. If cast-iron cells be used, they must be an inch or an inch and a half higher than the zinc plates. The width of the part of the cell which contains the zinc plate should not exceed one-fourth of an inch, otherwise the distance between the zinc and cast iron will be too great, and there will be a loss of galvanic power. The part of the cast-iron cell which is near the top or above the zinc plate should be about $1\frac{1}{2}$ inch wide, in order to hold a sufficient quantity of the exciting fluid. The fluid contained in the narrow part of the cell would be so small that its exciting power must be exhausted in a few minutes. The annexed figure represents a vertical section of the best form of cast-iron cells. The form is rectangular. The zinc plate is contained in the narrow part of it; the wide part, if it be 1½ inch high and 1½ inch wide, will contain as much of the exciting fluid as will be required for a lecture of an hour or two.



If cast-iron plates be used, earthenware or other cells must be employed for holding the exciting fluid, and then the outside of each cast-iron plate is inactive; it is exposed to the action of the fluid and must be protected. Ten or twelve pairs of cast-iron and zinc plates may, like the copper and zinc plates of a Wollaston battery, be fastened to a bar of wood; they may then be let down at once into a Wedgwood trough containing the exciting fluid, and may be taken up whenever it is found necessary or convenient to suspend the action of the battery.

The great galvanic power of cast iron and zinc excited by one part of sulphuric acid and two of water, was first discovered in the beginning of last June. In a few days after the discovery, I arranged in one series 72 cast-iron cells of our nitric acid battery, each containing a 4-inch zinc plate separated from the iron cells by pieces of wood. They were charged with one measure of sulphuric acid and two of water. The galvanic current was sent through a pair of very thick coke points, each about an inch long. The greater part of the coke was raised to a white heat; the result was a most brilliant light, which appeared to me to be equal in illuminating power to any coke light I have ever seen produced by an equal or much larger number of cells of the nitric acid battery. There was a remarkable difference between the appearance of the coke points when ignited by this battery and the nitric acid battery. On this occasion the negative and positive points appeared to be equally ignited and illuminated. With the nitric acid battery, the positive point is much more intensely ignited, and gives out a far greater quantity of light than the negative one.

At least one-third of the power of the battery just described was lost: first, because the distance between the zinc and cast iron was about three-eighths of an inch; secondly, because the cast-iron cells had been used several times in the nitric acid battery, and were coated with nitrate of iron, which greatly diminishes the galvanic power when the cast iron is excited by dilute sulphuric acid. I have found by means of the galvanometer, that the power of a galvanic current produced by a cast-iron cell excited by one part of sulphuric acid and two of water, whilst it is coated with nitrate of iron, is considerably less than one-half that of the current produced by the same cell excited by the same fluid after the nitrate of iron has been removed. It is necessary to clean the cast-iron cells or plates before they are used, by leaving them for some time in dilute sulphuric or muriatic acid.

In the beginning of September I showed in the College a battery of 182 cast-iron cells excited by one part of sulphuric acid and three of a strong solution of common salt. The experiments made with this battery consisted in the ignition of coke-points and of various metals. A scientific friend who came from Dublin to be present at the experiments, who had great experience of the working of nitric acid batteries, and witnessed in the College the action of 275 4-inch cells of the nitric acid battery on one occasion, and of 200 cells on another, stated that the 182 cells of the single fluid battery appeared to him to be superior in power to the 275 or the 200 nitric acid cells. On this occasion I requested several persons to compare the degree of ignition and illumination of the negative and positive coke-points. In order to be able to look steadily at the ignited points, they used deeply-coloured glasses. They all stated that they could not perceive any difference between the degree of ignition of one point and the other. When the connexion with the battery was broken, both points appeared equally heated, and equally or nearly equally consumed.

It has occurred to me, that the superior heat and light of the positive coke-point ignited by a nitric acid battery arise principally from the action of the exciting fluid on the positive metal; and that, because in the cast-iron battery excited by any of the fluids I have used, the exciting fluid acts a good deal on the negative as well as on the positive metal, the negative coke-point should be ignited and illuminated nearly as much as the positive one. Perhaps also the galvanic power of the single fluid cast-iron battery is proportional, not to the action on the zinc alone, but to the sum of the actions on the cast iron and zinc. If this be so, the expense arising from the consumption of metal in producing a given galvanic power will be less in the single fluid battery than in the nitric acid battery; for zinc is more than twice as dear as cast iron.

In using for the electric light a cast-iron battery of 60 cells, excited by about one part of sulphuric acid, one of muriatic, and two of water, I made an experiment which proves the great intensity of this battery. After the battery had been at work for about three-quarters of an hour, I emptied two of the cells at one end. Whilst they were empty, the ends of the battery were connected with a pair of coke-points. The voltaic current passed through the two empty cells, ignited the coke-points, and produced a brilliant light. The voltaic current had no other means of passing to the coke than through the damp pieces of wood which separated the zinc plates from the cast-iron cells, or through the wooden frames in which the two empty cells were placed.

The Rev. Dr. Robinson and Mr. Rergin, to whom I mentioned the various fluids which I found to excite cast iron and zinc so powerfully, have lately tried a cast-iron battery of 48 cells, charged with one part of strong sulphuric acid and three of a pretty strong solution of common salt. The distance between the zinc and iron was nearly $\frac{1}{8}$th of an inch. The quantity of fluid used in filling the battery was, I think, a gallon and a half, in which there were about three pints, or about seven or eight pounds of sulphuric acid. With this battery they had a brilliant coke light sufficiently steady to enable them to make observations on the light with the prism and polariscope; also lights produced by the ignition of various metallic points, on which lights similar observations were made. Various other experiments were made; they commenced at one o'clock, and were not given up till nine. There were of course several interruptions, during each of which the fluid was, by a very ingenious contrivance, poured off the metallic plates. During the eight hours the experiments lasted, the battery was in constant action at least three and a half or four hours. At the end of the experiments the two metals were quite clean, and there was no sensible diminution of voltaic power. Such is the substance of the account which I received of the action of this battery. The results of this trial of the battery show that a cast-iron battery, excited by one part of sulphuric acid and three of a solution of common salt, is very powerful, extremely constant in its action, and most œconomical in use. The sulphuric acid employed was made, not from pyrites, but from sulphur. I purchase, at the vitriol works of Messrs. Boyd, Belfast, sulphuric acid made in the same way, and of the same strength, at the rate of 8*s*. 6*d*. per cwt., or for less than one penny per pound. Hence at the price at which sulphuric acid is sold by Messrs. Boyd, the cost of the exciting fluid for eight hours' experiments scarcely exceeded eight pence. I have also got from Messrs. Boyd for 3*s*. 6*d*. per cwt., weak but pure muriatic acid of the specific gravity 1·130, which I have found to answer for the cast-iron battery better without being diluted, than strong muriatic acid diluted with a small quantity of water.

The cast-iron battery excited by any of the fluids which I have mentioned, is superior in power to any of the nitric acid batteries; it is less expensive in construction, since it does not require porous cells; it is far more œconomical in use; it is free from the trouble, annoyance, and all the very serious disadvantages which arise from the use of porous cells and nitric acid; it is extremely constant in its action, and may by a simple contrivance be made to act with almost perfect constancy till the zinc plates are worn out. The metals will remain clean for any length of time the battery may be in action: no hydrogen adheres to the cast iron. The only cause, then, of the diminution of galvanic power is the exhaustion of the exciting fluid. By siphon tubes, one of which reaches near the bottom of each cell, passes through a hole in the side of the cell a little above the upper edge of the zinc plate, and projects about an inch from the outside of the cell, and dropping tubes connected with the vessel which contains the exciting fluid, one of the tubes being set over each cell, a supply of fresh exciting fluid may be given to each cell, and the exhausted fluid may be made to pass out through the siphon tube. The dropping tubes should project from the bottom of the vessel which holds the fluid, and should be at such distances from each other that one will be over each cell of the battery. The vessel should be air-tight, and a stopcock fitted to the top of it, so that the communication between the inside of the vessel and the external air may be cut off by closing the stopcock, and may be renewed by opening the stopcock. When the communication between the inside of the vessel and the external air is cut off, the fluid ceases to flow into the cells from the dropping tubes: as soon as the communication is renewed, the fluid begins again to flow into the cells from the dropping tubes, and the exhausted fluid, as well as the sulphate of iron and zinc, at the bottom begins to flow out through the siphon tubes. By suitable clockwork, the stopcock may be opened and closed as may be required, in order to give the necessary supply, and no more, of the exciting fluid to each cell.

I have not yet tried a large series in which the zinc and cast iron were very near to each other. I am preparing for the College a battery of about 300 pairs of 4-inch cast-iron and zinc plates separated from each other about $\frac{1}{16}$th of an inch, and 20 pairs of plates, each about a foot square. The former are intended for effects which require considerable intensity, such as heat and light; and the latter for the decomposition of water and the lime light.

Having found that the galvanic power of cast iron and zinc excited by one part of sulphuric acid and two of water, was increased by dissolving sulphate of copper in the acid solution, it occurred to me to try the effect of cast iron instead of the copper used in Daniell's battery. I put into a saturated solution of sulphate of copper, a copper plate 4 inches square, and a zinc plate into a porous cell filled with dilute acid. When the porous cell was put into the solution of sulphate of copper, and the two metals connected with the opposite ends of the coil of the galvanometer, a steady deflection of 25° was produced. The copper plate was removed and a cast-iron one of the same size was put in its place. The cast iron and zinc plate produced a deflection of 30°. Hence by substituting cast iron for the copper used in Daniell's battery, a more powerful constant battery may be made. The cast iron may be in the form of cells, which will act as the negative metal, and will at the same time hold the solution of sulphate of copper. Thus the expense of glazed cells, which are so easily broken, may be saved.

I have also tried cast iron excited with a solution of sulphate of iron and zinc excited with dilute acid, both being separated by a porous cell. A 4-inch cast-iron plate gave a deflection of about 21°, whilst a Daniell's of the same size produced a deflection of 25°. Hence a battery nearly equal in power to Daniell's may be made by exciting cast iron with sulphate of iron and zinc with dilute acid. The cost of sulphate of iron is only about the eighth of that of sulphate of copper.

Maynooth College,
March 1, 1855.

P.S. I have lately tried a battery of 70 4-inch cells, in which the zinc and cast iron were very near to each other. The battery was charged with one part of sulphuric acid and three of a solution of common salt. The experiments consisted in the ignition of metals and coke-points. A brass wire $\frac{1}{4}$ of an inch diameter, and a piece of zinc about $\frac{3}{8}$ by $\frac{1}{4}$ of an inch thick, were burnt away so rapidly that the lecture room, which is about 40 feet square and 18 high, was soon filled with the smoke produced by the combustion of the metals.

For the coke light, I used Deleuil's apparatus; the light was sufficiently intense to enable a person to read the smallest print at the most distant part of the room whilst the window-shutters were closed. During the entire time of the experiments, which lasted nearly an hour and a half, the action of the battery was not suspended for more than about five minutes. At the end of the experiments, the light appeared to be as brilliant as at the commencement. Had there been time to continue the experiments, the power of the battery, would, I think, have been sufficient to produce a strong coke light for another hour or half-hour. The cost of the acid used in charging the 70 cells was about 9*d.* or 10*d.*

The voltaic current by which the metals and coke points were ignited was constantly passing through the coil of the galvanometer. I forgot to observe the deflection produced by the voltaic current during the combustion of the metals; but during the ignition of the coke-points, the deflection was frequently observed: the average deflection did not exceed 40°. I have since found that the current from a single pair of plates, each an inch and a half square, produces a deflection of 58°, which requires a current about twice as powerful as one which would cause a deflection of 40°. Hence not more than about the $\frac{1}{16}$th of the power of the 70 4-inch cells was employed in producing the coke light. Fifteen-sixteenths of their power was inactive, whilst the exciting fluid was acting on the metals, and wasting its own strength. Hence it strikes me, that for the electric light which is now used for practical purposes, a battery consisting of 80 or 90 cells, each about an inch and a half or two inches square, would be the most œconomical. I purpose to get a battery of 100 pairs of plates, each two inches or an inch and a half square, in order to try its power of producing the electric light.

The greatest length of the flame between the coke-points appeared to me to be less than it would be with a nitric acid battery of 70 4-inch cells. Hence I infer that the intensity of the single fluid battery is less than that of the nitric acid battery, although the quantity of electricity is much greater. The state of some of the cells after the experiments, excited an apprehension that the distance of $\frac{1}{16}$th of an inch between the zinc and iron may not be sufficient for the free descent of the sulphate of zinc, though it may permit the hydrogen to escape with perfect freedom. However, I am inclined to ascribe the condition of the cells, which were new, to the sand which remained attached to them after they were taken out of the mould.

Maynooth College,
March 15, 1855.

XLI. *Notes on Mineralogy.*—No. I. *On the Chemical Composition and Optical Properties of the Mica of the Dublin, Wicklow, and Carlow Granites. By the* Rev. SAMUEL HAUGHTON, *M.A., Professor of Geology in the University of Dublin*[*].

THE minerals included in the mica family may be divided into various groups, founded on their chemical composition and optical properties.

Chemically considered, they are divided by Rammelsberg and others into three families :—

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[*] Communicated by the Author.

1. Potash mica.
2. Lithia mica.
3. Magnesia mica.

Optically considered, they are divided into three groups also:—

1. Biaxial micas, the angle between the optic axes being from 44° to 75°.
2. Biaxial micas, with angle between optic axes from 5° to 20°.
3. Biaxial and uniaxial micas, with angle between optic axes from 5° to 0°.

The first optical group includes the potash and lithia micas of the chemical division, while the third chemical group, or magnesia mica, is divided between the second and third optical groups, the second group being usually designated phlogopite, and the third Biotite.

The potash and lithia micas are considered by Rammelsberg as represented by the following general formula,—

$$m(\mathrm{RO,\ SiO^3}) + n(\mathrm{R^2\,O^3,\ SiO^3})\,;$$

in which formula, in the potash micas,—

$$m=1,$$
$$n=2,\ 3,\ \text{or}\ 4\,;$$

the last two cases, $n=3$ and $n=4$, being the muscovite of mineralogists; and the first, $n=2$, or

$$\mathrm{RO,\ SiO^3} + 2(\mathrm{R^2\,O^3,\ SiO^3}) + \mathrm{kHO},$$

being known as margarodite.

The micas of the Dublin, Wicklow, and Carlow granites, analysed by me, belong to the margarodite genus, and contain two atoms of water, corresponding to $k=2$.

The following are the analyses of three micas selected from three distant localities of the granite chain of the south-east of Ireland.

No. 1. Mica from the Three Rock Mountain, county Dublin; gray, transparent, containing specks or flakes of a bronze-coloured or black mica.

	Per cent.	Atomic quotients.			
Silica	43·47	0·959		2·863	3
Alumina . . .	31·42	0·611	0·670	2·000	2
Peroxide of iron	4·79	0·059			
Lime	1·38	0·049	0·379	1·131	1
Magnesia . . .	1·13	0·056			
Potash	10·71	0·228			
Soda	1·44	0·046			
Loss by ignition	5·43	0·603		1·800	2
	99·77				

No. 2. Mica from Glendalough Valley, county Wicklow; gray, silvery, transparent, with specific gravity $=2·793$.

	Per cent.	Atomic quotients.			
Silica	44·71	0·987		2·973	3
Alumina	31·13	0·606	0·664	2·000	2
Peroxide of iron	4·69	0·058			
Lime	1·09	0·035	0·332	1·000	1
Magnesia	0·90	0·045			
Potash	9·91	0·211			
Soda	1·27	0·041			
Loss by ignition	6·22	0·691		2·083	2
	99·92				

No. 3. Mica from Mount Leinster, county Carlow; a gray, silvery, transparent mica.

	Per cent.	Atomic quotients.			
Silica	44·64	0·970		2·913	3
Alumina	30·18	0·587	0·666	2·000	2
Peroxide of iron	6·35	0·079			
Lime	0·00	0·000	0·298	0·900	1
Magnesia	0·72	0·036			
Potash	12·40	0·262			
Soda	trace				
Loss by ignition	5·32	0·591		1·774	2
	99·61				

If we take the mean of these analyses, we find,—

Average Mica of Dublin, Wicklow, and Carlow Granite.

	Per cent.	Atomic quotients.		
Silica	44·27	0·962		3
Alumina	30·91	0·601	0·667	2
Peroxide of iron	5·27	0·066		
Lime	0·82	0·029	0·338	1
Magnesia	0·92	0·046		
Potash	11·01	0·234		
Soda	0·90	0·029		
Loss by ignition	5·66	0·629		2

There can be little doubt, from the foregoing analyses, that the transparent, gray mica of the district under consideration is a margarodite, having the rational formula

$$\mathrm{RO,\ SiO^3 + 2(R^2O^3,\ SiO^3) + 2HO}.$$

The mica which deviates most from this formula is the mica of the Three Rock Mountain, and this deviation may possibly be due to the presence of grains of black mica, which also occurs in the mass of the granite, and of which I was unable to obtain a sufficient quantity for chemical or optical examination. The quantity of protoxides in the Three Rock mica is somewhat in excess of that required by the formula.

The angles between the optic axes of these micas were carefully determined, and found to be as follows:—

Angles between Optic Axes of Micas.

		°	′
1.	Three Rock mica	53	8
2.	Glendalough mica	70	4
3.	Mount Leinster mica . . .	72	18
4.	Lough Dan mica	70	0
5.	Glenmalure mica	67	11

I have added to the determination of angles of the micas analysed, the optic angles of two other specimens of gray, transparent mica from Lough Dan and Glenmalure, county Wicklow.

The four micas which were free from any intermixture of black mica have a high angle, while the angle between the optic axes of the mica from the Three Rock Mountain, which contained flakes of black mica, is nearly 20° less than that of the pure transparent gray micas.

The uniformity of the preceding analyses is sufficient to show that margarodite is entitled to be considered as a distinct species of hydrated mica, and that it is not merely an altered form of muscovite, an opinion advocated by M. Dana in the last edition of his 'System of Mineralogy.'

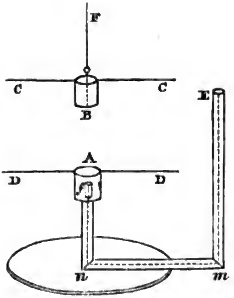
Crystallographically considered, the micas just described are trimetric, and occur in flat, right rhombic prisms, with angles of 6° and 120°; or in hexagonal tabular prisms, formed by the replacement of the acute angles of the rhombic prisms, all the angles of the hexagon being 120°.

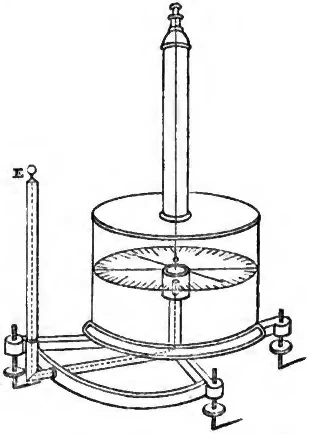
The plane of the optic axes is perpendicular to the flat plane of cleavage, and bisects the acute angles of the rhomb, or intersects perpendicularly two of the sides of the hexagon. I have not found among the micas of the granite of the south-east of Ireland, any instance of the optic axes lying in the shorter diagonal of the rhomb.

XLII. *On a new Electroscope.* *By* M. Melloni*.

WHEN a conductor in a natural state is brought near an electrified conductor, it renders imperceptible a portion of this electrical state, and by communicating its positive tension to the masked fluid in proportion as the perceptible fluid quits it by dispersion, prolongs the duration of the electric charge. On the other hand, it is well known that this effect is derived from the opposite electricity, developed by induction, in the nearest part of the induced body, and that the electricity, homologous with that of the inductive body, appears in the most distant portions, where it is distributed in proportions increasing with the diminution of the radius of curvature.

A combination of these three data, led me to conceive the possibility of constructing an electroscope of extreme sensibility, and capable of remaining electrified in either direction for a much longer time than any known apparatus of this description. The result has completely answered my expectations, and as I am convinced that this new instrument will become exceedingly useful in electrical researches of several kinds, I shall endeavour to describe it with all necessary details.





A is a little metallic cup, furnished with two long wire appendages, D, which are soldered to opposite points of its upper margin; it communicates, by a conductor passing through a glass tube, with a ball or disc of metal E. B is a second reversed metallic cup, a little smaller and much lighter than A; it is attached below a wire or very thin metallic lever CC, which is suspended by its middle with a silk thread F. The axes of the two cups are brought to the same vertical line, and the suspending thread, placed at such a level that the second cup is entirely contained within the first, where it must be able to turn freely round its point of suspension, without touching the walls of the fixed cup A*.

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* From the *Comptes Rendus*, Dec. 11, 1854, p. 1113.

When the apparatus is thus arranged, it is clear that if the conductor E receives an electrical charge, it will be propagated by transmission to the outer cup A, and that when there it will act by induction upon the inner cup B. If we suppose, for example, that the electricity communicated is positive, this electrical force distributed in A will disturb the equilibrium of the natural fluid of B; will repel the positive fluid and attract the negative, which will react in its turn upon the free fluid of A, mask a certain quantity of it, and abandon the rest to the known laws of electrical distribution on insulated conductors; so that the intensity of the action will depend on the curvature of the surfaces, and will be weaker on the walls of the cup than upon its appendages. The outer cup A of the charged apparatus will consequently contain a certain proportion of masked positive electricity (that is to say accumulated without tension and without mobility), and its appendages DD will possess a free electricity of the same kind, which will increase in energy in proportion as we approach the extremities.

As regards the inner mass B and its lever CC, there will be masked negative electricity on its central portion opposite the cup A, and free positive electricity on the remainder of the moveable system, that is to say, on the flat top of the reversed cup and on the lever above it. Now the latter description of electricity will evidently be far more energetic at the extremities of the lever than at its middle portion or on the top of the cup, —1st, because these extremities constitute the most distant points from the inductive action; and 2nd, because the curvature here is greater than anywhere else.

Thus the lever CC, possessing the same kind of electricity as the appendages DD, and being from its concentric position subject to the mutual action of their repulsive forces, will be energetically repelled, unless it be exactly in the same azimuth with them; after a few oscillations it will stop at a certain angle of deviation. The electrical charge, communicated to the fixed system EADD, will then begin to diminish.

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* In the electroscope which has been constructed, there is a peculiarity not mentioned in the description,—a small metallic cylinder *f* rises from the middle of the fixed cup A; when the moveable cup is well hung, this cylinder occupies its interior without touching it.

This diminution, however, takes place much more slowly than in the ordinary electroscopes, in consequence of the masked electricity which is gradually disengaged from the central part, and which replaces on the cup A its appendages DD, the wire of communication and the disc E, a portion of the free electricity lost by dispersion. The double or inductive electrization of the moveable system BCC will exactly follow the successive phases of the simple electrization of the fixed system, its two principles will be re-compounded gradually in proportion to the loss of the charge, and after a certain time the whole will return to the natural condition. All this is independent of the method employed for charging the conductor E, and will consequently apply equally to cases of direct charge by contact, and to cases of indirect or opposite charge obtained by means of induction.

To resume, the moveable part of the instrument is always electrified by induction and never by communication; the difference in the form of the centres and extremities of the fixed and moveable portions renders the distribution of the motive forces the most advantageous that can possibly be for the rotation of the index, and the inductive action of the central surfaces masking a portion of electricity which is afterwards set free in proportion to the loss undergone, prolongs the duration of the charge.

The small size of the pieces constituting the essential part of the apparatus accelerating the loss of electricity by diffusion in the surrounding medium, it is necessary that they should be enclosed in a case, in which the air must be kept very dry by means of some substance which possesses a great attraction for moisture. A dry state of the internal air is especially necessary to prevent variation in the torsion of the silk thread which supports the reversed cup, so as to enable the index CC to return constantly to the same azimuth, when the appendages DD have lost their electrical charge.

The case must be of a convenient form, and as the observations to be made require the knowledge of the angles of deviation formed by two rods superposed without contact, and kept at a certain distance from a dial plate placed below them, the most favourable arrangement is evidently to suspend the free extremity of the silk thread from the interior of the apex of a vertical tube, opening in the centre of a horizontal glass disc, the circumference of which rests upon a cylindrical metallic receiver, but little wider than the moveable lever and the subjacent appendages of the fixed cup. The upper edge of this receiver must be flattened and lined with leather, to intercept the communication between the air within and the air outside the apparatus, when the glass is fixed by the pressure of little screws upon the ring of metal which keeps it in its place.

The divided circle which measures the angles formed by the repulsion of the index, is to be perforated in the centre to give a free passage to the fixed cup A. The conducting wire is contained in a tube of varnished glass, filled with mastic; it is bent twice at a right angle in the same vertical plane, so that it resumes its original direction, and thus terminates at the outer piece of metal intended for the introduction of the electrical charge.

Into the space below the dial plate one or two reservoirs filled with chloride of calcium are passed, by means of openings in the bottom of the metal receiver, which are afterwards closed by screws. The receiver is supported on a tripod furnished with screws, which serve to place the suspended portion of the apparatus in equilibrium.

Lastly, the necessity of moving the apparatus from place to place, and of giving a certain initial angle of deviation to the moveable lever, renders it necessary that the upper extremity of the tube in which the silk thread is enclosed should be capable of motion in two directions. The first is a simple vertical movement, to allow the inner cup to rest on the flat bottom of the outer cup, and to raise it again to the proper height; the second a horizontal rotatory movement, to cause a slight deviation of the index at the commencement of each series of experiments. The rotatory movement will be communicated to the moveable system by means of the torsion of the silk.

As the same force is the cause of the resistance which stops the electrized lever at a greater or less angular distance, its strength must be proportioned to the weight of the moving mass. On this account, instead of a single thread from the cocoon, it is useful to employ several of them united, not by twisting, but simply glued together by the action of their own gummy nature and hot water.

If the force of the torsion of the thread be found too weak, and it be desired to abridge the time required for the observations, it is only necessary to place parallel to the index on the moveable cup a little magnetized needle, as is done with the indicator of Peltier's electroscope, and to arrange the appendages of the fixed cup in a direction forming an angle of 4 or 5 degrees with the magnetic meridian. We must not forget, however, that in this case we lose in exactitude what we gain in rapidity of observation. The assistance of the magnetized needle may nevertheless be useful under some circumstances, and especially when the great humidity of the air rapidly removes the electricity to the exterior of the instrument.

XLIII. *On some Extensions of Quaternions.* *By* Sir William Rowan Hamilton, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the French Institute, Hon. or Corr. Member of several other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from p. 51.]

Section V.

[30.] IN applying to associative *quines* the general theory of the Third Section*, we may (as has been seen) omit each of the signs Σ' as unnecessary, the index h receiving only one value in the sum thereby indicated; and may suppress each sum Σ'' as vanishing. In this manner the type IV., or the formula (151), becomes,

$$\text{IV..}\qquad (egf)(eff+egg)=geh\,.\,ehf; \qquad (191)$$

while the equation (127), already derived as a sub-type from II., gives, by interchanging e and f,

$$(egf)(fee+fgg)=(geh)(hee+hgg). \qquad (192)$$

Multiplying the latter of these two equations by $eff+egg$, and the former by $fee+fgg$, and subtracting, we eliminate the symbol (egf), and find that

$$(geh)\{(ehf)(fee+fgg)-(eff+egg)(hee+hgg)\}=0; \qquad (193)$$

and a similar elimination of (geh) gives the equation,

$$(egf)\{(ehf)(fee+fgg)-(eff+egg)(hee+hgg)\}=0. \qquad (194)$$

And because $(geh)=-(egh)$, by (34), we may make any separate or combined interchanges, of e with g, and of f with h, and so vary the expression within the { }, without introducing any new factor, distinct from (egf) and (egh), outside them. If, then, for any particular arrangement of the four unequal indices, e, f, g, h, as chosen from among the four numbers 1, 2, 3, 4, the *two* following conditions are not *both* satisfied,

$$(egf)=0, \quad (egh)=0, \qquad (195)$$

we must have, for that arrangement of the indices, a system of *four* other equations, whereof one is

$$\text{VI..}\qquad (ehf)(fee+fgg)=(eff+egg)(hee+hgg), \qquad (196)$$

while the three others are formed from it, by the interchanges just now mentioned. And if we further suppose that the two sums, $fee+fgg$ and $hee+hgg$, are for the same arrangement different from zero, and write for abridgment, *as a definition*,

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* Comprising paragraphs [17.] to [25.], and published in the Phil. Mag. for October, 1854.

$$(ehf)_0 = (fee+fgg)^{-1}(eff+egg)(hee+hgg), \quad . \quad (197)$$

the four equations furnished by the formula (196), which may be regarded as a *sixth type for quines*, may be concisely expressed as follows:

$$\left.\begin{array}{ll}(ehf)=(ehf)_0, & (ghf)=(ghf)_0; \\ (efh)=(efh)_0, & (gfh)=(gfh)_0.\end{array}\right\} \quad . \quad . \quad . \quad (198)$$

With the notations $l_1 \, .. \, u_3$, for the symbols (efg), (eff), we find thus that unless l_1 and p_1 both vanish, we must have the four equations,

$$\left.\begin{array}{ll}t_2(n_1-m_1)=(m_2-n_2)(r_2+r_3); & s_3(n_1-m_1)=(m_3-n_3)(r_2+r_3); \\ p_3(r_2+r_3)=(u_2+n_2)(n_1-m_1); & p_2(r_2+r_3)=(m_3-u_3)(n_1-m_1);\end{array}\right\} (199)$$

and that unless $s_1=0$, $t_1=0$, then

$$\left.\begin{array}{ll}-l_2(u_2-m_2)=(n_1+u_1)(n_3+u_3); & t_3(u_2-m_2)=(r_1+r_2)(n_3+u_3); \\ l_3(n_3+u_3)=(u_1-m_1)(u_2-m_2); & s_2(n_3+u_3)=(r_1+r_3)(u_2-m_2).\end{array}\right\} (200)$$

[31.] Supposing then that no one of the twelve symbols (efg) vanishes, and that each of the twelve sums $eff+egg$ is also different from zero, the various arrangements of the four indices $efgh$ give us a system of twenty-four equations, included in the new type VI., or in any one of the four formulæ (198); which equations may, by (34), be arranged in twelve pairs, as follows:

$$(ehf)=(ehf)_0=-(hef)_0. \quad . \quad . \quad . \quad . \quad (201)$$

It might seem that *twelve* equations between the twelve symbols of the form (eff) should thus arise, by the comparison of two expressions for each of the twelve symbols of the form (efg); but if we write for abridgement

$$[g]=(fee+fgg)(fhh+fgg)\{(ehf)_0+(hef)_0\}, \quad . \quad (202)$$

and observe that by the definition (197) of the symbol $(ehf)_0$, we have then

$$\begin{aligned}[g] = {} & (eff+egg)(fhh+fgg)(hee+hgg) \\ & +(hff+hgg)(fee+fgg)(ehh+egg), \quad . \quad . \quad (203)\end{aligned}$$

we shall see that this quantity $[g]$ is independent of the arrangement of the three indices e, f, h; and therefore that the twelve equations between the twelve symbols (eff), obtained through (201), reduce themselves to the *four* following relations,

$$[e]=0, \quad [f]=0, \quad [g]=0, \quad [h]=0; \quad . \quad . \quad (204)$$

which are not even all distinct among themselves, since any *three* of them include the fourth. An easy combination of the two first or of the two last of these four relations (204) conducts to this other formula, which is equivalent to three distinct equations:

$$\begin{aligned}&(eff+ehh)(fee+fgg)(ghh+gee)(hgg+hff)=\\&(eff+egg)(fee+fhh)(ghh+gff)(hgg+hee)\,; \quad . \quad (205)\end{aligned}$$

and which may also be thus written,

$$(hef)_0(egf)_0=(ehf)_0(gef)_0. \quad . \quad . \quad . \quad (206)$$

With the notations $l_1 \, . \, . \, u_3$, the twenty-four equations (201) are sufficiently represented by the formulæ (199) and (200), if cyclical permutation of the indices be employed; the four equations (204) take the forms,

$$\left.\begin{aligned}&(r_1+r_2)(n_3+u_3)(m_2-n_2)=(r_1+r_3)(m_2-u_2)(m_3-n_3),\\&(n_1+u_1)(n_2+u_2)(n_3+u_3)=(m_1-u_1)(m_2-u_2)(m_3-u_3)\,;\end{aligned}\right\} (207)$$

whereof the equation on the second line may be obtained from the product of the three represented by the first line: and the three equations (205) or (206) are included in the following, which is evidently a consequence of (207),

$$(r_1+r_3)(n_1+u_1)(n_2+u_2)(n_3-m_3)=(r_1+r_2)(u_1-m_1)(n_2-m_2)(u_3-m_3). \quad (208)$$

[32.] As regards the quotients and products of the symbols (*efg*), which we shall continue to write occasionally without parentheses, we have by type VI., or by (197) (198),

$$\frac{ehf}{ghf}=\frac{eff+egg}{gff+gee}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (209)$$

$$ehf\,.\,efh=(eff+egg)(ehh+egg)\,; \quad . \quad . \quad . \quad . \quad (210)$$

$$ehf\,.\,gfh=(eff+egg)(ghh+gee)\,; \quad . \quad . \quad . \quad . \quad (211)$$

eliminating (*ehf*) between the two last of which three equations, we obtain a relation of the same form as the first. Interchanging *g* and *h* in (210), and subtracting, we find that

$$\text{I.}\,. \qquad ehf\,.\,efh-egf\,.\,efg=(egg)^2-(ehh)^2\,; \quad . \quad . \quad (212)$$

but this is precisely what the type I., or the formula (113), becomes for quines, when we cyclically advance the four indices in the order *fegh*; the conditions (117) (118) of that *first type* will therefore be satisfied, if we satisfy those of the *sixth*. Had we divided instead of subtracting, we should have found

$$\frac{ehf\,.\,efh}{egf\,.\,efg}=\frac{eff+egg}{eff+ehh}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (213)$$

To interchange *f*, *g*, and divide, would only lead by (210) to another equation of this last form; but the same operations performed on (211) conduct to the equation

$$\frac{ehf}{heg}=\frac{ghh+gee}{fhh+fee}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (214)$$

which, when we interchange g and h, reproduces the formula (192); and shows thereby that the sub-type (127), included under type II., is satisfied by our new type VI., which indeed it had assisted to discover. The same equation (192) may also be derived from the formula (205), by dividing each member of that formula by $(fee+fhh)(hff+hgg)$, and attending to the expressions given by type VI., for (egf) and (geh) respectively. To interchange e, h, in (211), and divide, would only conduct to another equation of the same form as (214). Permuting cyclically the three indices e, f, g in (209), and multiplying together the two equations so obtained therefrom, the product gives

$$\frac{fhg\,.\,ghe}{ehg\,.\,fhe}=\frac{gff+gee}{eff+egg}; \quad . \quad . \quad . \quad . \quad . \quad . \qquad (215)$$

and if we multiply this equation by (209) itself, we find that

$$ehf\,.\,fhg\,.\,ghe=ghf\,.\,ehg\,.\,fhe. \quad . \quad . \quad . \qquad (216)$$

In fact if we operate thus on the expression (197) for $(ehf)_0$, or for its equal (ehf), or on the formula (196), we are led to this new equation,

$$ehf\,.\,fhg\,.\,ghe=(hee+hgg)(hff+hee)(hgg+hff), \quad . \qquad (217)$$

of which the second member does not alter, when we interchange any two of the three indices e, f, g. Another multiplication of three equations of the form (209), with the cycle egh, conducts to the equation $[f]=0$ of (204). Interchanging e, h in (210), and substituting the value so obtained for the product of the two extreme factors of the second member of (217), we find this other expression,

$$ehf\,.\,fhg\,.\,ghe=hef\,.\,hfe\,.\,(hee+hff)\,; \quad . \quad . \qquad (218)$$

which is still seen to remain unaltered, by an interchange of e and f. Interchanging f, g, and dividing, we obtain by (216) an equation of the same form as (213); and if we divide each member of (218) by (hef), we are conducted to the formula

$$\text{IV.}.\qquad fhg\,.\,hge=hfe\,.\,(hee+hff), \quad . \quad . \quad . \quad . \qquad (219)$$

which is of the same form as the equation (191), or as the type IV., and may be changed thereto by cyclical permutation of the four indices $efgh$. The same relation (219) may also be derived more directly from type VI., by substitutions of the values (198); for it will be found that the definition (197) gives this *identity*,

$$(fhg)_0(ghe)_0=(hee+hff)(fhe)_0. \quad . \quad . \quad . \qquad (220)$$

The conditions of type IV., like those of type I., and of the subtype (127) of II., are therefore all included in those of the new type VI.; which gives also in various ways this other formula respecting products of four symbols of the form (efg),

$$egh\,.\,fhg\,.\,gfe\,.\,hef=ehg\,.\,fgh\,.\,gef\,.\,hfe: \quad . \quad . \quad . \qquad (221)$$

indeed it will be found that the members of this last equation, taken in their order, are respectively equal by (196) to the members of the equation (205).

With the notations $l_1 \ldots u_3$, supposing that none of the twelve constants $lpst$ vanish, and that the twelve combinations of the forms n_1-m_1, n_1+u_1, u_1-m_1, r_1+r_2, are in like manner different from zero, we find thus, or from the equations (199), (200), combined with their consequences (207), the following among other relations, in which cyclical permutation of the indices is still allowed:

$$\left.\begin{array}{lll} \dfrac{l_1}{t_2}=-\dfrac{n_3+u_3}{r_1+r_3}, & \dfrac{t_2}{s_3}=\dfrac{m_2-n_2}{m_3-n_3}, & \dfrac{s_3}{l_1}=\dfrac{r_1+r_2}{u_2-m_2}, \\ \dfrac{p_1}{p_2}=\dfrac{m_2-u_2}{n_1+u_1}, & \dfrac{l_1}{p_1}=\dfrac{r_2+r_3}{n_1-m_1}, & \dfrac{s_1}{t_1}=\dfrac{n_3+u_3}{m_2-u_2}; \end{array}\right\} \qquad (222)$$

$$\left.\begin{array}{ll} l_2\,l_3=(n_1+u_1)(m_1-u_1), & s_2\,t_3=(r_1+r_2)(r_1+r_3), \\ p_1s_2=(m_2-n_2)(m_2-u_2), & p_1t_3=(m_3-n_3)(n_3+u_3), \\ -l_2\,s_2=(n_1+u_1)(r_1+r_3), & l_3\,t_3=(u_1-m_1)(r_1+r_2), \\ p_1s_1=(m_1-n_1)(n_3+u_3), & p_1t_1=(m_1-n_1)(m_2-u_2). \end{array}\right\} (223)$$

The conditions (152), (153), of the *fourth type*, are satisfied; and we have these other products, of which some have occurred already, in (176), (177), in connexion with the particular systems (A) and (B) of quines:

$$\left.\begin{array}{c} s_1s_2s_3=t_1t_2t_3=-(r_1+r_2)(r_2+r_3)(r_3+r_1); \\ s_1p_2l_3=t_1l_2p_3=(n_1+u_1)(m_1-u_1)(m_1-n_1); \\ s_2s_3l_2\,p_3=t_2t_3p_2l_3; \end{array}\right\} \quad . \qquad (224)$$

where the two members of the equation on the last line are easily proved by (223) to be respectively equal to those of (208).

[33.] As yet we have only partially satisfied the conditions of type II., or of the formula (123), which may for quines be written thus:

$$\text{II.} \ldots \qquad fgh\,.\,hff=fee\,.\,gee-fgg\,.\,gff+feh\,.\,ghe. \quad . \qquad (225)$$

Substituting for the last product in this formula its value given by (211), namely

$$feh\,.\,ghe=(fgg+fhh)(gee+gff), \quad . \quad . \quad . \quad . \qquad (226)$$

and writing for abridgement

$$v_f=fee+fgg+fhh, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (227)$$

we are in this way led to establish the following *seventh type** for quines:

$$\text{VII.}\ldots\quad fgh\,.\,hff = v_f\,.\,gee + fhh\,.\,gff. \quad\ldots\quad (228)$$

Or since, by the sixth type, we have already

$$\text{VI.}\ldots\quad fgh\,.\,(hee + hff) = (fee + fhh)(gee + gff), \quad (229)$$

it is only necessary, for the purpose of satisfying the conditions of type II., or the equations (143) (145), to suppose besides that

$$\text{III.}\ldots\quad fgh\,.\,hee = fee\,.\,gff - fgg\,.\,gee; \quad\ldots\quad (230)$$

such being the expression which remains, when we subtract (228) from (229). But this last equation (230) is precisely what the type III., or the formula (147), becomes for quines; it reproduces therefore the equations (148), with a correction elsewhere noticed (namely the substitution of s_2n_3 for s_2r_3): and conversely, if we retain that old type III., it will not be *necessary*, although it may be convenient, to introduce the new type VII., in combination with type VI. And if in (230) we substitute for the symbol (fgh) its value given by (229), and so combine types III. and VI., we obtain the equation

$$\frac{fee + fhh}{hee + hff} = \frac{gff\,.\,(fee + fgg) - fgg\,.\,(gee + gff)}{hee\,.\,(gee + gff)}; \quad . \quad (231)$$

that is, by (209),

$$\frac{fge}{hge} + \frac{fgg}{hee} = \frac{gff}{hee}\,.\,\frac{fhe}{ghe}; \quad\ldots\ldots\ldots\quad (232)$$

or finally,

$$\text{V.}\ldots\quad hee\,.\,fge = fgg\,.\,ghe - gff\,.\,fhe. \quad\ldots\quad (233)$$

But this is exactly what the type V., or the formula (154), becomes for quines, when we suppress the sum Σ'', change k to h, and advance cyclically the three indices efh; it includes therefore the equations (155), which were the only remaining conditions of association to be fulfilled. If then we satisfy the *two types*, III. and VI., we shall satisfy *all the conditions of association for quines*: since we shall thereby have satisfied *also* the *four other earlier types*, namely those numbered as I. II. IV. V. It only remains then to consider *what new restrictions* on the constants (eff) are introduced by the comparison of the values which type III. gives for the other constants (efg), as expressed in terms of them, with the values furnished by type VI.; or to discuss the consequences of the following general formula, obtained by eliminating the symbol (fgh) between (229) (230), and not essentially distinct from the recent equation (231):

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* It will be shown that this *single type* (the seventh) *includes all the others*, or is sufficient to express *all* the *general conditions* of association, between the 24 symbols of the forms (efg) and (eff). But the eliminations required for this deduction cannot be conveniently described at this stage.

$$\text{VIII}\ldots\quad hee.(fee+fhh)(gee+gff)=(hee+hff)(fee.gff-gee.fgg);$$

which contains all the old and new relations, subsisting between the twelve constants of the form (eff), and may be regarded as an *eighth type* for quines.

[34.] Denoting the first minus the second member of (234) by the symbol $[efgh]$, we easily see that

$$[efgh]=gee.(v_f.hee+fgg.hff)+gff.(fhh.hee-hff.fee)$$
$$=hee.(v_f.gee+fhh.gff)+hff.(fgg.gee-gff.fee); \qquad (235)$$

and therefore that we have, *identically*,

$$[efgh]=[efhg]; \quad . \quad . \quad . \quad . \quad . \qquad (236)$$

this last or eighth type (234) contains therefore, at most, only a system of twelve equations. Interchanging f and g, and attending to the notation (202), we see, by (203) (234), that of the three equations

$$[efgh]=0, \quad [egfh]=0, \quad [e]=0, \quad . \quad . \qquad (237)$$

any two include the third; if then we only seek what *new* conditions, additional to those marked (204), are to be satisfied by the symbols (eff), or rather by the eight following ratios of those symbols,

$$eff:egg:ehh;\ fee:fgg:fhh;\ gee:gff:ghh;\ hee:hff:hgg, \qquad (238)$$

we need only retain at most four new equations, suitably selected from among those furnished by type VIII., such as the four following, which differ among themselves by the initial letters within the brackets, and so belong to different groups,

$$[efgh]=0, \quad [fghe]=0, \quad [ghef]=0, \quad [hefg]=0; \qquad (239)$$

and then to combine these with any three of the four former relations (204), for example with the three first, namely

$$[e]=0, \quad [f]=0, \quad [g]=0; \quad . \quad . \quad . \qquad (240)$$

from which the fourth equation $[h]=0$ would follow, by means of the *identity*,

$$(ehf)_0(fge)_0(gfh)_0(heg)_0=(ehg)_0(fgh)_0(gfe)_0(hef)_0$$
$$=(eff+egg)(fee+fhh)(gee+ghh)(hff+hgg). \quad . \quad . \qquad (241)$$

It might seem however that the seven equations (239) and (240), thus remaining, should suffice to determine seven of the eight ratios (238): whereas I have found that it is allowed to *assume two pairs of ratios* arbitrarily, out of the four pairs (238), and then to deduce the two other pairs from them. For I find that it is sufficient to retain, instead of the *twelve* equations included under type VIII., or the *seven* equations (239) (240), a system of only *four* equations of the type just mentioned; namely *two pairs*, selected from any two of the four groups, which have (for each group, and also for each pair) a common initial letter within the brackets; for instance, these two pairs of equations:

$$[efgh]=0,\ [egfh]=0;\quad [fegh]=0,\ [fgeh]=0; \quad . \quad (242)$$

which leave as many as *eight arbitrary constants* (for example these eight, *eff, egg, ehh, fhh, ghh, hee, hff, hgg,* from which all the rest can be determined) *in the resulting system of associative quines.* An outline of the investigation by which this important reduction is effected, may be presented in the following way.

[35.] The two first equations (242) connect the three last pairs of ratios (238), in such a manner that when any two of those three pairs are assumed, or known, the third can be determined. Hence, with the interpretation (197) of the symbol $(ghf)_0$, we easily find that those two equations (242) give,

$$(e)\ldots\left\{\begin{array}{r} gee\,.\,hgg-ghh\,.\,hee=(ghf)_0\cdot fee;\\ v_g\,.\,hee+gff\,.\,hgg=(ghf)_0\cdot fgg;\\ -(v_h\,.\,gee+hff\,.\,ghh)=(ghf)_0\cdot fhh; \end{array}\right\}\quad . \quad (243)$$

because we find that *fee, fgg, fhh* are proportional to the left-hand members of these last equations (243); and that the sum of the two first of those left-hand members is identically equal to the product $(gee+gff)(hee+hgg)$. For the same reason, the two first of these three equations (243) express really only one relation, namely that which is contained in the second equation (242), although they do so under different forms, both of which it is useful to know; and it is convenient to have ready also this other combination, obtained by adding the three equations (243) together,

$$(e)\ldots\quad v_h\cdot gff-v_g\cdot hff=v_f\cdot(ghf)_0; \quad . \quad . \quad (244)$$

which, like those equations (243) themselves, we shall consider as belonging to the group (*e*), because they are all derived from two of the three equations of that group, included under type VIII., which in the recent notation $[efgh]$ have *e* for their initial letter; and because the third equation of that group, included under the same type, namely

$$(e)\ldots\quad [ehfg]=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (245)$$

may be derived from them, by the elimination of the symbol $(ghf)_0$ between the first and third of the equations (243). In like manner the two last equations (242) include a third of the same type VIII., and belonging to the same group (*f*), namely

$$(f)\ldots\quad [fheg]=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (246)$$

because they conduct to the following system of expressions, which may be formed from (243) (244) by cyclical permutation of *efg*:

$$(f)\dots\left\{\begin{array}{r} eff\,.\,hee-ehh\,.\,hff=gff\,.\,(ehg)_0\,;\\ v_e\,.\,hff+egg\,.\,hee=gee\,.\,(ehg)_0\,;\\ -(v_h\,.\,eff+hgg\,.ehh)=ghh\,.\,(ehg)_0\,;\\ v_h\,.\,egg-v_e\,.\,hgg=\ v_g\,.\,(ehg)_0. \end{array}\right\}\quad .\qquad (247)$$

Multiplying then the equations (243) and (244) by $(ehg)_0$, and observing that the identity (220) gives

$$(ehg)_0\,.\,(ghf)_0=(hee+hff)(ehf)_0,\quad .\quad .\qquad (248)$$

we find, on substitution of the first for the second members of (247), that the results are divisible by $hee+hff$; and that thus the elimination of the third pair of ratios (238), between (243) (244) (247), or between the four equations (242), conducts to expressions of the recent forms, namely,

$$(g)\dots\left\{\begin{array}{r} hee\,.\,egg-ehh\,.\,hgg=fgg\,.\,(ehf)_0\,;\\ v_e\,.\,hgg+eff\,.\,hee=fee\,.\,(ehf)_0\,;\\ -(v_h\,.\,egg+hff\,.\,ehh)=fhh\,.\,(ehf)_0\,;\\ v_h\,.\,eff-\ v_e\,.\,hff=\ v_f\,.\,(ehf)_0. \end{array}\right\}\quad .\qquad (249)$$

A similar analysis may be applied to effect the elimination of the fourth pair of ratios (238), with results entirely analogous. On the whole then it is found, that the four equations (242) express such connexions between the four pairs of ratios (238), as to satisfy not only the two remaining equations, (245) and (246), of their *own* groups, (*e*) and (*f*), but also the six other equations of the two *other* groups, (*g*) and (*h*), included under type VIII.; namely

$$\left.\begin{array}{l} (g)\dots.\ \ [gefh]=0,\quad [gfeh]=0,\quad [ghef]=0\,;\\ (h)\dots.\ \ [hefg]=0,\quad [hfeg]=0,\quad [hgef]=0\,; \end{array}\right\}\qquad (250)$$

for the first line is satisfied by the ratios (249), and the second line by the analogous ratios, which are found in a similar way. Thus all the *twelve* equations of type VIII. are satisfied, if we satisfy only *four* suitably selected equations of that type; for example, the equations (242): which was what we proposed to demonstrate.

[36.] The *eighty* equations of association, assigned in the Third Section, between the *twenty-four* constants $l_1\dots u_3$, or (*efg*), (*eff*), have therefore, by the recent analysis, been ultimately *reduced to sixteen*; namely the *four* equations which thus remain from the last type VIII.; and the *twelve* others which were contained in the type III., established in that earlier Section: and which (as was lately remarked) leave still no fewer than EIGHT

CONSTANTS ARBITRARY in this theory of ASSOCIATIVE QUINES. We may indeed vary in many ways, consistently with the same general theory, and by the assistance of the other recent types VI. and VII., the system of the sixteen equations of condition which are to be satisfied, and the choice of the eight constants which are to be regarded as still remaining arbitrary and undetermined: and it may not be useless, nor uninteresting, to make some remarks hereafter, upon the subject of such selections. But in the mean time it appears to be important to observe, that if some of the recent results, especially the formulæ (210), (228), be combined with some of those previously obtained, and more particularly with the equations (112), (121), of Section III., the following very simple expressions are found, for the ten remaining *constants of multiplication*, the discussion of which had been reserved:

$$(f)=v_f^2;\quad (fg)=v_f\,.\,v_g;\quad .\quad .\quad .\quad .\quad .\quad (251)$$

or, with the notations *abc*, and with the usual cyclical permutation of the indices 1, 2, 3,

$$a_1=v_1^2,\quad a_4=v_4^2,\quad b_1=v_2v_3,\quad c_1=v_1v_4.\quad .\quad .\quad (252)$$

If then we write for abridgement,

$$\left.\begin{aligned} v&=v_1x_1+v_2x_2+v_3x_3+v_4x_4,\\ v'&=v_1x'_1+v_2x'_2+v_3x'_3+v_4x'_4, \end{aligned}\right\}\quad .\quad .\quad .\quad (253)$$

the square of any quadrinomial vector ϖ, and the scalar of the product of any two such vectors, will take these remarkably simple forms:

$$\varpi^2=v^2;\quad \mathrm{S}\varpi\varpi'=v\,.\,v';\quad .\quad .\quad .\quad .\quad (254)$$

this latter scalar thus decomposing itself into a product* of two linear functions of the constituents, namely those here denoted by v and v'. And because it is easy to prove, from what has been already shown, compare (244), that in the present theory the constants v_e are connected by relations of the form

$$-v_e\,.\,efe=v_e\,.\,fee=v_f\,.\,eff+v_g\,.\,efg+v_h\,.\,efh,\quad .\quad (255)$$

we find, by multiplying this equation by v_g, and attending to (251), the following theorems for those *general associative quines* which have been in this section considered:

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[*] A similar decomposition into linear factors takes place for the *quadrinomes* (A) of par. [13.], but at the expense of one of the six arbitrary constants $l_1\,l_2\,l_3\,m_1\,m_2\,m_3$, when we establish between those symbols the relation,

$$l_1m_1^2+l_2m_2^2+l_3m_3^2=l_1l_2l_3+2m_1m_2m_3.$$

In general, I find that it is possible to satisfy all the conditions of association for *polynomes*, and at the same time to secure a decomposition of $\mathrm{S}\varpi\varpi'$ into linear factors, while yet preserving so many as $3n-4$ constants of multiplication arbitrary. (For quadrinomes, $3n-4=9-4=5$; for quines, $3n-4=12-4=8$.)

$$\left.\begin{array}{l} 0 = \mathrm{S}\iota_g \mathrm{V}\iota_e\iota_f = \mathrm{S}\iota_g\iota_e\iota_f; \\ 0 = \mathrm{S}\varpi\varpi'\varpi''; \quad 0 = (\mathrm{V}\varpi\varpi')^2; \end{array}\right\} \quad . \quad . \quad . \quad (256)$$

results which may be compared with some of those obtained in Section IV., for the two particular quine-systems, (A) and (B).

Observatory of Trinity College, Dublin,
February 14, 1855.

[To be continued.]

XLIV. *Observations on the "Magnetic Medium," and on the Effects of Compression. By* Professor W. THOMSON.

THE following letter was received a few days ago. It was not written for publication, but the subject to which it refers being of general interest at present, I ventured to suggest to Professor Thomson the desirableness of having the letter printed. This he at once agreed to. With the exception of a paragraph relating to matters of a purely private nature, the letter appears as I received it.

JOHN TYNDALL.

March 24, 1855.

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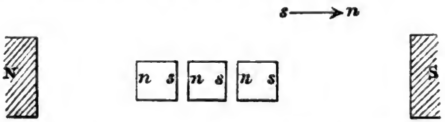
2 College, Glasgow,
March 12, 1855.

MY DEAR SIR,

Allow me to thank you for the abstract of your lecture on magnetism, and the copy of your letter to Mr. Faraday, which I have recently received from you, and have read with much interest. I am still strongly disposed to believe in the magnetic character of the medium occupying space, and I am not sure but that your last argument in favour of the reverse bodily polarity of diamagnetics may be turned to support the theory of universally direct polarity. There is no doubt but that the medium occupying interplanetary space, and the best approximations to vacuum which we can make, have perfectly decided mechanical qualities, and among others, that of being able to transmit mechanical energy in enormous quantities (a platinum wire, for instance, kept incandescent by a galvanic current in the receiver of an air-pump, emits to the glass and external bodies the whole mechanical value of the energy of current spent in overcoming its galvanic resistance). Some of these properties differ but little from those of air or oxygen at an ordinary barometric pressure. Why not, then, the magnetic property? (of which we know so little that we have no right to pronounce a negative). Displace the interplanetary medium by oxygen, and you have a slight increase of magnetic polarity in the locality with a drawing in of the lines of force. Displace it with a piece of bismuth or a piece of wood, and a slight decrease of magnetic polarity through the locality takes place, accompanied by a pushing out of the lines of force. A state of strain by compression may enhance, in the direction of the strain, that quality of the substance by which it lessens the magnetizability of the space from which it displaces air or "æther;" just as a similar state may enhance, in the direction of compression, the augmenting power of a paramagnetic substance.

By the bye, a long time ago (rather more than a year after the Edinburgh meeting of the British Association) I repeated with much pleasure some of your compression experiments, and found a piece of fresh bread instantly affected by pressure, so as always to turn the compressed line perpendicular to the lines of force, to whatever form the fragment was reduced. A very slight squeeze between the fingers was quite enough to produce this property, or again to alter it so as to make a new line of compression set equatorially. I repeated it a few days ago with the same results, and got a ball of bismuth, too, to act similarly. I remember formerly finding the bread *attracted* as a whole, instead of being repelled, as I expected from your results. I suppose, however, this must have resulted from some ferruginous impurities, which it may readily have got either in the course of the experiments with it, or in the baking. I mean to try this again*.

I do not quite admit the argument you draw from your compression experiments regarding the effect of contiguity of particles, because in fact we know nothing of the actual state of the molecules of a strained solid. You have made out a most interesting fact regarding their magnetic bearings; but experiments are neither wanted, nor can be made, to show any sensible effect whatever of the mutual influence of a row of small pieces of bismuth placed near one another, or touching one another. It is perfectly easy to demonstrate that it *must* be such as to impair the "diamagnetization" of each piece when the line of the row is parallel to the lines of force, and to enhance it when that line is perpendicular to the lines of force, but in each case to so infinitesimally minute a degree, as to be wholly inappreciable to the most refined tests that have ever been applied. For let the lines of force be parallel to the line shown in the figure,



and act on a steel needle in the manner there represented. Then, whatever hypothesis be true for diamagnetism, there is not a doubt but that each piece is acted on, and consequently reacts, precisely as a piece of steel very feebly magnetized, with its magnetic axis reverse to that of a steel needle free to turn, substituted for it, would do. Each piece of bismuth therefore acts as a little magnet, having its polarity as marked in the diagram, would do. Hence the magnetizing force by which the middle fragment is influenced is less than if the two others were away (this being such a force as would be produced by a north pole on the right-hand side of the diagram, and a south pole on the left). It is easily seen, similarly, that if the line joining the centres be perpendicular to the lines of force, the magnetizing force on the space occupied by the middle fragment is increased. Corresponding assertions are true for the terminal fragments, although the disturbing effect will be less on them in each case than in the middle one. Hence the diamagnetization of each will be enfeebled in the former case and enhanced in the latter, by the presence of the others. It follows, according to the principle of superposition of magnetizations, that if the line of the row be placed obliquely across the lines of force, the magnetic axis of each particle, instead of being exactly parallel to the lines of force, will be a little inclined to them, in the angle between their direction and the direction transverse to the bar. The magnets causing the force of the field must act on the little diamagnets, each with its axis thus rendered somewhat oblique, so as to produce on it a statical couple (as shown by the arrow heads), and the resultant of the couples thus acting on the fragments will, when all these are placed on a frame, or rigidly connected, tend to turn the whole mass in such a direction as to place the length of the bar along the lines of force. Still, I repeat, this action, although demonstrated with as much certainty as the parallelogram of forces, is so excessively feeble as to be absolutely inappreciable. A fragment of bismuth, of any shape whatever, held in any position whatever in any kind of magnetic field, uniform or varying most intensely, only exhibits the resultant action of couples on all its small parts if crystalline, and of forces acting always according to Faraday's law on them if the field in which it is placed be non-uniform. Some phænomena that have been observed are to be explained by the resultant of forces from places of stronger to places of weaker intensity in the field, others by the resultant of couples depending on crystalline structure, and others by the resultant of such forces and couples coexisting; and none observed depend at all on any other cause.

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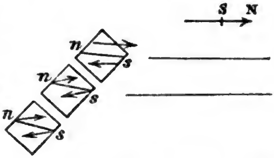
* Prof. Thomson's supposition is correct; pure bread is *repelled* by a magnetic pole. I may remark that I am at present engaged in the further examination of the influence of compression, and have already obtained numerous instructive results.—J. T.



I gave a very brief summary of these views (which I had explained somewhat fully and illustrated by experiments on paramagnetics of sufficient inductive capacity to manifest the effects of mutual influence, at the meeting at Belfast) as an abstract of my communication, for publication in the Report of the Belfast meeting of the British Association, where you may see them stated, I hope intelligibly. The experiments on the paramagnetics are very easy, and certainly exhibit some very curious phænomena, illustrative of the resultant effects due to the attractions experienced by the parts in virtue of a variation of the intensity of the field, and to the couples they experience when their axes are diverted from parallelism to the lines of force by mutual influence of the magnetized parts.

I had no intention of entering on this long disquisition when I commenced, but merely wished to try and briefly point out, that the assertions I have made regarding mutual influence are demonstrable in every case without special experiment, are confirmed amply by experiment for paramagnetics, and are absolutely incontrovertible, as well as incapable of verification, by experiment or observation on diamagnetics.

Believe me, yours very truly,

WILLIAM THOMSON.

Prof. Tyndall.

XLV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 236.]

Dec. 14, 1854.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—"The Physical Theory of Muscular Contraction." By Charles Bland Radcliffe, M.D.

The theory set forth in this paper is, that muscle is *prevented from contracting* by the several vital and physical agencies which act as stimuli upon muscle,—volition, nervous influence, blood, electricity, light, heat, and the rest,—and that *contraction happens on the cessation of stimulation*, by virtue of the operation of that universal principle of attraction which belongs to muscle in common with all matter, and, so happening, that it is a *physical* phænomenon of the same nature as that contraction which takes place in a bar of metal on the abstraction of heat.

This theory is supported by various arguments, some of which are now stated for the first time. It is argued:—

(*a*.) That *nervous influence* cannot cause muscular contraction, (1) because the degree of innervation, as measured by the supply of nerves, is inversely related to the tendency to contraction; (2) because contraction does not take place so long as the nerve gives evidences of electricity (Du Bois Reymond); (3) because, in some instances at least, contraction does not happen so long as the nerve gives evidences of "irritability"—for contraction is not caused by heat, by acids and alkalies, and by several other chemical and medicinal substances, until the possibility of provoking contraction by the touch of a needle has been destroyed by the action of the agent—until, that is to say, the "irritability" of the nerve has been destroyed by this action (Eckardt); and (4) because the influence of the nervous centre in causing contraction is to suspend the natural electricity of the nerve and muscle. This last conclusion is evident in the fact, that the signs of electricity, which are absent during tetanus, immediately reappear in the muscle and in the portion of nerve connected with it, when the influence of the nervous centre is cut off and the tetanus resolved by dividing the nerve.

(*b.*) That *blood* cannot cause contraction, (1) because the tendency to contraction is inversely related to the supply of blood; thus, this tendency is greater in the voluntary muscles of fishes and reptiles than of mammals and birds—greater in involuntary than in voluntary muscles—greater in the muscles of any given animal during the state of hybernation than during the period of summer life; and (2) because the state of *rigor mortis* may be relaxed more than once, and the lost "irritability" restored to the muscle by the injection of living blood into the vessels (Brown-Séguard).

(*c.*) That *electricity* cannot cause contraction, (1) because there is a constant current of electricity in a muscle during rest, *but not during contraction* (Du Bois Reymond),—because, that is to say, contraction is absent when muscle is in a state of electrical or *polar action*, and present when this state is absent, so that contraction appears to be antagonized by this state of polar action; and (2) because contraction is never coincident with the passage of a current of artificial electricity; for, not only is it true that a muscle does not contract during the time that a current of artificial electricity is passing through it, but *contraction is invariably relaxed if contraction preexisted* (Eckardt). There is, indeed, momentary contraction at the opening or at the closing of the circuit, but this contraction can be shown to be coincident with neutralization of electrical action, which neutralization is consequent upon the momentary opposition of the natural current of the muscle and the artificial current.

(*d.*) That *mechanical agents* cannot *stimulate* contraction, (1) because the electrical phænomena of muscle are opposed to such an idea; thus muscle affords evidences of electricity during rest, but not during contraction, and hence the probability is that electricity has been discharged when a muscle contracts on being touched by a needle,—a probability which is supported by the analogy which exists between the structure of muscle and the structure of the electrical organ of the Torpedo, and between the circumstances producing contraction on the one hand and discharge on the other (Owen, Faraday, and others); and (2) because the movements of the stomach, or uterus, or any other viscus are not to be accounted for on the supposition that the contractions are stimulated by the contents of the viscus; thus the food accumulates and the stomach expands until the appetite is satisfied, and contraction does not happen until the preliminary processes of digestion are at an end, and thus also the child grows and the uterus expands, and labour pains do not begin until the growth of the child is completed, and the *stimulus* of that growth suspended.

(*e.*) That *heat and cold* do not *stimulate* contraction, because contraction does not happen until the *natural polar action* of the muscle is suspended,—an event which happens equally under either extreme of temperature,—and thus the muscle would seem to contract because the heat or cold extinguishes that polar action of the muscle which antagonizes contraction.

(*f.*) That *light* cannot cause contraction, (1) because it exercises a directly opposite influence upon the irritable cushions of the sensitive plant; and (2) because it is as easy to agree with Bichât, and suppose that light expands the curtain of the iris, as that it causes contraction in sphincter-fibres surrounding the pupil, which fibres have no existence.

(*g.*) That *chemical and mechanical agencies* do not stimulate contraction, because contraction does not happen until the agent has destroyed that polar action of the muscle which antagonizes contraction (Eckardt).

It is argued, also, that the action of the *will* upon muscle is not necessarily that of a *stimulus*, for the will *may act* by withdrawing something from the muscle as well as by communicating something to the muscle, and, if so, then the previous considerations enhance the probability that it acts by withdrawing something.

In the course of the argument it is further shown that this conclusion is borne out by the history of the muscular movements which are manifested in the coats of vessels and in the heart, while at the same time this view is found to give the clue to the physical interpretation of "capillary action," and of rhythm, whether this be in the heart or elsewhere.

It is shown, also, that the same conclusion is borne out by the pathology of tremor, convulsion, and spasm,—of those diseases, that is to say, in which muscular contraction is in excess. Thus, (to mention one argument out of many,) the state of circulation which is invariably associated with tremor, convulsion, and spasm, is one which necessarily implies the diminution of all accustomed stimulation in the muscle, for it is a state which borders closely upon syncope or asphyxia.

And, lastly, it is shown that there is nothing in the phænomenon of muscular contraction which need prevent it from being referred to the operation of that common principle of attraction which belongs to muscle *in common with all matter*, and thus the general conclusion is that another barrier between the organic and inorganic world is broken down, and that muscular contraction is an effect of the universal law of gravitation.

There are, however, sundry grave objections to this theory, and one main object of the paper under consideration is to remove them. Thus, for example, if muscle contracts when nervous influence is withdrawn, how is it that it relaxes when the nerve is divided or otherwise paralysed? and if a muscle contracts for want of blood, how is it that it relaxes in syncope, asphyxia, and death? These objections are grave, but not unsurmountable, as the following hints at explanation will serve to show.

It must be understood, then, that that state of polar action which is present in a muscle during rest and absent during contraction, *is re-established immediately after contraction*; it must also be understood that this state of polar action in the muscle is suspended during ordinary muscular contraction by certain changes which take place in the nervous centre, and that it has *died out* when contraction happens after death, as in *rigor mortis*; and the rest is sufficiently simple.

It is quite in accordance with the theory, then, that a muscle should contract when nervous influence is withdrawn, and that it should relax after the nerve is divided or otherwise paralysed. At the moment when the continuity of the nerve is broken the muscle contracts, because the influence of the nervous centre is cut off; but this contraction cannot continue, because that state of polar action which antagonizes contraction is immediately re-established in the muscle, and in the portion of nerve connected with it. This relaxation, moreover, must continue, if the paralysed muscle be left to itself, so long as the muscle continues to be the seat of this polar action. And, on the other hand, this contraction must return when this action is suspended, or diminished, or extinguished, as indeed it does; thus the muscle contracts when the polar action is suddenly suspended by galvanism or by the touch of a needle; thus it contracts after the paralysis has continued for some time, and when the failure in the nutrition of the muscle has entailed a corresponding failure in its polar action; and thus it contracts in *rigor mortis*, when all polar action is finally extinguished.

It is also in accordance with theory that tremor, convulsion, and spasm should be caused by want of blood, and that they should cease when the circulation fails, as it fails in syncope, asphyxia, or death. During tremor, convulsion, or spasm, the muscles are insufficiently supplied with nervous influence, because the deficient supply of blood to the nervous centres involves a corresponding deficiency in the degree of innervation; but once let the circulation fail below a certain point, and the whole case is altered. During tremor, convulsion, and spasm, the supply of blood to the nervous centres is insufficient to keep up the normal degree of innervation, *but it is sufficient to prevent the nerves from being paralysed*, and hence the contractions in the muscles, for the nerves being conductors, the failure in the action of the nervous centres is propagated along them to the muscles, and of this failure the contractions are the consequence. But if the circulation fails below a certain point, *the nerves are paralysed for want of blood*, and being paralysed, the failure of innervation in the nervous centres, even though this be now complete, does not entail a corresponding failure in the polar action of the muscle, because the nerves are no longer conductors; and not doing this, the polar action of the muscle, which is much more vigorous than that of the nervous centre and nerve, and far less dependent upon the supply of blood, is immediately re-established, and being re-established, the muscle relaxes (just as it does in the case where paralysis is caused by division of the nerve), and tremor, convulsion and spasm are at an end. Nor is there any doubt that the nerves are paralysed when the circulation fails to the point which is here supposed. Thus, if the circulation in the hand be depressed by immersion in cold water, the sense of touch and the power of movement are partially or wholly destroyed; or if the principal vessel of a limb be tied, the nerves are similarly paralysed until the collateral circulation be established; and in each case, also, the power of provoking "reflex movements" is diminished or destroyed. In either case the nerves are more or less paralysed for want of blood, and, if so, it surely follows that the nerves must be paralysed, and still more effectually, when the circulation fails as it fails in syncope, asphyxia, or death, and when the movement of the blood is almost or altogether at an end. Hence it is quite intelligible that tremor, convulsion or spasm should be caused by want of blood, as is stated in the argument, and that they should cease in syncope, asphyxia, and death; and thus this objection falls to the ground, and with it all objections of the same kind.

Such is an imperfect sketch of the evidence upon which the physical theory of muscular contraction is founded.

Dec. 21, 1854.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"Remarks on the Anatomy of the *Macgillivrayia pelagica* and *Cheletropis Huxleyi* (Forbes); suggesting the establishment of a new genus of Gasteropoda." By John D. Macdonald, R.N., Assistant-Surgeon H.M.S. Herald.

Having examined the anatomy of the *Macgillivrayia pelagica* and several smaller species of pelagic Gasteropoda, not exhibiting the least similarity in the character of their shells, the author found that they all presented a very close relationship to each other in the type of their respiratory organs, and in other points of structure of less importance.

The gills in every instance seemed to be fixed to the body of the animal immediately behind the head, and did not appear to be appended to the mantle, as in the Pectinibranchiata properly so called. They were also invariably four in number, and arranged in a cruciform manner round a central point. They were free in the rest of their extent, elongated and flattened in form, with a pointed extremity, and fringed with long flowing cilia, set in a frilled border. They were, moreover, furnished with muscular fibres, both transverse and longitudinal, and exhibited great mobility when protruded, but lay side by side in the last whorl of the shell when retracted.

The auditory capsules, each containing a spherical otolithe, were closely applied to the inner and posterior part of the larger or anterior ganglion of the subœsophageal mass.

There were two tentacula, each bearing at the outer side of its base an eye consisting of a globular lens with optic nerve and retinal expansion. The foot was large and very mobile, but a vesicular float has been observed only in *Macgillivrayia*.

The respiratory siphon was either a simple fold of the mantle forming a temporary tube (*Cheletropis*), or a fold whose borders were united through their whole length, leaving an aperture at the end, as in *Macgillivrayia*.

A lingual ribbon with well-marked rachis and pleuræ occurs in all the species. Very perfect labial plates with closely-set dental points arm the mouth in some instances, and probably in all.

The little animals possessing in common the characters here described, nevertheless fabricate shells so very different as to admit of their division into well-marked genera.

The author conceives that the obvious difference between the pectinibranchiate type of respiratory organs and that observed in the group of Gasteropoda now under consideration, affords sufficient grounds for placing the latter in a distinct order by themselves; and as illustrations of it he proceeds to give some details of the structure of the two species mentioned in the title of the paper, whose shells have been already described by the late Prof. E. Forbes, and figured in Mr. Macgillivray's 'Narrative of the Voyage of H.M.S. Rattlesnake.'

In *Macgillivrayia* the disc of the foot is broad and connected by a narrow attachment to the body just beneath the neck; it carries an operculum behind, and is cleft by a notch in front. A raphe observable in the median line, as well indeed as the whole character of this part of the organ, seems to shadow forth the transformation of the single foot of the Gasteropod into the wing-like expansion of the Pteropod.

After describing the labial plates and lingual strap, the eyes and the branchiæ, the author observes that the tubular siphon protrudes from the shell on the left side and seems to indicate the coexistence of a respiratory chamber with naked branchiæ.

The vesicular float, like that of *Ianthina*, noticed by Mr. Macgillivray, consists of an aggregation of vesicles varying both in number and size in different cases. It is exceedingly delicate, and could not be found in the specimens first obtained, having probably been destroyed or detached from the foot by the force of the water running through the meshes of the net with which they were captured. Its coexistence with an operculum shows that it is not a modification of the latter.

Of the *Cheletropis Huxleyi*, numerous specimens were found in Bass's Straits and in the South Pacific, between Sydney and Lord Howe's Island.

After giving some details respecting the shell and the foot, the author observes that the latter organ was destitute of float, at least in the specimens he obtained, but was furnished with an operculum, which, probably from its extreme thinness and smallness, had escaped the notice of Professor Forbes. He then points out the peculiarities of the respiratory apparatus.

The portion of the mantle which forms the respiratory siphon, is short, and its opposite edges are merely in apposition, without organic union. The branchiæ are of two kinds, covered and naked. The covered gill is single but of considerable length. It is beautifully pectinated, and fringed with long cilia, and, doubtless, represents the respiratory organ of the pectinibranchiate Gasteropoda. The basis of this part is a long and narrow strip of a tough and fibrous material, folded upon itself into a series of loops invested with a coating of epithelium, and richly ciliated along the free border. The naked gills are four in number, similar both in situation and character to those of *Macgillivrayia*. Each gill is of an oval or elongated form, presenting a thin, frilled and corrugated border, beset with long whip-like cilia. In the central parts muscular fibres are distinctly discernible, some disposed lengthwise and others transversely.

The lingual strap is next described, as well as two file-like triturating plates with which the mouth is furnished.

The two tentacula of each side appear as it were enclosed in one envelope, so as to form a single tactile instrument, which bears a large dark eye on its outer side near the base. To this latter organ the tegumentary covering forms a kind of cornea, beneath which is a spherical lens resting on a mass of black pigment, both being enclosed in a little sac; and the optic nerve, emerging from the subœsophageal ganglion, joins the miniature globe and expands into a retina. The author was unable to trace an opening through the pigment for the passage of light, but thinks it probable that, as in the ocelli of insects, such an aperture exists in the central part. The auditory capsules are situated at some distance behind the eyes, and may be distinctly seen with the microscope when the surrounding parts are carefully removed with fine needles. They are of a rounded or oval form, and each contains a beautifully transparent and highly refracting otolithe, much larger than the lens of the eye.

January 11, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read :—

"On the Development of Muscular Fibre in Mammalia." By William S. Savory, M.D.

The author's observations were made chiefly upon fœtal pigs, but they have been confirmed by repeated examinations of the embryos of many other animals, and of the human fœtus.

If a portion of tissue immediately beneath the surface from the dorsal region of a fœtal pig, from one to two inches in length, be examined microscopically, there will be seen, besides blood-corpuscles in various stages of development, nucleated cells and free nuclei or cytoblasts scattered through a clear and structureless blastema in great abundance. These cytoblasts vary in shape and size; the smaller ones, which are by far the most numerous, being generally round, and the larger ones more or less oval. Their outline is distinct and well defined, and one or two nucleoli may be seen in their interior as small, bright, highly refracting spots. The rest of their substance is either uniformly nebulous or faintly granular.

The first stage in the development of striated muscular fibre consists in the aggregation and adhesion of the cytoblasts, and their investment by blastema so as to form elongated masses. In these clusters the nuclei have, at first, no regular arrangement. Almost, if not quite as soon as the cytoblasts are thus aggregated, they become invested by the blastema, and this substance at the same time appears to be much condensed, so that many of the nuclei become obscured.

These nuclei, thus aggregated and invested, next assume a much more regular position. They fall into a single row with remarkable uniformity, and the surrounding substance at the same time grows clear and more transparent, and is arranged in the form of two bands bordering the fibre and bounding the extremities of the nuclei, so that now they become distinctly visible. They are oval, and form a single row in the centre of the fibre, closely packed together side by side, their long axes lying transversely, and their extremities bounded on either side by a thin clear pellucid border of apparently homogeneous substance.

It is to be observed how closely the muscular fibres of mammalia at this period of their development resemble their permanent form in many insects.

The fibres next increase in length and the nuclei separate. Small intervals appear between them. The spaces rapidly widen, until at last the nuclei lie at a very considerable distance apart. At the same time the fibre strikingly decreases in diameter; for as the nuclei separate, the lateral bands fall in and ultimately coalesce.

This lengthening of the fibre and consequent separation of the nuclei is due to an increase of material, and not to a stretching of the fibre.

Soon after the nuclei have separated some of them begin to decay. They increase in size; their outline becomes indistinct; a bright border appears immediately within their margin; their contents become decidedly granular; their outline is broken and interrupted; and presently an irregular cluster of granules is all that remains, and these soon disappear.

It sometimes happens that the nuclei perish while in contact, before the fibre elongates; but the subsequent changes are the same.

The striæ generally first become visible at this period, immediately within the margin of the fibre.

The fibre is subsequently increased in size, and its development is continued by means of the surrounding cytoblasts. These attach themselves to its exterior, and then become invested by a layer of the surrounding blastema. Thus, as it were, nodes are formed at intervals on the surface of the fibre. These invested nuclei are at first readily detached, but they soon become intimately connected and indefinitely blended with the exterior of the fibre. All its characters are soon acquired, the nuclei at the same time gradually sink into its substance, and an ill-defined elevation, which soon disappears, is all that remains.

Lastly, the substance of the fibre becomes contracted and condensed. The diameter of a fibre towards, or at the close of intra-uterine life, is considerably less than at a much earlier period.

At the period of birth muscular fibres vary much in size.

The several stages in the development of muscular fibre, above mentioned, do not succeed each other as a simple consecutive series; on the contrary, two, or more, are generally progressing at the same time. Nor does each commence at the same period in all cases.

"On the General Integrals of the Equations of the Internal Equilibrium of an Elastic Solid." By W. J. Macquorn Rankine, F.R.SS.L. & E.

The *First Section* of this paper is introductory, containing a summary of principles already known respecting the elasticity of solids. Those principles are treated as the consequences of the following DEFINITION OF ELASTICITY, without introducing any hypothesis as to the molecular structure of bodies.

"*Elasticity is the property which bodies possess of preserving determinate volumes and figures under given pressures and temperatures, and which in a homogeneous body manifests itself equally in every part of appreciable magnitude.*"

The investigations are limited by the following conditions:—

1. The temperature of the elastic body is supposed to be constant and uniform.
2. The variations of the volumes and figures of its particles are supposed to be so small, that the elastic pressures may be considered as sensibly linear functions of those variations.
3. It is assumed, that the only force, besides elastic pressures, acting on the particles of the body, is that of terrestrial gravitation.

All possible small variations of volume and figure of an originally rectangular molecule, when referred to three orthogonal axes, may be resolved into six, viz. three linear dilatations or compressions, and three distortions.

In like manner the elastic pressures exerted on and by such a molecule may be resolved into six, viz. three normal pressures, and three tangential pressures.

Those six pressures are connected with each other and with the attractive force acting on the molecule, by three well-known differential equations of the first order.

They are also connected at every element of the surface of the body, by three well-known linear equations, with the components of the external force acting on that element.

The general problem to be solved is, to find the integrals of the first three equations, subject to conditions fixed by the last three.

The six variations of volume and figure of a rectangular molecule are expressed by six small fractions called "coefficients of displacement."

If the differential of each of these fractions be multiplied by the pressure which directly tends to vary it, the sum of the products is the complete differential of a function called the *Potential Energy of Elastic Forces* for the molecule in question, which is sensibly a homogeneous quadratic function of the six fractions. It has twenty-one terms, and twenty-one constant coefficients, which constitute the *Coefficients of Elasticity* of the body, for the system of orthogonal axes chosen.

Twenty-one equations express the relations between the systems of coefficients of elasticity in a given body for any two different systems of orthogonal axes.

When a body possesses a system of orthogonal *axes of elasticity*, its coefficients of elasticity, when referred to these axes, are reduced to *nine*.

A body *isotropic* with respect to elasticity has but *three* coefficients of elasticity, which are the same for all sets of orthogonal axes, and are connected with each other by an equation.

If the Potential Energy of Elastic Forces be expressed as a homogeneous quadratic function of the *six elastic pressures*, its coefficients constitute the *coefficients* of *compressibility* and *extensibility*, and of *pliability*. They have relations to the coefficients of elasticity which are consequences of the properties of determinants.

The *Second Section* of the paper relates to the problem of the general integration of the equations of the internal equilibrium of an Elastic Solid, especially when it is *not isotropic*. The method of solution consists of the following eight processes:—

I. The centre of gravity of the body being (in general) taken for the origin of co-ordinates, the forces applied to the surface of the body are subdivided into nine systems of "REDUCED EXTERNAL PRESSURES," which are of such a nature, that for any integration of the external forces as originally expressed over a portion of the surface of the body, may be substituted the sum of three integrations of certain of the reduced external pressures over the three projections of that portion of the surface upon the co-ordinate planes.

By such integrations, extended to the whole of the body, are found the mean values of the nine reduced external pressures, which are connected by simple equations with the mean values, or *constant terms*, of the six internal elastic pressures.

The deviations of the reduced external pressures above and below their mean values, constitute nine systems of *variable parts* of those pressures.

II. The *eighteen* coefficients of the three co-ordinates in the *linear terms* of the six internal elastic pressures are determined by means of eighteen equations; viz. three equations of internal equilibrium between certain of these coefficients and the force of gravity, and fifteen equations formed by means of the conditions of equilibrium of portions of the body cut off by the co-ordinate planes, and planes parallel to them.

III. The six constant terms, and the eighteen linear terms, of the three dilatations or compressions and the three distortions, are computed from the corresponding terms of the internal pressures by elimination, or by means of the coefficients of extensibility and compressibility, and of pliability. The coefficients of the co-ordinates in those twenty-four terms bear linear relations to the coefficients in the linear and quadratic terms of the three projections of the molecular displacement.

IV. The parts of the nine reduced external pressures corresponding to the constant and linear terms of the internal pressures having been determined for each element of the body's surface and subtracted from the nine actual reduced external pressures, there remain nine *residual* reduced external pressures for each such element, which form three systems, each suitable for development in series of trigonometrical functions of a different pair of independent co-ordinates.

V. The parts of the three projections of the molecular displacement, which correspond to each system of residues of the reduced external pressures, are to be expressed by infinite series in terms of the sines and cosines of linear functions of the proper pair of independent co-ordinates, each order of terms containing (except in some special cases) four kinds of trigonometric functions, multiplied by six exponential functions of the third co-ordinate, whose parameters are the roots of an equation of the sixth order, and by twenty-four arbitrary constants.

From the expressions thus formed are to be computed symbolical expressions for the values of the system of residues or transcendental parts of the reduced external pressures, for each pair of independent co-ordinates, which, by the aid of the equation of the form of the surface of the body, are to be transformed into series containing terms in trigonometric functions of the independent co-ordinates only, multiplied by linear functions of the arbitrary constants, which are (in general) twenty-four times as numerous as the orders of terms.

VI. By equating the constant factor of each term of the symbolical developments thus formed, to the constant factor of the corresponding term of the arithmetical developments found by the process IV., there are formed as many linear equations between the arbitrary constants and known quantities as there are constants to be determined, from which equations those constants are found.

VII. Cases in which one ordinate intersects the surface of the body in two or more pairs of points are to be treated by a special method.

VIII. The results of the previous processes are to be combined, and the solution of the problem completed by determining and adding to them the displacements and rotations of the body as a whole.

The *Third Section* relates to the internal equilibrium of a rectangular prismatic body.

Processes I., II. and III. The determination of the constant and linear terms of the internal pressures, and the corresponding terms of the molecular displacements, consists in the special application of the methods of the preceding section. The axes of figure are taken for axes of co-ordinates.

IV. The means and differences of the transcendental residues of the reduced external pressures on each pair of faces of the prism are developed in series of trigonometric functions of the pairs of independent co-ordinates of the respective faces to which they are applied; the series employed being of such a nature, that for the edges of the body all their terms vanish.

V. and VI. An order having been fixed for the consideration of the forces acting on the three pairs of faces, let yz, zx, xy be that order.

Series of functions trigonometric in y and z, exponential in x, and satisfying the equations of internal equilibrium, with arbitrary constant coefficients, are taken to represent the molecular displacements produced by the residual pressures on the faces normal to x. From those series are computed series representing symbolically those residual pressures, which series being equated to the series numerically representing those pressures, the arbitrary constants are found by elimination.

The formulæ thus obtained are employed to compute ideal systems of external pressures on the faces normal to y and z, called "*Provisional pressures,*" which are developed in trigonometric functions of the independent co-ordinates of the faces to which they are conceived to be applied. Should the provisional pressures agree with the actual residual pressures on those faces, the process is complete; should they not so agree, the provisional pressures are to be subtracted from the actual residual pressures, leaving systems of remainders called "*Secondary pressures.*"

The series representing the molecular displacements corresponding to the *Secondary Pressures* on the faces normal to y are to be found in the manner already referred to. The formulæ thus obtained are to be employed to compute an ideal system of "*Provisional Secondary Pressures*" on the faces normal to z, which are to be developed in trigonometric functions of x and y.

Should the provisional secondary pressures thus found agree with the actual secondary pressures on the faces normal to z, the process is complete. Should they not so agree, the provisional are to be subtracted from the actual secondary pressures, leaving a system of remainders called "*Tertiary Pressures*" on the faces normal to z, whose effects are to be computed in the usual manner.

Process VII. is not required.

Process VIII. consists in combining the terms of the molecular displacements due to the constant and linear terms of the internal pressures, the residual pressures on the faces normal to x, the secondary pressures on the faces normal to y, and the tertiary pressures on the faces normal to z, and finally determining and adding to the other terms, those depending on the displacements and rotations of the prism as a whole.

The *Fourth Section* relates to the integrals of the equations of the internal equilibrium of an isotropic elastic solid.

The constant and linear terms of the internal pressures are to be determined by the methods described in the previous sections, for all solids, whether isotropic or not.

The transcendental terms of the internal pressures and molecular displacements in an isotropic elastic solid, require a special method for their determination.

The three projections of the molecular displacement, with all their functions, in an isotropic solid, are deducible from one primitive function or series of primitive functions of the co-ordinates, by certain processes of derivation, distributive, but not necessarily commutative.

Each primitive function must satisfy the condition

$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right)\Phi=0,$$

and may belong to one or other of *eight* classes, according as it is even or odd with respect to x, y and z.

The processes of derivation applicable to the primitive functions contain *three* arbitrary constants for each primitive function. Hence when there is a series of primitive functions of different orders, there are *twenty-four* arbitrary constants for each order of terms.

In the developments of the residual external pressures, there are also *twenty-four* constant coefficients for each order of terms, of which the arbitrary constants are linear functions.

The notation of M. Lamé's work on the Mathematical Theory of the Elasticity of Solid Bodies, so far as it relates to isotropic substances, is compared with that of this paper.

Reference being made to the known method of representing the elastic pressures at a given particle of a solid, in magnitude and direction, by the radii of an ellipsoid, and the positions of the surfaces to which those pressures are applied by those of the tangent planes to an ellipsoid or hyperboloid, the difference (not generally attended to) between the *cone of tangential pressures*, and the *cone of sliding*, is pointed out. This difference is important in the theory of the strength of materials.

January 18, 1855.—Sir Benjamin Brodie, Bart., in the Chair.

A paper was in part read, entitled "Ocular Spectres and Structures as Mutual Exponents." By James Jago, A.B. Cantab., M.B. Oxon.

The paper opens by stating that for want of a methodical elimination of ocular spectres from one another—a want which its aim is to meet—physiological optics remain to this day without any real foundation; and even when we have followed the rays of light through all the refracting media of the eye, we cannot safely assert what sensations belong to them until we have detected everything connected with the *percipient membrane* which may obstruct the action of light on it, or which may originate sensations *as of light* through other sorts of impulses. Our eyes in many important respects provide us with an opportunity for microscopical research that no optical instrument employed on the dead eye can rival. We may thus gather a variety of information, physical and physiological, solve points of ocular structure that escape other means of investigation, and bring a profusion of ingenious speculations to a termination, by showing that the phænomena (and this is especially true of the retinal phænomena) which have occasioned them are simply exponential of anatomical facts; and important physiological laws may be arrived at by like means.

The first step in the author's task is to determine the conditions which render objects existing upon or within the eye visible by their shadows, and to obtain optical principles by which we may examine the interior of our own eye with facility, so as to recognize in what lenticular structure, and what part of it, the cause of any shadow or "diffractive image" resides. He shows that we may make every measurement of interest, may decide all the points just alluded to, at the instant, as it were, by mere inspection; and he illustrates his optical principles by appropriate experiments.

The paper then commences its actual elimination of ocular spectres from one another, starting from the appendages of the eye and going on through the ocular tissues in succession to the retina, under several heads, as—

Optical Effects of the Eye-lashes, Eye-lids, and Conjunctival Fluids.

These produce phænomena of reflexion, refraction, and inflection. They may multiply the images of objects which are without or which are within the eye, and occasion us to see the latter. The conjunctival fluids render apt illustrations of "recondite" diffractive shadows.

Optical Structure of the Crystalline Lens.

It is shown that the stelliform figure of our crystalline lens is distinctly visible in divergent light. The lens contains numerous bodies displaying a series of diffractive fringes. The fringes of the border of the iris are likewise conspicuous. Whenever light radiates into the eye from a near point, all these things happen. Hence when a line of radiants (an edge of any body) is before the eye, a mosaic fringe of these coloured shadows will be formed; and there is an *ocular* fringe, as well as the fringe on the edge of a body by light inflected at the body. The ocular colours mentioned seem to have been the cause of the belief that it can be *proved* experimentally that the eye contains no provision for the correction of chromatic dispersion; whereas the colours spoken of should only be compared with those that are produced by *flaws* in the glasses of optical instruments.

The Structure of the Vitreous Body derived from Optical Phænomena.

On this head the author arrives at the following conclusions.

In the vitreous body are innumerable vesicular globules, ranging in size from 0·0008 to 0·005 of a line, which are arranged in unbroken series, in tubes more or less transparent. These tubes precisely resemble veins and arteries in their mode of ramification; they frequently anastomose and are united to one another by capillary plexuses, and they are of less specific gravity than the vitreous fluid. The trunks of this peculiar system of vessels probably arise in the region of, or *at* the base of the optic nerve, and ramify in the vitreous humour; the larger branches passing circumferentially within a limited distance of the hyaloid membrane, and yielding again many branches, which, after repeated subdivisions, end in a capillary network exceedingly subtle and close. Many of the terminal loops of the capillaries are attached to the hyaloid membrane, so as to confine the majority of the branches in a lax manner to its vicinity. A true idea of this system may be gained by conceiving that the veins and arteries here existing in the fœtal eye have in after life been developed according to the growth of the body, but also metamorphosed into these light, peculiar, globule-holding, transparent vessels, and deprived of all foreign support except at their roots and a part of their capillary loops. The intricate ramifications of these vessels have the mechanical effect of in a great degree restraining the relative motion of the humour which fills the hyaloid capsule, and compelling it to concur in the various movements of the eye-ball, so as to obviate the risk of concussion from eddies of the fluid in rapid movements of the eye, and consequent disturbing effects on the lens, the retina and its vessels.

The paper goes on to take this subject up in detail; supplies the dynamical laws which must be kept in view in the application of previously obtained optical methods to the required examination; shows that it is the system of ramifications described which has given rise to the peculiar appearances simulating concentric lamellæ in the vitreous humour previously subjected to chromic acid, so differently interpreted by microscopists. Here too the hitherto vagrant *muscæ volitantes* are, for the first time, invested with form, disposition and office. They are now shown to be the essential element in the structure of the vitreous body; and certain radical misconceptions, as to the nature of these appearances and the constitution of the vitreous body, are pointed out.

The Optical Anatomy of the Retina.

The existence of the *vasa centralia retinæ* in the substance of the retina, and the movements of the blood therein, occasion diversified phænomena. We may examine these vessels in our own eyes, in their minutest distributions, by means of a pin-hole, lens, &c., *in movement across the eye's axis*, in virtue of a physiological law hereafter determined. Currents of blood in these vessels, by pressure upon the nervous matter *at their sides*, produce remarkable phænomena, differing for the superficial and deep vessels (that is, according to the place of the vessel in the five layers of the retina lately discovered by microscopists). These phænomena may all be distinguished from one another, and assigned *with precision* each to its cause. The phænomena of this kind are *always* before us by daylight and night. In every use of the organs of sight these effects may be observed. In twilight, and *into* night, the pressure of the blood-currents on the retina first equals and then *excels* the impression made by the *failing* external light; and the whole circulatory system may be seen, with proper attention, definitely figuring itself in white or golden colour. A great concentration of light appears at the middle of the retina, which requires a bountiful supply of blood, and owing to the pointing of vessels towards the *foramen centrale*, there is an apparent gyration of *light* currents round a darker pivot. The whole conduct of the retinal circulation may be traced by the *blood-light*. And the manner in which the blood flows through the retina may be equable, or irregular and fitful; it may be very slow, and it may roll with great rapidity. A rhythmical or recurrent circulation of the retinal blood is very frequent, and produces very singular phænomena. We may produce the uncommon states of the retinal circulation at pleasure, by artifices described; and it is shown that it is the retinal circulation which is the cause of all the phænomena which have been taken to prove spontaneous, vibratory, &c. sensations of light.

From these elementary facts being overlooked, fundamental errors as to the conduct of the retina proper have prevailed on all hands. When external light is so faint that the retinal light from blood-pressure exceeds it, *the middle of the retina is so occupied with retinal light* as to be, comparatively with other parts of the retina, unavailable for the usual purposes, and we do not see *anything* with direct nearly so well as with oblique vision; and this inefficiency of the centre of the retina is not limited to the case of "stars of the last degree of faintness" (Herschel and South), but all small objects that are quite visible by "lateral" inspection appear to be "suddenly blotted out" by the eyes being turned directly upon them.

The rhythmical waves of light, or rhythmical progression of the retinal blood (and the mode of movement of the retinal blood as rendered by optical phænomena can be observed by other means), may occur, in a certain sense, spontaneously, or may be produced at will. The retinal circulation may be excited to show astonishing luminous effects.

Among other ways of causing a rhythmical or recurrent movement of this blood, is that by simple fatigue of the retina by overstraining the sight, when the retina, more or less suddenly (or after a few oscillations), becomes flooded with blood, and complete obliteration of all objects having less than a certain luminosity ensues. This circumstance has misled Brewster and Purkinje, separately, into the belief that they had discovered that a sensation excited in one portion of a retina may be "extended" or "irradiated" to an adjacent portion. Other cases which are imagined by J. Müller and Brewster to support this view are subjected to examination; the real cause of each of the phænomena mentioned being pointed out. Some peculiar effects of retinal light are given; and it is determined that *the rigid correspondence of the limits of sensation with those of the painted image*, is a physiological law literally *absolute*.

Unsuspected difficulties of a solitary eye, and certain well-known phænomena are explained upon the foregoing principles.

May sensation be excited in the trunk of the optic nerve, or centrifugally?

The arguments which have been presumed to prove the affirmative are shown, one by one, to be fallacious, while there is presumption of a negative sort. Observations are offered as to the correct explanation of various physiological points which have been otherwise interpreted, and reputed physiological contrasts of colour are considered.

Images of external objects are painted on the limitary membrane, and perceived by the radial fibres.

This head commences with the quotation of a passage from Sir David Brewster's 'Optics' which he offered towards an explanation of the difficulty of seeing a very faint star by direct vision; and it is shown that the retina is not liable, as Brewster imagines, to be thrown into a state of "undulatory" perception by our looking through the teeth of a "fine comb" or through a single "narrow aperture." The paper points out that the effect observed in these circumstances is produced by our looking near the edge of any body whatever, provided, *and only then*, that the object *move*, be it never so little, *across the eye's axis*. It shows that the same effect is produced by light radiating from a point, by a flame, by lenses, curved reflectors, *whilst they are in the act of moving across* the eye's axis; or by the movement of the eye itself, merely in relation to the light entering it,—even the naked eye along the sky. The effect produced is shown to be simply owing to this; that the retina, under such action, ceases to perceive in the spaces corresponding to its blood-vessels and capillaries, so that they completely display themselves in the semblance of black bodies (or lines); and the law is arrived at, that the images of external points which are painted on the vessels and capillaries are not perceived when the retina *loses light from one point of space and receives light from another point of space within a certain interval of time*, or that the percipient points lying in front of the vessels *require a certain time to perceive*. A physiological hypothesis is suggested to account for this phænomenon, on the presumption that the "radial fibres," which project from the layer of rods and cones and end in the limitary membrane, are the ultimate percipients of light.

It is pointed out how wonderfully close we may find the correspondence between the microscopical and optical anatomy of the retina. Each pair of identical fibres of the two optic nerves must be regarded as one nerve. Another supposed anomaly to the simplicity of nervous action being explained on anatomical principles, a statement of ordinary optical nervous action is made, and a summary evinces how the anomalies in visual experience are due to the complex additions to a simple organ of sight.

ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 2, 1855.—"On the Pendulum-experiments lately made in the Harton Colliery, for ascertaining the mean Density of the Earth." By G. B. Airy, Esq., F.R.S., Astronomer Royal.

The Lecturer commenced with remarking that the bearing of the experiments, of which he was about to give a notice, was not limited to their ostensible object, but that it applied to all the bodies of the solar system. The professed object of the experiments was to obtain a measure of the density of the earth, and therefore of the mass of the earth (its dimensions being known); but the ordinary data of astronomy, taken in conjunction with the laws of gravitation, give the relative proportions of the mass of the earth to the masses of the sun and the principal planets; and thus the determination of the absolute mass of the earth would at once give determinations of the absolute masses of the sun and planets. To show how this proportion is ascertained, it is only necessary to remark, that a planet, if no force acted on it, would move in a straight line; that, therefore, if we compute geometrically how far the planet moves in a short time, as an hour, and then compute the distance between the point which the planet has reached in its curved orbit, and the straight line which it has left, we have found the displacement which is produced by the sun's attraction, and which is therefore a measure of the sun's attraction. In like manner, if we apply a similar calculation to the motion of a satellite during one hour, we have a measure of the attraction of its primary. The comparison of these two gives the proportion of the attraction of the sun, as acting upon a body, at one known distance, to the attraction of a planet, as acting upon a body at another known distance. It is then necessary to apply one of the theorems of the laws of gravitation, namely, that the attraction of every attracting body is inversely as the square of the distance of the attracted body, and thus we obtain the proportion of the attractions of the sun and a planet, when the bodies upon which they are respectively acting are at the same distance from both: and finally, it is necessary to apply another theorem of the law of gravitation, namely, that the attractions thus found, corresponding to equal distances of the attracted bodies, are in the same proportion as the masses of the attracting bodies (a theorem which applies to gravitation, but does not apply to magnetic and other forces). Into the evidence of these portions of the law of gravitation, the Lecturer did not attempt to enter: he remarked only that they rest upon very complicated chains of reasoning, but of the most certain kind. His only object was to show that the proportion of the masses of all bodies, which have planets or satellites revolving round them, can easily be found (the proportion for those which have no satellites is found by a very indirect process, and with far less accuracy); and that if the absolute mass of the earth be known, the absolute mass of each of the others can be found. As their dimensions are known, their densities can then be found. Thus it rests upon such inquiries as those on which this lecture is to treat, to determine (for instance) whether the planet Jupiter is composed of materials as light as water or as light as cork.

The obvious importance of these determinations had induced philosophers long since to attempt determinations of the earth's density: and two classes of experiments had been devised for it.

The first class (of which there was only one instance) is the attraction of a mountain, in the noble Scheballien experiment. It rests, in the first place, upon the use of the zenith sector; and, in the next place, upon our very approximate knowledge of the dimensions of the earth. [The construction of the zenith sector was illustrated by a model: and it was shown, that if the same star were observed at two places, the telescope would necessarily be pointed in the same direction at the two places, and the difference of direction of the plumb-line, as shown by the different points of the graduated arc which it crossed at the two places, would show how much the direction of gravity at one place is inclined to the direction of gravity at the other place.] Now from our knowledge of the form and dimensions of the earth, we know that the direction of gravity changes very nearly one second of angle for every 100 feet of horizontal distance. Suppose then that two stations were taken on Schehallien, one on the north side and the other on the south side, and suppose that their distance was 4000 feet; then, if the direction of gravity had not been influenced by the mountain, the inclination of the directions of gravity at these two places would have been about 40 seconds. But suppose, on applying the zenith sector in the way just described, the inclination was found to be really 52 seconds. The difference, or 12 seconds, could only be explained by the attraction of the mountain, which, combined with what may be called the natural direction of gravity, produced directions inclined to these natural directions. In order to infer from this the density of the earth, a calculation was made (founded upon a very accurate measure of the mountain) of what would have been the disturbing effect of the mountain if the mountain had been as dense as the interior of the earth. It was found that the disturbance would have been about 27 seconds. But the disturbance was really found to be only 12 seconds. Consequently the proportion of the density of the mountain to the earth's density was that of 12 to 27, or 4 to 9 nearly. And from this, and the ascertained density of the mountain, it followed that the mean specific gravity of the earth would be about five times that of water. The only objection to this admirable experiment is, that the form of the country near the mountain is very irregular, and it is difficult to say how much of the 12 seconds is or is not really due to Schehallien.

The second class is what may be called a cabinet experiment, possessing the advantage of being extremely manageable, and the disadvantage of being exceedingly delicate, and liable to derangement by forces so trifling that they could with difficulty be avoided. Two small balls upon a light horizontal rod were suspended by a wire, or two wires, forming a torsion balance, and two large leaden balls were brought near to attract the small balls from the quiescent position. We could make a calculation of how far the great balls would attract the little ones, if they were as dense as the general mass of the earth; and comparing this with the distance to which the leaden balls really do attract them, we find the proportion of the density of the earth to the density of lead. The peculiar difficulty and doubt of the results in this experiment depend on the liability to disturbances from other causes than the attraction of the leaden balls, especially the currents of air produced by the approach of bodies of a different temperature; and after all the cautions of Cavendish, Reich and Baily, in their successive attempts, it seems not impossible that the phænomena observed may have been produced in part by the temperature of the great balls as well as their attraction.

These considerations induced the Lecturer, in 1826, to contemplate a third class of experiments, namely, the determination of the difference of gravity at the top and the bottom of a deep mine, by pendulum experiments. Supposing the difference of gravity found, its application to the determination of density (in the simplest case) was thus explained. Conceive a spheroid, concentric with the external spheroid of the earth, to pass through the lower station in the mine. It is easily shown that the attraction of the shell included between these produces no effect whatever at the lower station, but produces the same effect at the upper station as if all its matter were collected at the earth's centre. Therefore, at the lower station we have the attraction of the interior mass only: at the upper station we have the attraction of the interior mass (though at a greater distance from the attracted pendulum) and also the attraction of the shell. It is plain that by making the proportion of these theoretical attractions equal to the proportion actually observed by means of the pendulum, we have the requisite elements for finding the proportion of the shell's attraction to the internal mass's attraction, and therefore the proportion of the matter in the shell to the matter in the internal mass; from which the proportion of density is at once found. Moreover, it appeared probable, upon estimating the errors to which observations are liable, that the resulting error in the density, in this form of experiment, would be less than in the others.

Accordingly, in 1826, the Lecturer, with the assistance of his friend Mr. Whewell (now Dr. Whewell), undertook a series of experiments at the depth of nearly 1200 feet, in the Dolcoath mine, near Camborne, in Cornwall. The comparison of the upper and lower rocks (to which further allusion will be made) was soon found to be the most serious difficulty. The personal labour was also very great. They had, however, made a certain progress when, on raising a part of the instruments, the straw packing took fire (the origin of the fire is still unknown), and partly by burning and partly by falling, the instruments were nearly destroyed.

In 1828 the same party, with the assistance of Mr. Sheepshanks and other friends, repeated the experiment in the same place. After mastering several difficulties, they were stopped by a slip of the solid rock of the mine, which deranged the pumps and finally flooded the lower station.

The matter rested for nearly twenty-six years, the principal progress in the subjects related to it being the correction to the computation of "buoyancy" of the pendulum, determined by Colonel Sabine's experiments. But in the spring of 1854, the manipulation of galvanic signals had become familiar to the Astronomer Royal and the Assistants of the Greenwich Observatory, and it soon occurred to him that one of the most annoying difficulties in the former experiment might be considered as being practically overcome, inasmuch as the upper and lower clocks could be compared by simultaneous galvanic signals. Inquiries, made in the summer, induced him to fix on the Harton Colliery near South Shields, where a reputed depth of 1260 feet could be obtained; and as soon as this selection was known, every possible facility and assistance were given by the owners of the mine. Arrangements were made for preparing an expedition on a scale sufficient to overcome all anticipated difficulties. A considerable part of the expense was met by a grant from the Board of Admiralty. The Electric Telegraph Company, with great liberality, contributed (unsolicited) the skill and labour required in the galvanic mountings. The principal instruments were lent by the Royal Society. Two observers were furnished by the Royal Observatory, one by the Durham Observatory, one by the Oxford Observatory, one by the Cambridge Observatory, and one by the private observatory of Red Hill (Mr. Carrington's). Mr. Dunkin, of the Royal Observatory, had the immediate superintendence of the observations.

The two stations selected were exactly in the same vertical, excellently walled, floored, and ceiled; the lower station, in particular, was a most comfortable room, or rather suite of rooms. Every care was taken for solidity of foundation and steadiness of temperature. In each (the upper and the lower) was mounted an invariable brass pendulum, vibrating by means of a steel knife edge upon plates of agate, carried by a very firm iron stand. Close behind it, upon an independent stand, was a clock, carrying upon the bob of its pendulum an illuminated disc, of diameter nearly equal to the breadth of the tail of the invariable pendulum; and between the two pendulums was a chink or opening of two plates of metal, which admitted of adjustment, and was opened very nearly to the same breadth as the disc. To view these, a telescope was fixed in a wall, and the observer was seated in another room. When the invariable pendulum and the clock pendulum pass the central points of vibration at the same instant, the invariable pendulum hides the illuminated disc as it passes the chink, and it is not seen at all. At other times it is seen in passing the chink. The observation, then, of this disappearance determines a coincidence with great precision. Suppose the next coincidence occurs after 400 seconds. Then the invariable pendulum (swinging more slowly) has lost exactly two swings upon the clock pendulum, or the proportion of its swings to those of the clock pendulum is 398 : 400. If an error of a second has been committed, the proportion is only altered to 397 : 399, which differs by an almost insignificant quantity. Thus the observation, in itself extremely rude, gives results of very great accuracy. As the proportion of invariable-pendulum-swings to clock-pendulum-swings is thus found, and as the clock-pendulum-swings in any required time are counted by the clock dial, the corresponding number of invariable-pendulum-swings is at once found. Corrections are then required for the expansion of the metal (depending on the thermometer-reading), for the arc of vibration, and for the buoyancy in air (depending on the barometer-reading).

But when the corrected proportion of upper-invariable-pendulum-swings to upper-clock-pendulum-swings is found, and the proportion of lower-invariable-pendulum-swings to lower-clock-pendulum-swings is found, there is yet another thing required :—namely, the proportion of upper-clock-pendulum-swings to lower-clock-pendulum-swings in the same time ; or, in other words, the proportion of the clock rates. It was for this that the galvanic signals were required. A galvanometer was attached to each clock, and an apparatus was provided in a small auxiliary clock, which completed a circuit at every 15 seconds nearly. The wire of this circuit, passing from a small battery through the auxiliary clock, then went through the upper galvanometer, then passed down the shaft of the mine to the lower galvanometer, and then returned to the battery. At each galvanometer there was a small apparatus for breaking circuit. At times previously arranged, the circuit was completed by this apparatus at both stations, and then it was the duty of the observers at both stations to note the clock times of the same signals ; and these evidently give comparisons of the clocks, and therefore give the means of comparing their rates. Thus (by steps previously explained) the number of swings made by the upper pendulum is compared with the number of swings made in the same time by the lower pendulum.

Still the result is not complete, because it may be influenced by the peculiarities of each pendulum. In order to overcome these, after pendulum A had been used above and pendulum B below, they were reversed ; pendulum B being observed above, and A below ; and this, theoretically, completes the operation. But in order to ensure that the pendulum received no injury in the interchange, it is desirable again to repeat the experiments with A above and B below, and again with B above and A below.

In this manner the pendulums were observed with 104 hours of incessant observations, simultaneous at both stations, A above and B below ; then with 104 hours, B above and A below ; then with 60 hours, A above and B below ; then with 60 hours, B above and A below. And 2454 effective signals were observed at each station.

The result is, that the pendulums suffered no injury in their changes ; and that the acceleration of the pendulum on being carried down 1260 feet is $2\frac{1}{4}$ seconds per day, or that gravity is increased by $-\frac{1}{19190}$ part.

It does not appear likely that this determination can be sensibly in error. The circumstances of experiment were, in all respects, extremely favourable ; the only element of constant error seems to be that (in consequence of the advanced season of the year) the upper station was cooler by 7° than the lower station, and the temperature-reductions are therefore liable to any uncertainty which may remain on the correction for 7°. The reductions employed were those deduced by Sabine from direct experiment, and their uncertainty must be very small.

If a calculation of the earth's mean density were based upon the determination just given, using the simple theory to which allusion is made above, it would be found to be between six times and seven times the density of water. But it is necessary yet to take into account the deficiency of matter in the valley of the Tyne, in the hollow of Jarrow Slake, and on the sea-coast. It is also necessary to obtain more precise determinations of the specific gravities of the rocks about Harton Colliery than have yet been procured. Measures are in progress for supplying all these deficiencies. It seems probable that the resulting number for the earth's density will probably be diminished by these more accurate estimations.

XLVI. *Intelligence and Miscellaneous Articles.*

ON THE ARTIFICIAL PRODUCTION OF MINERALS BELONGING TO THE FAMILY OF SILICATES AND ALUMINATES BY THE REACTION OF VAPOURS UPON ROCKS. BY M. DAUBRÉE.

MODERN geology admits as proved, that the rocks in contact or in the neighbourhood of the massive crystalline strata have undergone some modification; to this theoretical idea they owe their name of *metamorphic rocks*.

Ever since the remarkable experiments of James Hall, an elevated temperature has been admitted as the principal agent of metamorphism. Nevertheless the intervention of heat alone cannot explain all the details of the extraordinary modifications which rocks have undergone in different countries; complex chemical actions have also evidently contributed to the alteration of the primitive type.

In some previous researches presented to the Academy, I paid attention principally to the reproduction of the minerals peculiar to the stanniferous deposits and the reciprocal action of the vapours. The new experiments, of which the present memoir contains the results, start from the same theoretical idea, of which they extend the application to a series of crystalline rocks.

When chloride of silicium in a state of vapour and at a red heat acts upon the bases which enter into the composition of rocks, it is decomposed, forming chloride of calcium and silicic acid. Sometimes this acid remains free, sometimes it combines with the excess of the base and forms simple or complex silicates. The most remarkable point connected with this reaction in a chemical, and especially in a geological point of view, is that the silicic acid thus formed, and the silicates produced from it, have an extreme tendency to crystallization. The crystals are small, but generally very distinct. It must be observed, moreover, that the crystallization of these compounds takes place at a temperature far below their point of fusion.

With lime, magnesia, alumina and glucina, *crystallized quartz* is obtained in the ordinary form of the pyramidal hexagonal prism, and a portion of the base passes into the state of silicate. In this manner the silicate of lime called *Wollastonite* is obtained in the form of rhombic tables, with two broad bevelled faces replacing the obtuse angles, which is the usual form of the native crystals. These tables are often grouped perpendicularly to each other, like the prisms of *staurolite*. In the same way, with magnesia, *peridote* is obtained in rectangular prisms.

Alumina gives a silicate in elongated prisms, with oblique bases, which are not attacked by acids, are infusible, and possess all the characters of *disthene*. It is very interesting in this case to see chloride of aluminium formed at the expense of the silicium.

To form a double or multiple silicate, it is necessary not only to mix the bases in suitable proportions, but also to furnish the oxygen necessary for the formation of the silicic acid, by the addition of an excess of one of the bases or of lime.

A mixture of lime and magnesia furnishes crystals of *diopside* perfectly colourless and transparent; they present the broad truncation and the bevel usual with augite. Seven equivs. of potash or soda and one equiv. of alumina, or one equiv. of alkali, one equiv. of alumina, and six equivs. of lime, produce by the action of chloride of silicium, crystals in oblique prisms with obtuse bevelments, which are scarcely acted upon by acids, and fuse before the blowpipe, presenting, in fact, all the characters of the *feldspaths*.

By the same process, varying the proportions and the nature of the bases submitted to the chloride of silicium, I obtained silicates presenting the crystallographic characters of *willemite*, *idocrase*, *garnet*, *phenakite*, *emerald*, *euclase* and *zircon*.

By mixing the elements corresponding with the compositions recently given by Rammelsberg for the magnesian and ferromagnesian tourmalines, adding an excess of magnesia or lime to furnish oxygen to the silicium, I obtained in the midst of crystals of quartz some very distinct hexagonal prisms, presenting in other respects all the external and chemical characters of *tourmaline*.

Chloride of aluminium may be used in the same manner as chloride of silicium. When passed over lime at a red heat, it produces chloride of calcium and aluminium, in crystals belonging to two types proper to *corundum*—the prism, and the acute double pyramid. The same reaction takes place with magnesia; and besides, in this latter case, a portion of the regenerated alumina may combine with the excess of magnesia, so as to produce *spinel*, recognizable by its crystals in regular octahedra truncated at the edges. Nevertheless it is preferable, in order to obtain spinel, to submit a mixture of chlorides of aluminium and magnesium in contact with lime to a red heat. With chlorides of zinc and aluminium, we obtain zinciferous spinel or *gahnite*.

Chloride of titanium conveyed over lime, furnishes, along with some other crystals which will be studied hereafter, oxide of titanium in the form of *brookite*. Oxide of tin, obtained in a similar manner, is in crystals of the same form as that which I had previously produced by the action of aqueous vapour. Thus the rectangular prismatic form is persistent for the acids of titanium and tin, produced by the decomposition of the chlorides of these metals, at temperatures included between 572° and 1652° F.

By the reaction of perchloride of iron upon lime I have obtained hæmatite, either in very distinct specular crystals like those of Saint Gothard, or in transparent hexagonal laminæ, presenting by refraction a ruby-red colour. Perchloride of iron mixed with chloride of zinc, furnishes, under the same conditions, a crystalline compound analogous to *franklinite*.

Lastly, crystallized magnesia, or the *periclase* of La Somma, may easily be obtained by the reaction of lime upon chloride of magnesium, which is found amongst the abundant chlorinated vapours of the fumaroles of Vesuvius. The same chloride, decomposed by aqueous vapour, also furnishes periclase, and chloride of zinc gives crystalline oxide of zinc.

The results just described lead to geological consequences to which I can only refer very briefly in this place. I do not pretend to say that all the silicates composing the mass of the crystalline rocks are formed by vapours. But even in the midst of the fused rocks of Vesuvius, a certain number of minerals, to which M. Scacchi has recently directed attention, are met with which appear to be products of sublimation.

Amongst the minerals of the oldest formation, there are many also which could not by fusion have lined the fissures in which they are now found so well isolated; such are the diopside pyroxene, with garnet of Piedmont and the Ural, the adular feldspaths and the pericline of the Alps, the epidotes and axinites of l'Oisans, and many others.

The great richness of the crystalline limestones in minerals often foreign to the neighbouring rocks, cannot result entirely from the fact that the lime, by reacting upon the silica, has served for the formation of peculiar silicates. Whatever might have been the original impurities of the limestones, corundum, spinel, periclase, or chondrodite could not have been produced in them without the subsequent introduction of foreign chemical agents.

All these various products of transport,—silicates, aluminates, oxides, and other combinations, formed either in fissures or in the midst of rocks now become very compact, are explained, it appears to me, in the most satisfactory manner by the intervention of chlorinated or fluorated emanations. Besides, in the case of such volatile and penetrating compounds, there is nothing to oppose the idea that their action may have extended itself over spaces of considerable thickness from their centre of disengagement, as in the crystalline schistose rocks of the Alps or of Brazil. Sometimes the substitution of the silicates thus formed has only been partial, as in many of the crystalline limestones, which remain as perpetual witnesses of the ancient exhalations which have escaped from the neighbouring eruptive rocks. Sometimes the attack has been more complete, and the primitive mass has even disappeared in the state of soluble chloride, just like the water which has formed the specular iron ore of the volcanos.

If we turn to the crystalline limestones and dolomites most largely furnished with minerals, such as those of Saint Gothard, Sweden, Finland, or the United States, we find that the introduction of chlorides, mixed with fluorides and sometimes with sulphuretted compounds, accounts for the formation of their most characteristic minerals. In this explanation we must include the rich deposits of red oxide of zinc with franklinite in New Jersey, as well as different masses of specular iron, and protoxide of iron, which have also been produced in the limestone.

We find magnesian compounds, such as spinel, chondrodite, mica, pyroxene, amphibole, warwickite, and serpentine accumulated, sometimes in large quantities, in limestones which contain no magnesia. This fact, hitherto unexplained, would only be a consequence of the different chemical affinities of lime and magnesia, for throughout our experiments we see chloride of magnesium precipitated by lime; and when these bases are both in the presence of chloride of silicium or aluminium, the lime yields its oxygen, and the magnesia remaining in the state of oxide enters by preference into an oxidized combination with the regenerated silica or alumina. The same principle explains the presence of magnesia, with exclusion of lime, in the protoxidized irons. Are we to attribute to the same cause the preponderance of magnesia over lime in the elements of granite and in serpentine?

The mode in which quartz and the silicates are connected principally with the granite rocks, has long been a difficulty in all the hypotheses upon the formation of the strata called primitive. Now we find in our experiments that quartz crystallizes at the same time, or even later than the silicates, at a temperature scarcely exceeding a cherry-red heat, and consequently infinitely below its point of fusion.

Is not this also the cause which appears sometimes to protect quartz from the influence of the basic silicates or of the aluminates, as in granite when it envelopes crystals of cymophane instead of forming a double silicate, like emerald or euclase?

If mica still gives off, by heat, fluorides of silicium, boron or lithium, shall we venture to affirm that the granitic pastes have not also originally contained chlorides of silicium, boron or aluminium? These are deficient, it is true, amongst the vapours collected in the present day in the neighbourhood of the apertures of volcanos, where they are decomposed and precipitated by aqueous vapour in contact with the atmosphere, but where they very probably contribute to the formation of the silicates, already considered by the best observers as products of volatilization. Do we not also find chlorine in considerable quantity in certain masses, such as the zirconian syenite of Norway, and the rock of Ilmen (miascite), where this body is principally combined in *elæolite*, and where it appears to have carried with it zirconium, tantalium, and all that series of rare elements which belong to these rocks?

It has never been shown that the presence of a certain quantity of water is an obstacle to such reactions at a high temperature, since we find silica and alumina separate, in an anhydrous state, from a watery solution at a temperature of 572° to 752° F. And if hitherto the experiments have principally borne upon the *extreme conditions* of the different modes of formation, by the dry and humid way, a similar effect produced in these extreme states, such as quartz and corundum, may perhaps authorize us in concluding that it would also take place in the intermediate conditions.—*Comptes Rendus*, July 17, 1854, p. 135.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1855.

Chiswick.—February 1. Clear: overcast. 2. Drifting snow. 3. Hazy. 4. Dense fog: foggy throughout. 5. Overcast: drizzly. 6. Snow-flakes: slight rain. 7. Cloudy: clear and frosty. 8. Snowing: drifting snow: boisterous at night. 9. Overcast: sharp frost. 10. Clear and frosty: severe frost at night. 11. Thermometer within 1 degree of zero: clear. 12. Overcast: clear and cold: partially overcast. 13. Snowing: cloudy: clear: intense frost: thermometer at zero. 14. Clear and cold. 15. Snow-flakes: bright sun: frosty haze. 16. Snow-showers: overcast. 17. Snowing, with cold wind: clear: severe frost. 18. Frosty: hazy: severe frost at night. 19. Uniform haze: clear and frosty. 20. Clear and frosty. 21. Overcast. 22. Hazy. 23. Overcast: snowing. 24. Fine. 25. Overcast. 26. Rain. 27. Foggy: rain. 28. Drizzly: large halo round the moon in the evening.

Mean temperature of the month	28°·01
Mean temperature of Feb. 1854	37 ·67
Mean temperature of Feb. for the last twenty-nine years ...	39 ·07
Average amount of rain in Feb.	1·54 inch.

Boston.—Feb. 1. Cloudy. 2. Fine. 3. Cloudy: rain and hail P.M. 4. Foggy: rain P.M. 5. Foggy: rain A.M. 6, 7. Cloudy: snow P.M. 8. Cloudy: stormy P.M. 9—11. Cloudy. 12—14. Fine. 15. Fine: snow A.M. 16. Fine: thermometer early A.M. 6°·5. 17—22. Fine. 23. Cloudy. 24. Fine: snow A.M. 25. Cloudy. 26. Cloudy: rain P.M. 27. Cloudy: snow A.M. 28. Cloudy.

Sandwick Manse, Orkney.—Feb. 1. Damp A.M.: sleet-showers P.M. 2. Thaw, bright A.M.: clear, frost P.M. 3. Thaw, showers A.M.: damp P.M. 4. Thaw, damp A.M.: showers P.M. 5. Showers A.M. and P.M. 6. Sleet-showers A.M.: clear P.M. 7. Clear, frost A.M.: snow-showers P.M. 8. Bright, frost A.M.: cloudy, frost P.M. 9. Clear, frost A.M.: cloudy, frost P.M. 10. Cloudy A.M. and P.M. 11. Snow-showers A.M. and P.M. 12. Snow-showers A.M.: snowing, aurora P.M. 13. Snow-drift A.M.: clear P.M. 14. Snow, bright A.M.: clear, aurora P.M. 15. Snow, bright A.M: cloudy P.M. 16. Snow, bright A.M.: clear P.M. 17. Snow, thaw A.M.: cloudy P.M. 18. Snow, clear A.M.: clear P.M. 19. Snow, clear A.M.: cloudy P.M. 20, 21. Snow, clear A.M.: clear, aurora P.M. 22. Snow, clear A.M.: lunar halo P.M. 23. Snow, cloudy A.M.: snow drift P.M. 24. Snow drift A.M. and P.M. 25. Snow, cloudy A.M.: snow, clear P.M. 26. Snow-showers A.M.: snow, clear P.M. 27. Snow, bright A.M.: snow clouds P.M. 28. Thaw A.M.: rain P.M.

Mean temperature of Feb. for twenty-eight previous years .	38°·24
Mean temperature of this month	31 ·64
Mean temperature of Feb. 1854	39 ·22
Average quantity of rain in Feb. for fourteen previous years	3·39 inches.

The mean temperature of this month is lower than that of any month for the last twenty-eight years—the whole period of observation—except February 1838, when it was 31°·31, and when there was snow during all the month and for three weeks previously. This month it lay from the 11th till the last day, and the drift on the 23rd and 24th formed high wreaths in many places, rendering the roads impassable to vehicles.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. eb.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·002	29·786	29·60	30·06	30·22	33	17	30	35	35½	ne.	ene.	w.	....	....	·08
○ 2.	29·990	29·752	29·85	30·22	30·21	32	27	27	37½	30	e.	ene.	e.	·32	....	·08
3.	29·510	29·429	29·38	30·00	29·73	39	23	34	35	35	e.	ene.	e.	·05	....	·10
4.	29·384	29·229	29·13	29·53	29·60	39	33	32·5	36½	36	e.	ne.	e.	·02	·10	·03
5.	29·308	29·287	29·03	29·70	29·76	40	31	35·5	36	35½	sw.	n.	nne.	·01	·73	·04
6.	29·542	29·318	29·20	29·86	29·98	35	29	33	36½	26	ne.	n.	nne.	·02	·31	·03
7.	29·811	29·744	29·55	30·09	30·20	36	27	33	35	33	ne.	nne.	nne.	....	·15	·04
8.	29·792	29·662	29·64	30·24	30·24	32	27	31	33½	32½	e.	e.	sse.	....	·11	·03
9.	29·833	29·871	29·68	30·34	30·24	30	15	28·5	31½	32	ne.	e.	......	....	....	....
☾ 10.	29·867	29·635	29·64	30·00	29·89	30	1	27	37½	35	ne.	ene.	ne.	....	....	·01
11.	29·534	29·472	29·35	29·81	29·83	35	20	24	34½	30	ne.	nne.	nne.	....	....	....
12.	29·481	29·465	29·30	29·75	29·70	36	23	28	33	29	ne.	ene.	ne.	....	....	·12
13.	29·443	29·356	29·22	29·58	29·58	29	0	28	29	23	ne.	calm	......	....	....	·15
14.	29·400	29·322	29·10	29·57	29·69	36	17	21·5	26	23	n.	n.	e.	....	....	....
15.	29·925	29·699	29·49	29·80	29·87	34	10	22	25	20	n.	calm	sw.	....	....	....
● 16.	29·954	29·811	29·72	29·88	29·83	32	19	29	31	28	ne.	se.	ssw.	....	·09	·11
17.	29·897	29·869	29·70	29·70	29·74	27	2	24	35	34½	ne.	ne.	wnw.	....	....	·06
18.	30·010	29·980	29·80	29·91	29·95	29	3	12	34½	32	ne.	w.	nw.	....	....	·01
19.	29·966	29·883	29·77	29·97	30·01	33	20	24	31½	29	e.	e.	......	....	....	·07
20.	29·732	29·691	29·58	29·96	29·91	35	8	23·5	28	24½	e.	ne.	......	....	....	....
21.	29·855	29·837	29·60	29·94	30·00	30	10	24	31	27	ne.	nne.	ssw.	....	....	....
22.	29·934	29·877	29·72	29·98	29·93	34	23	11	30	20	e.	nnw.	e.	....	....	·03
☽ 23.	29·986	29·853	29·65	29·66	29·70	37	18	28	29½	30	ne.	s.	w.	....	....	·02
24.	30·007	29·391	29·70	29·46	29·37	44	33	26	28	32	sw.	ssw.	wnw.	·60	·12	....
25.	29·354	29·314	29·05	29·37	29·46	49	37	37	33	27½	sw.	n.	n.	·10	....	....
26.	29·532	29·469	29·25	29·48	29·66	40	32	35	30	32½	nw.	ne.	wnw.	·04	·15	....
27.	29·811	29·602	29·50	29·69	29·56	42	37	33	27	35½	s.	s.	se.	·06	·13	....
28.	29·777	29·737	29·43	29·41	29·17	50	29	36	38½	36	sw.	s.	sse.	·13	·29	·31
Mean.	29·738	29·619	29·48	29·820	29·823	35·64	20·39	27·7	32·44	30·85				1·35	2·18	1·32

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[FOURTH SERIES.]

MAY 1855.

XLVII. *On the Interference of Light near a Caustic, and the Phænomena of the Rainbow. By* Professor POTTER, *A.M.*[*]

AT the meeting of the British Association for the Advancement of Science at Cambridge in 1833, I exhibited the experiment of the interference of light near a caustic, which I had a short time before discovered when examining the circle of aberration of a *spherical* mirror for a reflecting microscope. This circle of aberration was formed by using a luminous point, which was an image of the sun given by a small globule of mercury, for an object of which the image produced by the spherical mirror was to be examined. In place of the circle of aberration being a disc brightest at the edge and shading away gradually towards the centre, as we should expect from geometrical optics, it appeared a magnificent phænomenon of the interference of light, consisting of brilliant and black rings coloured on their edges.

The outer ring, instead of being brightest at its outer edge, as we should have expected from the undulatory theory, since the rays had travelled over equal spaces, shaded away gradually. I concluded that I had fallen upon a confirmation of what I had before found in experiments with the two mirrors slightly inclined, namely, that the central band of interference, where the rays have travelled over equal distances, was in normal circumstances a *black* band and not a *bright* one, as it had been asserted to be by the late M. Arago[†] and Professor Airy[‡], in accordance with the undulatory theory of light.

[*] Communicated by the Author.

[†] See *Annales de Chimie et de Physique* for 1819, vol. xi. p. 12.

[‡] Tract on the Undulatory Theory of Optics, articles 51 and 54.

Whilst an undergraduate at Cambridge, on studying the theory of the rainbow I found that it belonged to the same class of interferences near a caustic, and was not aware, until informed by Professor Whewell, that Dr. Young had applied the principle of ray interference to explain the occurrence of supernumerary rainbows seen frequently within the primary bow, and sometimes exterior to the secondary bow, of which the Newtonian theory of the rainbow furnishes no account.

In a paper published in vol. vi. p. 140 of the Cambridge Transactions, I have shown the necessity of the doctrine of interference to solve the phænomena, and have traced the caustic for the primary bow. At the time of writing that paper in 1835, I had not entered into any adequate examination of the point whether the rainbow occurred at or within the caustic, but did so soon afterwards, and I urged that it was found within the caustic in a discussion in the Cambridge Philosophical Society in the spring of 1836. In the Philosophical Magazine for July 1838 will be found the calculations on which this conclusion was founded, showing that there was darkness at the place formerly attributed to the rainbow.

This has been fully confirmed by M. Bravais* in numerous measures of the radii of fog-bows, which he found to be less than calculated for the rainbow, and to vary from 34° to 42°. Not being aware that the primary rainbow was also within its calculated place, he supposed that fog-bows required the occurrence of *vesicular* vapour for their formation. The existence of any such vapour has not, however, been demonstrated, and is generally disbelieved. His arguments from the absence of colour in the fog-bow, or *arc-en-ciel blanc*, are not of any weight as to its origin being different from that of the rainbow, for I believe the outer edge is always tinged with red.

Mr. Airy, the Astronomer Royal, has given an investigation in vol. vi. p. 379 of the Cambridge Transactions, of the intensity of light near a caustic, by employing M. Fresnel's principle, that each point in the front of a wave may be considered as the origin of a new wave, and the intensity at any given point is the resulting intensity from the surface of the wave in any of its previous positions considered as made up of such origins. This method of discussing interferences, which fails in so many cases when examined with impartial eyes, gave the maximum intensity of the primary bow to be within the caustic, but that the brightness was still nearly half the maximum at the place of the caustic, and that the light extended on its outside, shading gradually to darkness at an indefinite distance.

The measures before referred to were a sufficient guarantee that this solution did not represent the natural phænomena. Professor Miller, however, undertook an experimental examination of the analogous phænomena, in M. Babinet's experiment, in which they are observed in a small cylindrical vertical stream of water, and he has published a paper in vol. vii. of the Cambridge Transactions, detailing the measures of some of the results. He found the light to extend outside the calculated place of the caustic, in accordance with Mr. Airy's investigation, and that the *dark bars* or minima near the caustic were nearly in the relative places as given by that investigation; but he has not given the places of the *bright bars* or maxima, which I find do not agree with their calculated places.

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* See *Répertoire d'Optique Moderne*, by the Abbé Moigno, p. 1610.

At that time I commenced a repetition of these experiments, and found like results with Professor Miller as to light extending outside the caustic. I found, however, as will be seen below, that the light extended further and further outside it as the velocity of the stream was greater. The discrepancy between these results and those from the natural phænomena evidently arose from the refractive index being slightly different, and a little less for water falling in such a stream and about to break into drops, from that for still water.

I did not at that time think of any method of experimenting with water at rest, which I now find is so easy to be employed, but I prepared a prism and small cylinder of plate-glass; the prism served to determine the refractive indices of the rays to be used, and the cylinder gave the irises in position in accordance with those from the natural phænomena of the rainbow, and not in accordance with those from the running stream. The experiments extended to within a few days of my leaving Cambridge to commence the duties of my Professorship at University College. They have been only resumed lately in consequence of my arriving at this part of physical optics in a general investigation of the whole science, which I hope before much longer time has elapsed to lay before the scientific public in a continuous treatise.

The following were obtained in Queens' College, Cambridge. A heliostat reflected the sun's light through a narrow vertical aperture, which falling at a distance of about 5 feet on a fine Munich prism with its edge vertical, furnished a very pure prismatic beam of light. Of this beam the rays from orange to azure were reflected at right angles by a plane metallic mirror across one room and into another which was dark, and there fell at about 41 feet from the prism on the small vertical stream of water. This stream of water was directly over the centre of the circle of an excellently divided protractor, which, being laid horizontal, had on the arms which carried the verniers a support for a small telescope, of length 4½ inches, which magnified rather more than twice, and adjusted to show distant objects distinctly. It had micrometer lines at the common focus of the object and eye-glasses crossed at an angle of about 35°, the bisection of which was vertical. The image to be viewed in the telescope was brought to the bisection of this angle, and the measures were taken from the iris on one side of the perpendicular ray to that on the opposite side of it; the angle traversed being divided by two, gave the distance from the perpendicular ray for each iris.

On the 17th of September, 1841, in two good experiments with green light for the fixed line E, the extreme visible light was found to extend to 42° 21½′ in one experiment, and to 42° 21′ in the other, from the ray falling perpendicularly on the stream; and the calculated place of the primary rainbow, if it were at the caustic, for this colour is 41° 40′, therefore light was seen 41′ beyond the caustic.

In two experiments with orange light, fixed line D, the extreme visible light was found to extend to 42° 41′ in one, and 42° 58½′ in the other from the normal ray. The calculation for the caustic gives 42° 2′ nearly, and hence the light was seen 39′ and 56½′ outside the caustic.

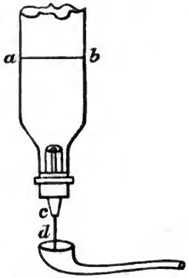
Another experiment with yellow green light between the lines D and E gave a distance 42° 33½′, to which the light could be traced. Measures of the first six maxima or bright bars were taken, and though probably correct, yet on returning to the first maximum for verification some derangement had occurred, so that no argument can be taken from them to have weight.

The stream was formed in passing through a small circular aperture of $\frac{1}{25}$th of an inch diameter, in a brass nozle on a pipe furnished with a stopcock, which acted as a siphon from an elevated pneumatic trough, so that the velocity of the stream could be regulated by the stopcock.

On October 2, I tried the effect of different velocities on the position of the bars, and found with green light, when the interference bars were most distinct, the extreme visible light extended to a reading on the circle of 269° 9′, and when the velocity was greater, it extended to a reading of 267° 35′; and from the nature of the reading, the light extended to 1° 34′ further beyond the caustic in the latter than in the former case. In another trial with green light and the bars most distinct, the extreme visible light gave a reading of 269° 4′; but with a greater velocity and the bars still visible, the reading was 267° 16′, or the difference was 1° 48′. When the full stream was allowed to run, the extreme light extended still further, but the bars were not seen.

These experiments, coupled with the measures of the natural rainbow, lead us to conclude, that if the cylinder of water had been at rest, the extreme visible light might not have extended even up to the caustic.

In resuming the experiments lately, I have had as a reservoir for the water, a common glass bottle with the bottom broken off, as in the figure: a glass tube drawn with the blowpipe to a fine tapering aperture passed through the cork in its neck. When the water in the reservoir was at some height, as *a b*, and the stream *c d* was formed, it was received in a catch-tube, as in the figure, and led away. The part of *c d* which appeared cylindrical and without ripples, either from the lower end falling on the catch-tube, or from its breaking into drops, was adjusted so as to be over the centre of the horizontal protractor, and level with the axis of the telescope, carried on the arm of the vernier, before mentioned. The stream of water, where used, was very nearly $\frac{1}{30}$th of an inch diameter, and its distance from the narrow slit of $\frac{1}{100}$dth of an inch breadth between metallic edges, through which the sun's light was reflected by a mirror of a solar microscope fixed in the shutter of a dark room, was 22 feet.



Preliminary measures on the 5th of April last gave the distance from the perpendicular ray, to which the extreme red extended for the primary iris, with the height of the surface of the water in the reservoir 4½ inches above the aperture, equal to 43° 26′; and with a height of water 7½ inches, equal to 43° 30′.

When the side of the tube had been oiled and the water had ceased running out, a drop was obtained hanging from the end of the tube, when readings for the extreme red were taken, which gave in one experiment the distance from the normal ray 42° 1′, and in another 41° 51½′. Calculating with the refractive index of the fixed ray B, the radius of the primary bow, if at the caustic, is 42° 22′. So that with the *vertical stream* the light is visible outside the caustic, and with the *stationary drop* it does not extend to it.

With the adjustments more perfect, I obtained on the 7th of April as follows:—

				°	′
With 4¼ inches water pressure the angle was				43	3½
With average 7¼ inches		...	...	43	17
...	$8\frac{7}{16}$	...	...	43	24½
...	$8\frac{13}{16}$	...	...	43	20½

I conclude that with this fine aperture the difference of the pressures produces a slight but sensible effect upon the distance to which the light extends beyond the caustic.

The side of the tube being oiled, with a little care in adding small quantities of water after the stream had ceased to run, I obtained on the 10th of April a good drop at the extremity of the tube, which remained perfect to the end of the experiments. The drop obtained in this manner is larger than one would wish, but the telescope being carefully adjusted to distant objects, or for parallel rays, and the observations being always taken at the centre of the field of view, there is no correction of the observations required on that account. The iris being steady, was much better seen than in the running stream, and the maxima or bright bars were counted to upwards of forty when a piece of red glass was held before the eye.

The drop being $\frac{1}{10}$th of an inch in diameter, and the other circumstances as in the last experiments, the following measures were obtained for the limit beyond which no light could be discerned, from the perpendicular ray:—

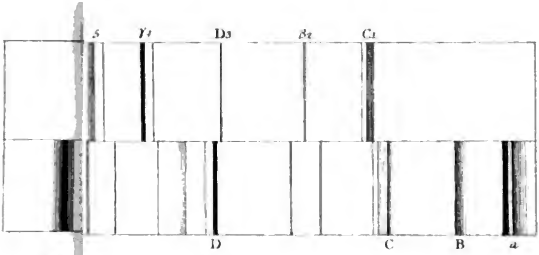
$$
\begin{array}{ll}
42^\circ & 22' \\
42 & 23\tfrac{1}{2} \\
42 & 24\tfrac{1}{2} \\
42 & 21
\end{array}
$$

A good set of measures of the places of the first six maxima were also obtained, whose distances, counted from the caustic, approximated in progression to those of Newton's reflected rings, counting from a black centre as ordinarily considered, namely they approximated to the progression of the values of 1, $\sqrt{3}$, $\sqrt{5}$, $\sqrt{7}$, &c.

As the extreme limit of the visible red was taken, it is clear that since the asymptote of the caustic or place ordinarily assigned to the primary rainbow, for the fixed line B is 42° 22′ from the perpendicular ray, therefore no light extends beyond this; and that the rays which have traversed equal spaces produce darkness in interfering, contrary to the fundamental requirement of the undulatory theory.

The support which that theory has so long had, from experiments made with running water compared with calculations for still water, must now cease.

London, April 13, 1855.


5
γ1
D3
β2
C1
D
C
B
a

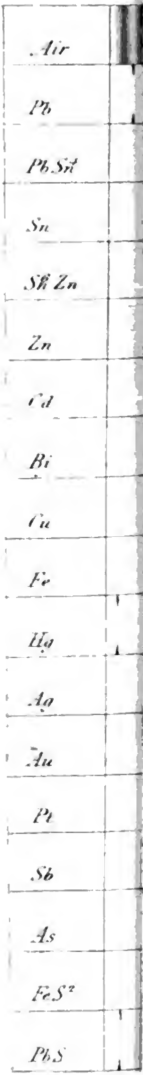
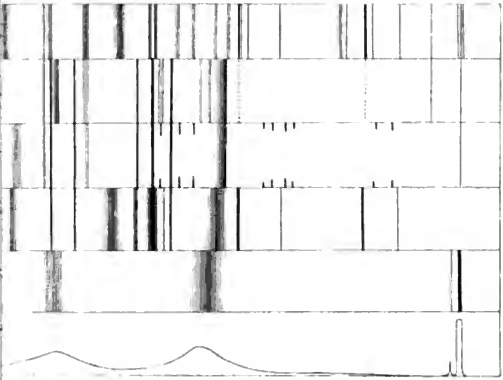
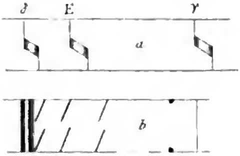

Air
Pb
Pb Sn
Sn
Sb Zn
Zn
Cd
Bi
Cu
Fe
Hg
Ag
Au
Pt
Sb
As
FeS^2
PbS

Fig. 3.



1.

Fig. 5.


δ
E
γ
a
b

XLVIII. *Optical Researches.* *By* A. J. Angström*.

[With a Plate.]

1. THE dispersion, absorption, and diffusion of light, are effects, the complete solution of which, it may be assumed, is still very distant from us. Thus the explanation given by Cauchy, in conformity with the notions of Fresnel, that the sphere of attraction of the molecules is comparable in magnitude with the length of undulation, has not yet been applied to doubly refracting bodies; and so far as Cauchy's theory has reference to isophanous bodies it also needs completion, inasmuch as the action of the medium itself is not taken into account. Further, as regards the phænomena of absorption, there is certainly no lack of explanations, but the difficulty is to apply them in each particular case; and, as several causes work together, to appropriate to each its due proportion in the production of the phænomena to be explained. The grounds more particularly referred to in the explanation of the absorption and diffusion of light, are the following:—

a. An unsymmetrical distribution of the molecules of the medium, the consequence of which is, that in the differential equations for the motion, differential coefficients of an uneven degree are introduced. As far as I know, this cause was first assigned by Cauchy in a letter to Libri.

b. The principle adduced by Euler in his *Theoria Lucis et Caloris,* according to which the colour of a body is produced by the resonance of the oscillations, which can be assumed by the particles themselves.

c. The interference of light. On this principle Newton is known to have based his celebrated theory of colours, which was further expanded by Biot in the spirit of the theory of emission. M. von Wrede was the first to apply it in the sense of the theory of undulation, by showing that the phænomena, in the case of absorption by gases, &c., might be referred with ease to the principle of interference.

Now with regard to the first of the three grounds of explanation, it alone is by no means sufficient to explain the phænomena of colours. That in general the opacity of bodies might be referred to it as a cause is easy to discern; but it does not explain why a certain colour, to the exclusion of all others, is produced.

The principle of Euler also explains, not so much the colour *which a body actually exhibits,* as that *which it is unable to assume,* because most of the oscillatory motions which bodies assume in consequence of absorption, make no impression on our organs of sight, but generally fall in the domain of feeling.

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* From Poggendorff's *Annalen*, vol. xciv. p. 141.

If therefore the colours of bodies are to be explained by resonance, this property must also be ascribed to the æther, and thus we arrive with ease at the third of the principles referred to, namely to that of interference.

2. It is necessary to distinguish strictly the absorption of light from its diffusion. Both, indeed, produce the common result of weakening the passing rays; but they differ from each other essentially in the circumstance, that the diffused light possesses the same properties as the incident light; the absorbed portion, on the contrary, exhibits itself as heat, or as a chemical agent; in other words, in the former case the time of oscillation is unchanged, but in the latter this is not the case. This is an essential distinction, which has not always been borne in mind in optical investigations, although Melloni proved it long ago in the case of heat. That the diffused light possesses the same properties as the incident light is most evident from the fact that the lines of Fraunhofer, in the solar spectrum, are also exhibited by the light of the planets, and by all bodies illuminated by the sun. This is not disproved by the newest investigations of Stokes, in which he seeks to show, that, in the case of absorbed light, the time of vibration is altered, because the facts there brought forward belong to the latter class.

Diffused light must therefore, according to the foregoing, be produced by modifications of the undulatory motion of the æther itself, while *absorbed* light is transferred to the molecules of the body. These are thereby moved from their positions of equilibrium and thrown into vibrations, which vary with the peculiar characteristics of the body. It is, however, to be well borne in mind, that a medium absorbs, not only those vibrations which it most readily assumes, but also those which occupy a simple relation to its own time of vibration, such as the octave, third, &c. As bodies in general are not luminous, it follows that the absorbed light must escape the cognizance of the eye.

That this, however, is not always the case, is proved by Brewster's *remarkable discovery* in the case of the alcoholic solution of the green colouring matter of leaves. When a beam of light is transmitted through such a medium, the liquid in the path of the beam is coloured blood-red by unpolarized light. This phænomenon, to which Brewster has applied the name of *inner dispersion,* furnishes a manifest proof *that the absorbed light does not always pass to oscillating series of a lower order.* The so-called epipolarized rays of Herschel belong to the ordinary case, in which the absorbed light reappears as heat, to the extent that here it is chemical rays which are absorbed, and which, according to Stokes, produce the luminous effects discovered by Herschel.

3. Now, as according to the fundamental principle of Euler, a body absorbs all the series of oscillations which it can itself assume, it follows from this that the same body, when heated so as to become luminous, must emit the precise rays which, at its ordinary temperature, is absorbed. The proof of the correctness of this proposition is, however, surrounded with great difficulties; for the condition of the heated body, as regards elasticity, is altogether different from the state in which the light is supposed to be absorbed. An indirect proof of the truth of the proposition is furnished by the connexion, discovered by M. Nièpce de Saint Victor, between the colour imparted by a body to the flame of alcohol, and that developed by light upon a disc of silver which has been chlorinized by the body under consideration. As the disc of silver, treated with chlorine alone, assumes all the tints of the solar spectrum, and, when treated at the same time with a colouring body, exhibits almost exclusively the colour of the latter, this cannot occur otherwise than by the exclusive absorption on the part of the so-prepared silver disc of the precise tint which belongs to the colouring body.

4. One of the most convenient and most practicable means of studying a glowing body is presented by the electric spark, although the body in this case is probably in the gaseous condition. I have therefore believed that an investigation of the spectra obtained from electric sparks drawn from different metals would not be without interest for the theory of light. The subject has been already treated by Wollaston, Fraunhofer, Wheatstone, and lastly by Masson, in a memoir in the *Annales de Chimie et de Physique,* 1851. Masson has also measured the electric spectra of various metals, and has drawn them by means of the *Camera lucida.* Nevertheless I venture to hope that the present investigation will not be deemed superfluous, when the results obtained by me are compared with those obtained by my predecessors.

I have found that the spectrum of the electric spark must really be regarded as consisting of two distinct spectra; one of which belongs to the gas through which the spark passes, and the other to the metal or the body which forms the conductor.

The electric spectrum is traversed,—1. By a multitude of luminous lines, comparable in number and distribution with the lines of Fraunhofer in the solar spectrum. These lines are the same for all metals, but vary in intensity with the strength of the condenser, and the greater or less humidity of the air. The nature of the conductor does not appear to exercise any other influence than that some of its own lines render, by their brightness, some of the others less perceptible.

2. By shining lines peculiar to each metal. These lines are easily distinguished from the foregoing by the fact, that when the condenser is not altogether too strong, and consequently the charge too feeble, they do not form complete transverse lines, but appear to proceed from both edges of the prism, and to be extinguished before they reach the centre. They are no less distinguished from the former lines by their brighter light, and seem, as it were, to stand forth against the dull ground on which the said lines are drawn in undiminished intensity through each point of the transverse section. This predominating brightness, particularly with strong charges, is doubtless the reason why Masson, in all the spectra which he has drawn, has not found more than four lines identical.

To explain the production of the first spectrum, I assume with Faraday and Masson that the electric spark is produced by a current which propagates itself across, and by means of, ponderable matter, which it heats in the same manner, and according to the same laws, as a voltaic current heats a metallic wire. The other spectrum, on the contrary, is produced by the heating and dispersion of the particles of the conductor which proceed simultaneously from both the poles, but on the way lose both their temperature and their luminous properties.

Among the constant lines there are two which were observed by Fraunhofer, and which exceed the others in brightness, namely, a double line in the yellow and one in the green light. In Masson's discovery, and also here in fig. 1, Plate II., they are denoted by γ and δ*. It is possible that these two lines are sometimes common to both spectra, and that their brightness is thus increased; I will not venture to deny this. In the case of bismuth, at least, this is the case with the line γ.

To render the comparison between the solar spectrum and that of the electric spark more evident, I have drawn both of them, side by side, in fig. 1. Both drawings are made from nature, an angle of one minute embracing a millimetre and a half of the drawings. The difference between the spectra is, that to form a correct notion of the upper or electric spectrum, the black lines must be regarded as luminous ones; luminous with the colour which corresponds to their position in the spectrum. For these observations, a flint-glass prism of Merz, with a refracting angle of 46° 34′ 57″, was made use of.

The observations on which the drawing of the electric spectrum is based are contained in the following table. It is to be remarked, that the measurements for the different metals were not determined at once, but that the prism was so fixed for each series that the line D gave the minimum of deflection*.

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* In Masson's memoir, however, the line δ for antimony is incorrectly given; in like manner the line β, which is in general very weak, is in the case of copper confounded with the line which I call D.

[illegible]	Tin.	Lead.	Zinc.	Platinum.	Mean.
C_1	+° 16′ 10″	° ′ ″	° ′ ″	+° 16′ 30″	+° 16′ 20″
β_2	......	......	......	+ 8 40	+ 8 40
γ_4	− 9 11	− 8 36	− 9 3	− 9 3	− 8 58
5	− 13 20	− 13 11	− 13 55	− 13 38	− 13 31
E_6	− 29 35	− 29 26	− 30 5	− 29 33	− 29 40
δ_7	− 39 37	− 39 1	− 39 23	− 38 32	− 39 8
8	......	......	......	− 51 30	− 51 30
9	......	......	......	− 56 20	− 56 20
F_{10}	−1 1 52	−1 1 56	−1 2 12	−1 2 13	−1 2 4
11	−1 19 50	−1 16 11	−1 17 10	−1 17 15	−1 18 16
G_{12}	−1 27 40	−1 27 11	−1 27 40	−1 26 20	−1 27 13
13	−1 38 35	......	−1 36 0	−1 35 20	−1 36 38
H_{14}	−1 58 50	......	−1 58 45	−1 58 50	−1 58 48

In order to calculate the indices of refraction, and from them the wave-lengths which correspond to the principal lines of the spectrum, I have measured the absolute deflections of some of them; two spheres of zinc served as conductors.

	a.	*b.*	
D . .	241° 26′ 5″ 20	309° 9′ 15″	Δ=33° 50′ 45″
γ . .	... 16 55 ... 17 5	309 18 30	Δ=34 0 48
E . .	240 56 15 40	309 39 0	Δ=34 21 22
δ . .	... 46 30 40	309 48 15	Δ=34 30 50
F . .	... 23 50	310 8 55	Δ=34 52 45

The observations for the position *b* of the prism were made *between* the double readings which correspond to the other position *a*.

Two corresponding readings gave for the lines of Fraunhofer, D and *b*, of the solar spectrum,

D . . . . . Δ=33° 51′ 40″
b . . . . . Δ=34 21 57

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[*] A circumstance which interferes with the accuracy of the measurements is due to the fact that the sparks do not always spring from the same point. This may be prevented by coating the balls with a layer of wax. The first spark bursts the coating, and all the succeeding ones follow in its path.

that is to say, values which are nearly equal to those of D and E in the electric spectrum.

A direct experiment nevertheless showed that D in the electric spectrum does not completely coincide with the vivid yellow line formed in the spectrum of the flame of alcohol, but lies somewhat nearer to the red. The yellow line coincides with the line to the left of D which is common to all the metals, and which I have denoted by *n* in fig. 2, Plate II.

5. When the solar spectrum is compared with the electric one, it is found that some of the lines, such as C, D, E, G, and we may also say H, have their corresponding lines in the solar spectrum; but for the strongest, γ and δ, this is not the case.

A complete correspondence, however, between the solar and the electric spectrum is the less to be expected, as the lines in the former, as is generally assumed, are not only due to the action of the atmosphere, but also to be referred to the action of the sun itself. For the present we are not in a position to separate the two systems of lines from each other, although Brewster's numerous researches on the peculiar influence of the atmosphere have furnished interesting results. Thus the atmosphere produces dark lines on the violet side of C, and on the red side of D; in like manner is produced, on the other side of D, a wide band, possibly in the neighbourhood of γ, &c.

Miller states that during an approaching thunder-storm he has seen dark lines between D and C, but nearer to D, which would seem to coincide with the line γ. In Broch's drawing of the solar spectrum, taken as the sun was near rising or setting, a dark zone also occurs near D, although the distance from D appears to be smaller than that between γ and D. Between F and G is a dark zone, which possibly corresponds to the line F in the electric spectrum.

The analogy between the two spectra may, however, be more or less complete when abstraction is made from all the minuter details. Regarded as a whole, they produce the impression that one of them is a reversion of the other. I am therefore convinced that the explanation of the dark lines in the solar spectrum embraces that of the luminous lines in the electric spectrum, whether this explanation be based upon the interference of light, or the property of the air to take up only certain series of oscillations. The first view has only one difficulty, and that is to explain how the different retardations, which are necessary for the effect, are produced; this will be the more difficult for the electric spectrum, inasmuch as all these retardations must occur in the inconsiderable mass of air which is in direct contact with the electric current.

6. In fig. 2 I have given a sketch of the principal lines

which exhibit themselves in spectra of different metals. That by the use of stronger charges, particularly with the metals which melt at high temperatures, such as platinum and iron, other lines may appear, is probable; I believe, however, that I have omitted none of the more important ones. The simple inspection of the figure shows, however, a great regularity in the distribution of the lines; also, in the case of some metals, a certain tendency to appear in groups. In this manner they show themselves with lead, particularly in the yellow and blue-violet field; with zinc in the red and blue; with bismuth several distinct groups are formed. In general, with the more fusible metals the lines seem to spread themselves more. With mercury and lead the longest go into violet. It is singular that gold and silver have a common line in green; but besides this, silver has a few common with mercury.

To all the metals the line *n* is common; also the line *m* in red, although in consequence of its feebleness it is difficult to determine its position with accuracy, seems to be common to all; with mercury, however, I did not see it. Coal gave no lines, except those which were to be found in all spectra.

7. Wheatstone has observed that when the poles are formed of different metals, the spectrum contains the lines of both. It was therefore interesting to examine whether a compound of the same metals, particularly a chemical one, would also exhibit the lines of both metals, or distinguish itself by the exhibition of new lines. The first was found to be the case. The only difference observed was, that certain lines were absent, or showed themselves with greater difficulty; but when they appeared, it was always at the same places corresponding to the single metals. It is easy to convince oneself of this when one pole is formed, for example, of $Sn^4 Pb$. From the side of the spectrum which corresponds to the chemical compound, lines proceed which belong to both metals, and the position of these lines may be controled by making use of both metals successively for the opposite pole.

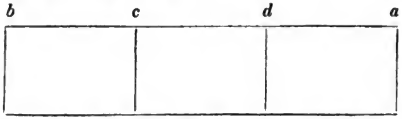
If the metals are combined in other proportions, no difference appears. With $Zn Sn$ the lines in blue were displaced a little towards the violet, but very inconsiderably. This result is so far of interest for the theory, as it shows that the particles of both metals are not united in particular groups, but that each forms distinct centres of oscillation.

8. It does not seem that the lines peculiar to each metal in the electric spectrum can be due to interference. By interference lines of sharp outline cannot be produced except when they lie very near each other, otherwise they spread themselves over a larger portion of the spectrum. Through the union of unequal retardations, groups of fine lines may certainly be formed, but

not well isolated lines. What pronounces particularly against the assumption referred to, is the fact that these luminous lines seem to proceed from the edge of the prism, and to lose themselves in the centre, while the ground on which they exhibit themselves does not appear to be in the slightest degree changed.

In the same manner as we assume that the almost homogeneous light emitted by a flame of alcohol is due to the fact that its oscillations belong almost exclusively to the yellow light, although a trace of luminous lines appears on the rest of the spectrum also, we seem, on the other hand, to be forced to assume that the bright lines belonging to a metal in the electric spectrum, the number of which increases with the strength of the electricity, correspond to the vibrations of the metal in the gaseous condition.

9. In connexion with the foregoing, I will here mention an observation which I have made on the flame of a solution of iodine in alcohol. The alcohol flame alone gives a spectrum shown in the following figure:—



where *a* denotes the vivid yellow line, *b* a weaker one in the extreme blue, and *c* and *d* traces of two other lines. The solution of iodine produces, on both sides of *d*, several bright lines with equal intervals; their distance apart amounts to about 10′. Twice, when the wick was furnished with iodine, I saw dark bands formed between *b* and *c*, which appeared to be at about the same distance from each other. The phænomenon seems to be the same as in the case of the absorption by iodine; the bands were not sharply defined, but resembled real bands of interference.

10. The study of the spectra of the electric spark, it seems to me, can also contribute to the solution of a question equally interesting to both astronomy and optics. Doppler has endeavoured to explain the colour of the double stars by the assumption that the velocity of a body has an influence upon its colour. On the other hand, Petzval, in a discourse before the Academy of Sciences in Vienna, has proved analytically that the motion of a medium can have no influence on the time of oscillation, on which alone the colour depends. Although this proposition is probable in itself, still a practical proof of it is not, I think, without interest.

From Wheatstone's researches on the velocity of electricity, we know, not indeed the actual velocity of the spark, but at least a limit value, under which it cannot fall. He found that a spark, 4 inches in length, passed in a less time than the millionth part of a second. But a stream of light 6 feet long, in a glass tube partially exhausted, showed itself also unchanged in the rotating mirror; and as, according to Masson, the spark cannot otherwise propagate itself *in vacuo* than by the particles from the poles themselves, it would follow that at least 3 feet were passed over in less than the millionth part of a second. This gives a velocity which in all probability far surpasses that of the double stars.

Now, if a spark be permitted to pass in an oblique direction, according to Doppler's theory the colour of the particles which stream out from one of the poles must be different from that of the particles which stream from the other; the more so, as they move in opposite directions, the velocity above mentioned being thus doubled as regards the production of the colour. Such a difference, however, is not observed. The bright lines correspond to each other perfectly as before. Hence, from both practical and theoretical grounds, we are justified in concluding, that time of oscillation and colour are independent of the velocity of the medium from which the light emanates.

11. In the foregoing pages I have shown that the spectrum of the electric spark is really to be regarded as an overlapping of two spectra, one of which belongs to the metal, and the other to the air through which the spark passes. From this it necessarily follows, that when the spark passes through a gas different from atmospheric air, the spectrum must exhibit a corresponding alteration. Partly to ascertain the nature of this alteration, and partly to place the fact of a superposition of an air and metal spectrum beyond all doubt, I have observed the phænomena exhibited by the electric spark in different gases. The gases which, up to the present time, I have had an opportunity of examining are,—oxygen, carbonic acid, nitric oxide, hydrogen, carburetted hydrogen and nitrogen. As the results obtained appear to me to be not without interest, I take the liberty of adding them to the foregoing.

The apparatus made use of in these observations is shown in fig. 4. It consists of a glass tube and two brass caps, the whole forming a closed cylinder, in which the electric spark passes between the two spheres of brass *a* and *a*. One of the brass balls can be raised or depressed by means of a screw. The tube *b* serves for the introduction of the gas, which then issues through a hole in the opposite end of the cylinder. To ensure that the cylinder contained no atmospheric air, the gas was generally permitted to stream for an hour through the vessel before the observations began. I made use of the same flint-glass prism as before, so that the results obtained, strictly speaking, are only valid for those kinds of light for which the flint-glass is diaphanous. This is the more necessary to be mentioned here, as it is easy to forget how much glass in general absorbs the extreme ends of the spectrum. I must also mention that the gases made use of were not perfectly pure, although I do not believe that an erroneous result could be produced by the impurity present; the reader will be able to infer this from the statement of the manner in which the gases were prepared.

12. *Oxygen.*—The gas was obtained from chlorate of potash, and was introduced directly into the apparatus. The spectrum obtained in this instance is shown at O, fig. 3. If this spectrum be compared with that of the atmosphere, it is seen,—1st, that the strongest lines of the air spectrum, which in fig. 1 are denoted by γ, δ, D, E, have disappeared; and 2nd, that the blue and violet field exhibit new lines which do not appear in the other case.

With regard, further, to the metal lines in the case of copper and zinc, they were almost imperceptible at the end of the experiment. This was particularly the case with the zinc in the blue field. From this it seems that we may conclude, that the bright lines corresponding to the metal are not produced by the oxidation of the latter, for if this were the case, they must show themselves strongest in oxygen. Nevertheless the surface of the brass spheres showed that an oxidation had taken place, and it is possible that the cause of the phænomenon alluded to is to be sought here.

13. *Carbonic Acid.*—The gas was prepared in the ordinary way by means of sulphuric acid and chalk, and was carried, for the purpose of drying it, through a bottle filled with pumice-stone which had imbibed sulphuric acid. The spectrum obtained is shown at CO^2 in fig. 3; it resembled perfectly the spectrum of oxygen, as far as the strong lines in the blue and violet are concerned. Some difference was, however, observed in regard to the weaker lines; a bright band, corresponding to 13, fig. 1 of the air spectrum, was also observed, which did not appear in the oxygen spectrum. Nevertheless both spectra might be regarded as identical. This is easily explained by the fact, that, according to Berzelius, the electric spark decomposes the carbonic acid into carbonic oxide and oxygen, and the latter gas produces its own peculiar lines in the spectrum.

As it was possible that the violet and blue lines in the oxygen spectrum, and in that of carbonic acid, might have been produced by the oxidation of the metallic particles during their motion through the gas, and as, if this were the case, they must disappear if an unoxidizable metal were made use of, I had the two spheres thickly gilt (with mercury gilding) and repeated the experiment with carbonic acid. There was no alteration of these lines, which I regard as belonging to the gas. The lines of the metal had, however, been changed, but without becoming quite like those observed in former instances with gold. A luminous band, broken in the middle, showed itself in the oxygen spectrum three lines higher, which corresponds to F in the air spectrum, without, however, coinciding with one of the zinc lines. Besides this, at 5 and E fine lines were observed.

14. *Binoxide of Nitrogen*, NO^2.—A mixture of sugar and dilute sulphuric acid was heated, and the developed gas was conducted, first through water, and then through the drying bottle before mentioned. The spectrum of the gas showed a union of the air and oxygen spectra, although by no means complete. As, according to Berzelius, the electric spark decomposes the nitric oxide into nitrogen and nitrous acid, or more probably hyponitrous acid, the two lines γ and δ must belong to the nitrogen. It is more difficult to explain the production of the oxygen lines, if we do not assume that the nitrous acid is still further decomposed.

15. *Hydrogen*.—This gas was produced from zinc and sulphuric acid, being dried in the ordinary manner. The spectrum is shown at H_a and H_b in fig. 3. H_b represents, in an approximate manner, the luminous intensity of the different parts of the spectrum. Remarkable, in the case of hydrogen, are the strongly luminous and wide lines at the red end of the spectrum which, moreover, besides a feeble line in the vicinity of the former, contains only two bright portions, one at the limit of blue and green, and the other in the extreme blue.

In general the hydrogen spectrum bears a great resemblance to that of the flame of alcohol, only the red lines must be conceived to be moved nearer to the yellow light. The facility with which hydrogen transmits electricity is remarkable. When the spheres were almost 2 inches asunder, the sparks passed with the greatest ease.

16. *Carburetted Hydrogen*, CH.—This gas was developed from alcohol and sulphuric acid, then conducted through a solution of potash, and afterwards through the drying apparatus. The spectrum was nearly the same as that obtained with hydrogen, and for this reason I have not drawn it. It was however brighter, particularly in the green, and also showed to the left of E, fig. 1, a bright line, which I did not find in the hydrogen spectrum. As the electric spark decomposes carburetted hydrogen into its elements, this result is not to be wondered at, particularly as coal exhibits no lines peculiar to itself in the spectrum.

17. *Nitrogen.*—It follows from what has gone before, that the bright lines in the air spectrum belong almost exclusively to the nitrogen. To submit this conclusion to a direct test, I enclosed a stick of phosphorus in the apparatus and ignited it by means of a heated copper wire, introduced through the small opening which permitted the issue of the gas. The aperture was then closed. The nitrogen thus obtained is not pure, but is mixed with a white smoke of phosphoric acid; this, however, falls and leaves the nitrogen alone behind. *The electric spark showed the same appearances as in atmospheric air.* This result not only corroborates the conclusion already referred to, but it also shows that the presence of the phosphoric acid does not produce any change, and that the air spectrum cannot be regarded as the result of the combustion of oxygen and nitrogen.

18. Another question follows:—Is the glowing of the gaseous particles produced by the propagation of the electricity, or is it a purely mechanical consequence of the motion of the metallic particles? The latter appears to be principally the case. When, for instance, the sparks pass between two points in the same horizontal plane, the metallic particles will nevertheless be scattered vertically, and the lines γ, E, δ appear as in *a*, fig. 5. Hence we see that the glowing of the air particles is strongest in the direction in which the metallic particles are scattered, and that both directions commingle, as it were, in the centre. It is remarkable, also, that the metallic particles show no tendency whatever to combine with each other. Thus, for example, on one occasion the zinc lines appeared on the blue, as at *b*, fig. 5, without any sensible change being produced in the air spectrum. The latter observation seems, however, to be opposed to the foregoing, and hence to leave the question in respect to glowing unanswered. A further contribution to its solution is furnished, I believe, in what follows.

In experiments with the sulphurets, for example with sulphuret of silver and sulphuret of antimony, when the same charge was used, two kinds of sparks showed themselves alternately, one stronger than ordinary, the other much weaker and of a dull red appearance, but so that at the surface of the metal itself it possessed its ordinary character. Regarded through the prism, the spark produced at the edge of the spectrum some bright points, but beyond this the field was dark. I placed the prism at a distance of only 4 feet from the spark, but in this case also the field was so dark that it was not possible to determine directly the position of the bright points. Only by the circumstance that now and then stronger sparks showed themselves, was such a determination possible, and it was thus found that the bright points corresponded exactly to the lines peculiar to the metal, which here showed themselves upon the surface of the metal only. It seems, therefore, that in this case the air alone transmitted the electricity, but did not impart to the ordinary spectrum a sufficient strength of light, because the metallic particles did not by their motion cause the particles of air to glow. It would be well, however, to make this the subject of further investigation. To the same portion of the subject belongs probably a fact which I have several times observed, namely, that when one of the poles is formed of a fluid, of sulphuric acid for example, the lines in the corresponding half of the air spectrum are much weaker than in the other.

19. The experiments which I made with different metallic sulphurets led to no remarkable result. In general they gave the composition of the metal more or less completely. There was no difference observed, whether the sulphuret received from the apparatus positive or negative electricity. As examples, I have given in fig. 2 the spectra for the sulphuret of lead and sulphuret of iron. The latter, however, is distinguished by the fact, that in the vicinity of the line γ, and also in violet, bright lines are shown which were not observed in my experiments with iron. These lines could not, however, belong to the sulphur, because they must then exhibit themselves with other metallic sulphurets.

Sulphur, like carbon, seems to have no peculiar lines. I have made the experiment, not only with charcoal, but also with graphite, and have obtained no lines which could be regarded of the same character as those exhibited by the metals. The lines of the air spectrum, indeed, particularly in the violet, appeared stronger than ordinary; and in several of the bright portions fine lines were plainly visible (*e. g.* at H, fig. 1), but I have had no reason to conjecture that these lines belong to the carbon, particularly as platinum also shows the violet end of the spectrum in a peculiarly manifest manner. The deportment must be referred to the fact, that the coal particles, by their finer division, set more easily the air particles in a glow.

20. Summing up the observations on the spectra of gases, it follows that, in the oxygen spectrum, the greatest number of bright lines occur in the blue and violet; in the nitrogen spectrum, in green and yellow; and in the hydrogen spectrum, in red. These phænomena must necessarily be connected with the chemical and thermic properties of the gases. Regarded from the theory of undulation, the chemical activity must exert itself chiefly in the motion of the molecules; and it may be assumed with some probability, that two bodies, whose vibrations are isochronous, may combine more easily than two others. In this way the possibility of a combination is finally referred to the

possibility of exciting in two bodies coincident molecular vibrations, where under ordinary circumstances such do not exist.

The chemical activity belongs, nevertheless, to a certain class of rays. Becquerel states, "*que chaque substance sensible voie le rayonnement à sa manière,*" and this is capable of very extensive application. Thus hydrogen, in contact with spongy platinum, is thrown into a condition in which it can enter into combination with oxygen; and the blue colour of sunlight excites in chlorine the power of entering into union with hydrogen. These blue rays act also most powerfully on the solid constituents of plants, while the yellow rays have most to do in producing the green colour, and so on. Hence there are no rays in the solar spectrum which do not possess a chemical power, although the violet end of the spectrum, on account of the part acted by oxygen in most chemical changes, shows itself here most influential. Further, the combustion of most bodies and their combination with oxygen produces a blue flame; zinc has a great affinity for oxygen, and nitrogen a feeble one; and all this seems to speak in favour of the above coincidence in the motions of vibration, which I have stated to be a condition of chemical combination, and proves at the same time that the oscillations of oxygen belong more particularly to the blue and violet portions of the spectrum.

That isomeric bodies possess different chemical properties seems also natural, as a change of position of the molecules must of necessity be accompanied by other vibrations.

21. It is more difficult to explain how, in some cases, a moderate heating calls chemical action into play; for the electric relations of the medium cannot thereby have suffered changes so considerable as to produce new series of oscillation; or, in other words, how it is that one and the same kind of oscillation, simply by increasing its intensity, is capable of producing quite different effects. This phænomenon seems to be explicable in the following manner:

Let ξ, η, ζ be the coordinates of a molecule with reference to its position of equilibrium, and let them be supposed to be so small, that, in the differential equations for the motion, their higher powers may be neglected. For a certain kind of motion $n=\frac{2\pi}{t}$, we may then set

$$\begin{aligned}\xi&=a\,.\cos(nt+\lambda)\\ \eta&=b\,.\cos(nt+\lambda)\\ \zeta&=c\,.\cos(nt+\lambda).\end{aligned}$$

Let the amplitudes be now supposed to increase, so that finally ξ^2, η^2, ζ^2 become sensible; in this case I have found that the equations of motion will be satisfied by the following assumption:—

$$\xi=\epsilon\ +a'\cos(nt+\lambda)+\alpha\cos 2(nt+\lambda)$$
$$\eta=\epsilon'\ +b'\cos(nt+\lambda)+\beta\cos 2(nt+\lambda)$$
$$\zeta=\epsilon''+c'\cos(nt+\lambda)+\gamma\cos 2(nt+\lambda),$$

so that a simple augmentation of the amplitudes can excite vibrations of a higher order, which in their turn can produce chemical effects.

22. A question of great interest, to the solution of which, however, great difficulties oppose themselves, is the following:—Why is it that the spectrum obtained from the glowing metallic particles of the electric spark exhibits only certain colours, and does not, like the glowing metal itself, show all colours in a continuous series? A glowing platinum wire gives a spectrum without shining lines; the electric spark from platinum gives such lines in numbers. Not without some hesitation as to whether I have hit upon the true cause of this difference, do I venture upon the following explanation.

The general theory of small motions teaches us that the number of the molecular motions of a body amounts to thrice the number of molecules within the sphere of attraction; and from this it follows, that when the number of the molecules is infinite the number of their motions must also be infinite, and hence the spectrum must exhibit itself as a continuous whole. This, however, is not necessarily the case with the metallic particles which are scattered by the electricity. These particles may, in magnitude, remain considerably under the sphere of attraction of the molecules; by this the number of the possible oscillations becomes limited, and hence also the spectrum cannot form a connected whole.

23. I have had only recently an opportunity of becoming fully acquainted with the interesting memoir of Stokes "On the Change of Refrangibility of Light." I see with satisfaction that Stokes's explanation of the remarkable phænomena of dispersion in the green colours of plants, in sulphate of quinine, and in an infusion of horse-chestnut bark, namely, that the medium, when illuminated by the sun, becomes itself luminous, is exactly the same as that which I have given in the foregoing pages of the same phænomenon.

24. The law at which Stokes has arrived is, that in the case of inner dispersion, the refrangibility of the incident light is changed, being generally diminished; or, what is the same, that the time of oscillation of the dispersed light is, in comparison with the incident light, augmented. This law is by no means opposed to dynamical principles. The unchangeability of the

time of oscillation is only true of wave motion; and as long as the luminous ray exists in this form, the time of oscillation is unchanged. When, however, it is absorbed by a medium, the particles of which are thereby set in vibration, the wave motion passes into a pendulum motion, and the law of unchangeability ceases to be true. The molecular motions, however, communicate themselves to the æther, and a new wave motion is thus generated, which, as regards the time of oscillation, is different from that of the incident light, but the same as that of the pendulum motion from which it originates.

As before remarked, we have long possessed in the absorption of calorific rays an example of a similar change. Between the dispersed light and the absorbed heat there is, however, an important difference; the former ceases with the illumination, the latter not. What can be the cause of this difference? It is difficult to answer with certainty. But the reason is probably to be sought in the circumstance, that the substances under examination do not possess any capacity of conduction for colours similar to their conductivity for heat; so that the molecular motions of the medium are immediately transferred to the æther, and become dispersed in the real sense of the term. It is also possible that the molecular motions produced by the action of light are not infinitely small, and hence, in accordance with what has been adduced, a higher order, such as the octave, may be communicated to the æther-vibrations as long as the body is illuminated, but that on the withdrawal of the latter they become insensible. In such a case the medium would, however, have its temperature increased.

Stokes also assumes that the molecular motions of the absorbing medium are not infinitely small, but he draws from this the conclusion, that the periods of motion of the medium are longer than the æther waves which produce them, while according to my investigation exactly the opposite must be the case; premised, moreover, that the parts of the medium swing isochronous with the æther, and not in conformity with its own elasticity.

XLIX. *On the Oblique Aberration of Lenses.*

By JOHN BRIDGE, *M.A. University College, London*[*].

THE importance of destroying oblique aberration in the construction of photographic lenses has induced me to attempt the problem. This I have done in the manner which seemed most suited to the purpose, which was to ascertain the point where a given ray of a pencil emerging from any point of a flat object meets a flat screen after refraction, both object and screen being perpendicular to the axis of the lens, and the points where they intersect the axis being conjugate foci. The formulæ give immediately the amount of the diffusion, or spread of the rays of any pencil over the surface of the photographic plate, and the effects due to obliquity and aperture respectively are easily deduced. Though they are adapted directly to the case just mentioned, they may without much difficulty be made to suit any other. The following investigation might, in fact, be accurately described as a general view of the theory of spherical mirrors and lenses; this will appear by the deduction of the most important cases usually treated, as well as that which is the immediate object of inquiry. I cannot find that the method of reference to coordinates in a plane perpendicular to the axis at the prime conjugate has been previously adopted, nor that the problem has been solved in its general form*.

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[*] Communicated by the Author.

To find the direction of a ray after refraction at a spherical surface.

Let λ, μ, ν be the cosines of the angles made by the incident ray with three rectangular coordinates having the centre of the sphere for origin; x, y, z the point of incidence of the ray; m the refractive index.

Then $\frac{x}{r}, \frac{y}{r}, \frac{z}{r}$ are the direction-cosines of the normal

$$1-\left(\frac{\lambda x+\mu y+\nu z}{r}\right)^2=\sin^2 \text{ angle of incidence},$$

$\mu z-\nu y$, &c. proportional to the direction-cosines of the plane of incidence.

Let λ', μ', ν' be the direction-cosines of the refracted ray; then the equations for determining λ', μ', ν' are

(1.) $\lambda'(\mu z-\nu y)+\mu'(\nu x-\lambda z)+\nu'(\lambda y-\mu x)=0.$

(2.) $\lambda' x + \mu' y + \nu' z = r\sqrt{1-\frac{1}{m^2}\left(1-\frac{(\lambda x+\mu y+\nu z)^2}{r^2}\right)} = \text{A suppose}.$

(3.) $\lambda'^2 + \mu'^2 + \nu'^2 = 1.$

These equations express,—that the refracted ray is in the plane of the normal and the incident ray—the proportionality of the sines of the angles of incidence and refraction—and that λ', μ', ν' are direction-cosines.

Whence

$$\lambda'(\mu r^2-y.\overline{\lambda x+\mu y+\nu z})=\mu'(\lambda r^2-x.\overline{\lambda x+\mu y+\nu z})-\text{A}(\lambda y-\mu x),$$

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* For previous inquiries into the subject of oblique aberration, see Potter's 'Optics,' and Clairaut, *Mémoires de l'Institut*, 1762.

and

$$\nu'(\mu r^2 - y\,.\,\overline{\lambda x+\mu y+\nu z}) = \mu'(\nu r^2 - z\,.\,\overline{\lambda x+\mu y+\nu z}) + \mathrm{A}(\mu z - \nu y)\,;$$

$$\begin{aligned}\therefore\ \overline{\mu r^2 - y(\lambda x+\mu y+\nu z)}^2 = {} & \mu'^2(r^4 - r^2\,.\,\overline{\lambda x+\mu y+\nu z}^2) \\ & + 2\mu'\,.\,\mathrm{A}(\overline{\lambda x+\mu y+\nu z}^2 - r^2)y \\ & + \mathrm{A}^2(y^2 + \mu^2 r^2 - 2\mu y\,.\,\overline{\lambda x+\mu y+\nu z}),\end{aligned}$$

whence

$$\mu' = \frac{y}{mr^2}\sqrt{m^2r^2 - r^2 + (\lambda x+\mu y+\nu z)^2} \pm \frac{1}{mr^2}(\mu r^2 - y.\overline{\lambda x+\mu y+\nu z}).$$

As the process is thus far symmetrical, similar expressions will hold for λ' and ν'.

The equations above are satisfied by two lines in the plane passing through the normal and the incident ray, one on each side of the normal; the upper sign belongs to the refracted ray.

Expansion of μ' and ν',

$$x = \sqrt{r^2 - y^2 - z^2} = r\left(1 - \frac{1}{2}\frac{y^2+z^2}{r^2}\right) \quad \lambda = \sqrt{1-\mu^2-\nu^2} = 1 - \frac{1}{2}(\mu^2+\nu^2).$$

By substitution we have

$$\begin{aligned}\lambda x + \mu y + \nu z &= r + \mu y + \nu z - \frac{1}{2}\cdot\frac{y^2+z^2}{r} - \frac{r}{2}(\mu^2+\nu^2) \\ &= r - \frac{r}{2}\left(\frac{y^2+z^2}{r^2} - 2\,\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right)\end{aligned}$$

$$r^2 - (\lambda x+\mu y+\nu z)^2 = r^2\left(\frac{y^2+z^2}{r^2} - 2\,\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right)$$

$$m^2r^2 - r^2 + (\lambda x+\mu y+\nu z)^2 = r^2\left[m^2 - \left(\frac{y^2+z^2}{r^2} - 2\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right)\right]$$

$$\frac{\sqrt{m^2r^2 - r^2 + (\lambda x+\mu y+\nu z)^2}}{mr^2} = \frac{1}{r}\left[1 - \frac{1}{2m^2}\left(\frac{y^2+z^2}{r^2} - 2\,\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right)\right]$$

$$\frac{\lambda x+\mu y+\nu z}{mr^2} = \frac{1}{mr}\left[1 - \frac{1}{2}\left(\frac{y^2+z^2}{r^2} - 2\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right)\right];$$

$$\begin{aligned}\therefore\ \mu' = {} & \frac{y}{r}\left[1 - \frac{1}{2m^2}\left(\frac{y^2+z^2}{r^2} - 2\,\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right)\right] + \frac{\mu}{m} - \frac{y}{r} \\ & \left[\frac{1}{m} - \frac{1}{2m}\left(\frac{y^2+z^2}{r^2} - 2\,\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right)\right] \\ = {} & \frac{\mu}{m} + \frac{m-1}{m}\cdot\frac{y}{r} + \frac{m-1}{2m^2}\cdot\frac{y}{r}\left(\frac{y^2+z^2}{r^2} - 2\,\frac{\mu y+\nu z}{r} + \mu^2+\nu^2\right).\end{aligned}$$

As the axis of x is no longer to be symmetrical with the other two, it will be more convenient that μ, ν, μ', ν' should represent the tangents of the angles made with the axis of x by the projections on the planes of xy, xz. We must therefore write for μ, $\mu\left(1-\frac{1}{2}\overline{\mu^2+\nu^2}\right)$, &c.; this gives

$$\mu'=\frac{\mu}{m}+\frac{m-1}{m}\cdot\frac{y}{r}+\frac{m-1}{2m^2}\cdot\frac{y}{r}\left(\frac{y^2+z^2}{r^2}-2\frac{\mu y+\nu z}{r}+\mu^2+\nu^2\right)$$

$$+\frac{1}{2}\cdot\frac{\mu}{m}\left[\frac{\mu^2+\nu^2}{m^2}+2\frac{\mu y+\nu z}{r}\cdot\frac{m-1}{m^2}+\frac{y^2+z^2}{r^2}\cdot\frac{\overline{m-1}|^2}{m^2}\right]$$

$$-\frac{1}{2}\frac{\mu}{u}(\mu^2+\nu^2)$$

$$+\frac{1}{2}\cdot\frac{m-1}{m}\cdot\frac{y}{r}\left[\frac{\mu^2+\nu^2}{m^2}+2\frac{\mu y+\nu z}{r}\cdot\frac{m-1}{m^2}+\frac{y^2+z^2}{r^2}\cdot\frac{\overline{m-1}|^2}{m^2}\right]$$

$$=\frac{\mu}{m}+\frac{m-1}{m}\cdot\frac{y}{r}$$

$$+\frac{m-1}{2m^3}\cdot\mu\left[\overline{m-1}\,.\frac{y^2+z^2}{r^2}+2\frac{\mu y+\nu z}{r}-(m+1)\,.\,\overline{\mu^2+\nu^2}\right]$$

$$+\frac{m-1}{2m^3}\cdot\frac{y}{r}\left[\overline{m^2-m+1}\,.\frac{y^2+z^2}{r^2}-2\frac{\mu y+\nu z}{r}+\overline{m+1}(\mu^2+\nu^2)\right].$$

Next, *to find the direction after refraction at the second surface of the lens* whose central thickness is t.

Let s be the radius of this surface, the curvature being supposed in the same direction as before; y_2z_2 the point of incidence on it; $\mu_1\nu_1$ the direction-tangents of the emergent ray.

The refractive index is here $\frac{1}{m}$; then by substitution in the above, we have

$$\mu_1=\mu' m-(m-1)\,.\frac{y_2}{s}$$

$$-\mu'\,.\frac{(m-1)m^2}{2}\left[-\frac{m-1}{m}\cdot\frac{{y_2}^2+{z_2}^2}{s^2}+2\,\frac{\mu' y_2+\nu' z_2}{s}-\frac{m+1}{m}\cdot(\mu'^2+\nu^2)\right]$$

$$-\frac{y_2}{s}\,\frac{(m-1)m^2}{2}\left[\frac{m^2-m+1}{m^2}\cdot\frac{{y_2}^2+{z_2}^2}{s^2}-2\frac{\mu' y_2+\nu' z_2}{s}+\frac{m+1}{m}\,.\,(\mu'^2+\nu^2)\right].$$

Taking now the centre of the second surface as origin,

$$ {x_2}^2+{y_2}^2+{z_2}^2=s^2, \qquad \therefore\ x_2=s-\frac{1}{2}\,\frac{y^2+z^2}{s} \text{ nearly;}$$

$$(x+r-s-t)^2+y^2+z^2=r^2, \quad \therefore\ x+r-s-t=r-\frac{1}{2}\cdot\frac{y^2+z^2}{r};$$

whence $$x-x_2=t-\frac{1}{2}(y^2+z^2)\left(\frac{1}{r}-\frac{1}{s}\right),$$

and

$$y_2=y-\mu'(x-x_2)=y-t\left(\frac{\mu}{m}+\frac{m-1}{m}\cdot\frac{y}{r}\right)$$
$$+\left(\frac{\mu}{m}+\frac{m-1}{m}\cdot\frac{y}{r}\right)\frac{y^2+z^2}{2}\cdot\left(\frac{1}{r}-\frac{1}{s}\right).$$

Substituting for y_2 and z_2,

$$\mu_{\prime}=\mu' m-(m-1)\frac{y}{s}+\frac{m-1}{m}\cdot\frac{t}{s}\left(\mu+\overline{m-1}\,.\frac{y}{r}\right)$$
$$+\mu'\,.\frac{m.(m-1)}{2}\left[(m-1)\frac{y^2+z^2}{s^2}-2m.\frac{\mu' y+\nu' z}{s}+(m+1)(\mu'^2+\nu'^2)\right]$$
$$-\frac{y}{s}\cdot\frac{m-1}{2}\left[(m^2-m+1)\frac{y^2+z^2}{s^2}-2m^2.\frac{\mu' y+\nu' z}{s}+m(m+1)(\mu'^2+\nu^2)\right]$$
$$-\mu\,.\frac{m-1}{2m}\left(\frac{1}{r}-\frac{1}{s}\right)\frac{y^2+z^2}{s}$$
$$-\frac{y}{r}\cdot\frac{(m-1)^2}{2m}\cdot\left(\frac{1}{r}-\frac{1}{s}\right)\cdot\frac{y^2+z^2}{s}.$$

By substituting for μ' and ν', and collecting, this becomes

$$\mu_{\prime}=\mu+(m-1)\,y\left(\frac{1}{r}-\frac{1}{s}\right)+\frac{m-1}{m}\cdot\frac{t}{s}\left(\mu+\overline{m-1}\,.\frac{y}{r}\right)$$
$$+\mu.\frac{m-1}{2m}\left[(y^2+z^2)\left(\frac{m\,.\,\overline{m-1}}{r^2}-\frac{2m^2-2m+1}{rs}+\frac{m^2-m+1}{s^2}\right)\right.$$
$$\left.+2(\mu y+\nu z)m\left(\frac{1}{r}-\frac{1}{s}\right)\right]$$
$$+y.\frac{m-1}{2m}\left[(y^2+z^2)\left(\frac{m^3-2m^2+m+1}{r^3}-\frac{3m^3-5m^2+2m}{r^2 s}\right.\right.$$
$$\left.+\frac{3m^3-4m^2+2m-1}{rs^2}-\frac{m^3-m^2+m}{s^3}\right)$$
$$+2(\mu y+\nu z)\left(\frac{m^2-m-1}{r^2}-\frac{2m^2-m-1}{rs}+\frac{m^2}{s^2}\right)$$
$$\left.+(\mu^2+\nu^2)\overline{m+1}\,.\left(\frac{1}{r}-\frac{1}{s}\right)\right].$$

For $\frac{1}{r}-\frac{1}{s}$ write $\frac{1}{n}$, then

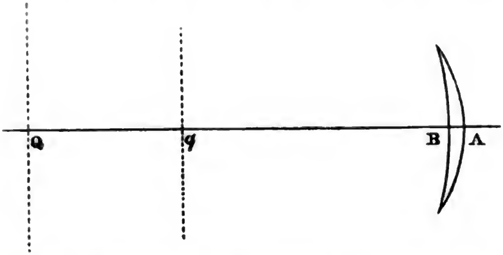
$$\mu_{\prime}=\mu+(m-1)\frac{y}{n}+\frac{m-1}{m}\,.\,t\left(\frac{1}{r}-\frac{1}{n}\right)\left(\mu+\overline{m-1}\,.\frac{y}{r}\right)$$
$$+\mu\,.\frac{m-1}{2mn}\left[\overline{y^2+z^2}.\left(-\frac{1}{r}+\frac{m^2-m+1}{n}\right)+2m.\,\overline{\mu y+\nu z}\right]$$

$$+y\cdot\frac{m-1}{2mn}\Big[\overline{y^2+z^2}\Big(\frac{m+2}{r^2}-\frac{m^2+m+1}{rn}+\frac{m^3-m^2+m}{n^2}\Big)$$

$$+2\,.\,\overline{\mu y+\nu z}\Big(-\frac{m+1}{r}+\frac{m^2}{n}\Big)$$

$$+\quad\overline{\mu^2+\nu^2}(m+1)\Big].$$

Let u be the distance AQ along the axis of the focus of a



direct pencil of rays before incidence; v the distance Bq of the focus of an indefinitely small central pencil of these rays after refraction by the lens. Let planes perpendicular to the axis of the lens be drawn through Q and q, and let b, c be the coordinates in the directions of y, z of a point in the plane of Q towards which a ray is proceeding before incidence; η, ζ the point in the plane of q towards which the same ray proceeds after refraction. Then

$$\mu=\frac{y-b}{u-\dfrac{y^2+z^2}{2r}}=\frac{y-b}{u}+(y-b)\frac{y^2+z^2}{2ru^2}$$

$$\mu_{,}=\frac{y_2-\eta}{v-\dfrac{y^2+z^2}{2s}};$$

$$\therefore\ \frac{y_2-\eta}{v-\dfrac{y^2+z^2}{2s}}=\frac{y-b}{u}+(m-1)\frac{y}{n}+\frac{m-1}{m}.t\Big(\frac{1}{r}-\frac{1}{n}\Big)\Big(\frac{y-b}{u}+\overline{m-1}\,.\frac{y}{r}\Big)$$

$$+(y-b)\,\frac{y^2+z^2}{2ru^2}$$

$$-\frac{b}{u}\cdot\frac{m-1}{2mn}\Big[\mathrm{P}\,.\,(y^2+z^2)+\frac{\mathrm{Q}}{u}\,(y^2+z^2)-\frac{\mathrm{Q}}{u}\,(by+cz)\Big]$$

$$+y\,.\,\frac{m-1}{2mn}\Big[(y^2+z^2)\Big(\mathrm{P}'+\frac{\mathrm{Q}'}{u}+\frac{\mathrm{R}'}{u^2}\Big)-(by+cz)\Big(\frac{\mathrm{Q}'}{u}+\frac{2\mathrm{R}'}{u^2}\Big)$$

$$+\frac{b^2+c^2}{u^2}\,.\,\mathrm{R}'\Big]$$

$$+y\,.\,\frac{m-1}{2mn}\cdot\Big[(y^2+z^2)\Big(\frac{\mathrm{P}}{u}+\frac{\mathrm{Q}}{u^2}\Big)-\frac{by+cz}{u}\cdot\frac{\mathrm{Q}}{u}\Big]^{*},$$

P, Q, &c. being written for the coefficients of (y^2+z^2), $(\mu y+\nu z)$ &c. in μ;

$$\therefore\ \frac{y_2-\eta}{v}=\frac{y-b}{u}+\ldots+\frac{b}{2suv}(y^2+z^2)-\frac{y}{2suv}(y^2+z^2)-(m-1)\frac{y}{n}\cdot\frac{y^2+}{2s}$$

$$\frac{y-\eta}{v}=\frac{y-b}{u}+\ldots+\ldots+\frac{t}{mv}\Big(\frac{y-b}{u}+\overline{m-1}\,.\frac{y}{r}\Big)$$

$$-\frac{y^2+z^2}{2mnv}\Big(\frac{y-b}{u}+\overline{m-1}\,.\,\frac{y}{r}\Big)$$

$$=(m-1)\frac{y}{n}+\frac{y}{u}-\frac{b}{u}+\frac{t}{m}\cdot\Big(\overline{m-1}\,.\frac{y}{r}+\frac{y}{u}-\frac{b}{u}\Big)\Big(\frac{m-1}{r}+\frac{1}{u}\Big)$$

$$+y\,.\frac{m-1}{2mn}\Big[\overline{y^2+z^2}\,.\Big(\frac{m+2}{r^2}-\frac{4m+4}{ru}+\frac{3m^2+m}{un}-\frac{2m^2+m}{rn}$$

$$+\frac{3m+2}{u^2}+\frac{m^3}{n^2}\Big)$$

$$+2\,.\,\frac{by+cz}{u}\Big(\frac{m+1}{r}-\frac{m^2}{n}-\frac{2m+1}{u}\Big)+\frac{b^2+c^2}{u^2}(m+1)\Big]$$

$$+\frac{b}{u}\cdot\frac{m-1}{2mn}\Big[\overline{y^2+z^2}\,.\Big(\frac{m+1}{r}-\frac{m^2}{n}-\frac{2m+1}{u}\Big)+2m\,.\,\frac{by+cz}{u}\Big].$$

Since $\frac{1}{v}=\frac{m-1}{n}+\frac{1}{u}+\frac{t}{m}\Big(\frac{m-1}{r}+\frac{1}{u}\Big)^2$, this gives

$$\frac{\eta}{v}=\frac{b}{u}+\frac{b}{u}\cdot\frac{t}{m}\Big(\frac{m-1}{r}+\frac{1}{u}\Big)$$

$$-y\,.\,\frac{m-1}{2mn}\Big[\mathrm{A}\,.\,\overline{y^2+z^2}+2\mathrm{B}\frac{by+cz}{u}+(m+1)\frac{b^2+c^2}{u^2}\Big]$$

$$-\frac{b}{u}\cdot\frac{m-1}{2mn}\Big[\mathrm{B}\,.\,\overline{y^2+z^2}+2m\,.\,\frac{by+cz}{u}\Big],$$

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[*] Whence, by putting $b=0$, $c=0$, and diminishing y and z without limit we have $\frac{1}{v}=\frac{m-1}{n}+\frac{1}{u}+\frac{t}{m}\Big(\frac{m-1}{r}+\frac{1}{u}\Big)^2$ for the focus of a small centra direct pencil.

where

$$A=\frac{m+2}{r^2}-\frac{4m+4}{ru}+\frac{3m^2+m}{un}-\frac{2m^2+m}{rn}+\frac{3m+2}{u^2}+\frac{m^3}{n^2}$$

$$B=\frac{m+1}{r}-\frac{m^2}{n}-\frac{2m+1}{u}.$$

The expressions in [ ] are symmetrical with respect to y and z, so that the expression for $\frac{\zeta}{v}$ is found by writing c and z for b and y outside these brackets.

Putting $c=0$, that is, supposing the ray, when incident, to be proceeding to a point of the plane of Q where it is cut by the plane of xz, we have

$$\begin{aligned}\frac{\eta}{v}=\frac{b}{u}+\frac{b}{u}\cdot\frac{t}{m}\left(\frac{m-1}{r}+\frac{1}{u}\right)\\ -y\cdot\frac{m-1}{2mn}\left[A(y^2+z^2)+2B\frac{by}{u}+\frac{b^2}{u^2}(m+1)\right]\\ -\frac{b}{u}\cdot\frac{m-1}{2mn}\left[B(y^2+z^2)+2m\frac{by}{u}\right]\end{aligned}$$

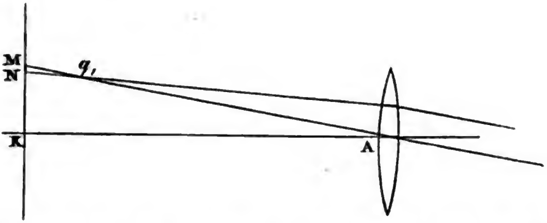
$$\frac{\zeta}{v}=-z\cdot\frac{m-1}{2mn}\left[A(y^2+z^2)+2B\frac{by}{u}+(m+1)\frac{b^2}{u^2}\right].$$

It is obvious that when $b=0$, $c=0$, the above gives the expression for the aberration in the same form as that in Encyc. Britt., art. *Telescope**. I have found that form more convenient for the calculation of a large number of cases than those in Coddington.

It is satisfactory to find that these expressions very easily yield those for particular cases as found in Coddington and elsewhere, as follows:—

Primary and secondary foci of a small centrical oblique pencil.

For the primary focus, that is, for rays incident in the axis of y (c being supposed $=0$),



MK the plane perpendicular to the axis at the focus for a small, direct, centrical pencil. Then

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* Clairaut is, I find, the author of this form.

$$\frac{\mathrm{MN}}{\mathrm{M}q_1}=\frac{y}{\mathrm{AM}} \text{ nearly,}$$

or

$$\mathrm{M}q_1=x_1=-\frac{\mathrm{AM}}{y}\,.\,\Delta\eta=-\frac{v}{y}\Delta\eta=+v^2\,.\,\frac{b^2}{u^2}(3m+1)\,\frac{m-1}{2mn}$$

$$x_1=\frac{v^2}{2f}\cdot\frac{b^2}{u^2}\left(3+\frac{1}{m}\right).$$

For the secondary focus,

$$x_2=-\frac{\mathrm{AM}}{z}\Delta\zeta=-\frac{v}{z}\Delta\zeta=v^2\frac{b^2}{u^2}(m+1)\,\frac{m-1}{2mn}$$

$$=\frac{v^2}{2f}\cdot\frac{b^2}{u^2}\cdot\left(1+\frac{1}{m}\right).$$

(See Coddington, p. 119.)

Primary and secondary foci of a small excentrical pencil which crosses the axis ($z=0$).

For the primary,

$$\frac{\Delta\eta}{x_1}=\frac{\Delta y}{v} \qquad x_1=-v\frac{d\eta}{dy} \text{ nearly,}$$

$$=+v^2\,.\,\frac{m-1}{2mn}\left(3\,.\,\overline{\mathrm{A}y^2+2\mathrm{B}\frac{by}{u}+m\frac{b^2}{u^2}}+\frac{b^2}{u^2}\right)$$

$$=\frac{v^2}{2f}\cdot\left(3\,.\,\frac{\mathrm{A}y^2+2\mathrm{B}\frac{by}{u}+m\frac{b^2}{u^2}}{m\,\frac{b^2}{u^2}}+\frac{1}{m}\right)\frac{b^2}{u^2}$$

$$=\frac{v^2}{2f}\cdot\frac{b^2}{u^2}\left(3\mathrm{V}+\frac{1}{m}\right);$$

since it is found that

$$\frac{\mathrm{A}y^2+2\mathrm{B}\frac{by}{u}+m\frac{b^2}{u^2}}{m\,\frac{b^2}{u^2}}$$

is equal to what in Coddington is called V*. (See Coddington, pp. 137 and 138, or the *résumé* in pp. 196, &c.)

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* This is a much more convenient form than Coddington's V, since it enables us to distinguish immediately the effects due to aperture and obliquity.

In a similar manner for the secondary focus,

$$x_2 = \frac{v^2}{2f} \cdot \frac{b^2}{u^2}\left(\mathrm{V} + \frac{1}{m}\right).$$

Also, by substituting for μ and μ' in the expression for refraction at one spherical surface, their values

$$\frac{y-b}{u} + \frac{y-b}{u} \cdot \frac{y^2+z^2}{2ru}$$

and

$$\frac{y-\eta}{v} + \frac{y-\eta}{v} \cdot \frac{y^2+z^2}{2rv} = \frac{y-\eta}{v} + \frac{2y}{r} \cdot \frac{y^2+z^2}{2rv} - \frac{y-b}{u} \cdot \frac{y^2+z^2}{2rv} \text{ nearly,}$$

and -1 for m, we find

$$-\frac{\eta}{v} = \frac{b}{u} + \left[y\left(\frac{1}{r} - \frac{1}{u}\right) + \frac{b}{u}\right]\frac{1}{r}\left(\overline{y^2+z^2}\left(\frac{1}{r} - \frac{1}{u}\right) + 2\,\frac{by+cz}{u}\right)$$

for the point of intersection of a reflected ray with a plane perpendicular to the axis through the focus of rays proceeding to the projection on the axis of the focus of incident rays.

By limiting the expressions to the case considered in Potter's 'Optics,' part 2. p. 110, &c., they will be found to give easily the same results as those in pp. 117, 118, 119 of the same work.

To find the plane perpendicular to the axis of the lens on which the aberration of a given ray of an oblique pencil is a minimum, that is, the distance between the points of intersection by it and by the central ray.

The equations of the ray, taking the origin at distance v from the lens, are (putting μ for μ_1, μ_0 for the central ray)

$$\mathrm{Y} = \mu \mathrm{X} + \eta$$
$$\mathrm{Z} = \nu \mathrm{X} + \zeta;$$

and for the central ray,

$$\mathrm{Y}_0 = \mu_0 \mathrm{X} + \eta_0$$
$$\mathrm{Z}_0 = \nu_0 \mathrm{X} + \zeta_0;$$

the distance of the points of intersection of these rays with a plane perpendicular to the axis at distance X from the origin is

$$\mathrm{D} = \sqrt{(\mathrm{Y}-\mathrm{Y}_0)^2 + (\mathrm{Z}-\mathrm{Z}_0)^2}$$

and

$$\mathrm{D}^2 = [(\mu-\mu_0)^2 + (\nu-\nu_0)^2]\mathrm{X}^2 + 2(\overline{\mu-\mu_0} \cdot \overline{\eta-\eta_0} + \overline{\nu-\nu_0} \cdot \overline{\zeta-\zeta_0})\mathrm{X}$$
$$+ \left((\eta-\eta_0)^2 + (\zeta-\zeta_0)^2\right).$$

This is a minimum when

$$\mathrm{X} = -\frac{(\mu-\mu_0)(\eta-\eta_0) + (\nu-\nu_0)(\zeta-\zeta_0)}{(\mu-\mu_0)^2 + (\nu-\nu_0)^2};$$

and then

$$D^2=\frac{\left((\mu-\mu_0)(\zeta-\zeta_0)-(\nu-\nu_0)(\eta-\eta_0)\right)^2}{(\mu-\mu_0)^2+(\nu-\nu_0)^2}.$$

Now

$$\eta-\eta_0=-\left(My+N\frac{b}{u}\right)v$$

$$\mu-\mu_0=.\frac{y}{v}\text{ nearly,}$$

where M and N are symmetrical about the axis,

$$X=+\frac{M(y^2+z^2)+N\frac{by+cz}{u}}{y^2+z^2}.v^2=v^2\left(M+N.\frac{by+cz}{u(y^2+z^2)}\right)$$

$$D=\frac{v}{u}.N\frac{bz-cy}{\sqrt{y^2+z^2}}$$

$$Y-Y_0=-v.N.\frac{bz-cy}{u(y^2+z^2)}.z \quad Z-Z_0=+v.N\frac{bz-cy}{u(y^2+z^2)}.y$$

$$\frac{Z-Z_0}{Y-Y_0}=-\frac{y}{z}.$$

The line joining the two points in the intersection is therefore perpendicular to the line joining the two points of incidence.

From these formulæ may be deduced the expressions for the primary and secondary foci of an oblique centrical pencil.

If the focus of incident rays be supposed to be a point in the plane of zx, $b=0$.

Then for a ray incident in the axis of z, $y=0$, and it is clear that $D=0$, or it meets the central ray.

$X_1=\frac{v^2}{2mf}(3m+1)\frac{c^2}{u^2}$, if $\frac{z}{r}$ be supposed very small as compared with $\frac{c}{u}$.

For a ray incident in the axis of y, $z=0$, $N=0$; $\therefore$ $D=0$, and $X_2=\frac{v^2}{2mf}(m+1)\frac{c^2}{u^2}$.

If for brevity we write the expressions for $\frac{\eta}{v}$ in the form $\frac{\eta}{v}=\frac{b}{u}+T+U$, we have for the coordinate η_2 of the point where a ray after refraction at two lenses in contact meets the plane perpendicular to their common axis through the focus for central direct rays,

$$\frac{\eta_2}{v_2}=\frac{\eta_1}{v_1}+T_2+U_2=\frac{b}{u}+T_1+T_2+U_1+U_2.$$

In the small terms y_2, z_2, $\frac{\eta_1}{v_1}$, $\frac{\zeta_1}{v_1}$ may be considered as equal to y_1, z_1, $\frac{b}{u}$, $\frac{c}{u}$.

The conditions that all rays proceeding from *any* one point in a plane perpendicular to the axis may meet in one point on the screen placed to receive the image, are

$$\mathrm{U}_1+\mathrm{U}_2+\ldots=0 \text{ (and a similar one for } \zeta)$$

for all values of y, z, b, c, involving among other conditions, that

$$\frac{m_1-1}{n_1}+\frac{m_2-1}{n_2}+\ldots=0, \quad \text{or } \frac{1}{f_1}+\frac{1}{f_2}+\ldots=0,$$

f_1, f_2, &c. being the principal focal lengths of the lenses, the thicknesses being neglected. This equation is impossible so long as the lenses remain in contact.

For two lenses not in contact, but whose interior surfaces are separated by an interval d, the aberration is found as follows.

Let $v_1-d=u_2$, and let y_2, z_2 be the point of incidence on the second lens; $\mathrm{C}_2=m_2+1$, $2m=2(\mathrm{C}_2-1)$. Then

$$y_2=\frac{u_2}{v_1}y+d.\frac{b}{u};$$

and by substitution we obtain

$$\begin{aligned}\frac{\eta_2}{v_2}=\frac{\eta_1}{u_2}+\frac{v_1}{u_2}\cdot\frac{b}{u}\cdot\frac{t_2}{m_2}\left(\frac{m_2-1}{r_2}+\frac{1}{u_2}\right)&\\ -y\cdot\frac{m_2-1}{2m_2n_2}\bigg((y^2+z^2)\mathrm{A}_2\frac{u_2^{\,3}}{v_1^{\,3}}+2\frac{by+cz}{u}\left(\mathrm{A}_2\frac{u_2^{\,2}}{v_1^{\,2}}d+\mathrm{B}_2\frac{u_2}{v_1}\right)&\\ +\frac{b^2+c^2}{u^2}\left(\mathrm{A}_2\frac{u_2}{v_1}d^2+2\mathrm{B}_2d+\mathrm{C}_2\frac{v_1}{u_2}\right)\bigg)&\\ -\frac{b}{u}\cdot\frac{m_2-1}{2m_2n_2}\bigg((y^2+z^2)\left(\mathrm{A}_2d\frac{u_2^{\,2}}{v_1^{\,2}}+\mathrm{B}_2\frac{u_2}{v_1}\right)+2\frac{by+cz}{u}&\\ \left(\mathrm{A}_2\frac{u_2}{v_1}.d^2+2\mathrm{B}_2d+\mathrm{C}_2\frac{v_1}{u_2}-\frac{v_1}{u_2}\right)&\\ +\frac{b^2+c^2}{u^2}\left(\mathrm{A}_2d^3+3\mathrm{B}_2\frac{v_1}{u_2}d^2+3\mathrm{C}_2\frac{v_1^{\,2}}{u_2^{\,2}}\cdot d-2\frac{v_1^{\,2}}{u_2^{\,2}}\cdot d\right)\bigg).&\end{aligned}$$

It may sometimes be convenient to use the polar forms of some of the expressions, as follows.

If $z=\rho\sin\alpha$, $y=\rho\cos\alpha$, $b=\sigma\cos\beta$, $c=\sigma\sin\beta$, $\frac{\mathrm{Z}-\mathrm{Z}_0}{\mathrm{Y}-\mathrm{Y}_0}=\tan\theta$, then

$$X=v^2\left(M+N\frac{\sigma\cos(\alpha-\beta)}{\rho u}\right)$$

$$D=N\frac{v}{u}\sigma\sin(\alpha-\beta)$$

$$\tan\theta=-\frac{1}{\tan\alpha},$$

and

$$M=\frac{1}{2mf}\left(A\rho^2+2B\frac{\rho\sigma}{u}\cos(\alpha-\beta)+(m+1)\frac{\sigma^2}{u^2}\right)$$

$$N=\frac{1}{2mf}\left(B\rho^2+2m\frac{\rho\sigma}{u}\cos(\alpha-\beta)\right).$$

The practical object will of course be to find those forms of lenses which by their combination reduce $\Delta\eta$ and $\Delta\zeta$ to nothing, if possible, for all values of y, z, b, c. This involves the solution of five equations, upon the discussion of which I do not propose to enter at present. It may be desirable at some future time to extend the solution so as to introduce the 4th powers of b and c, but not of y and z. I would suggest also the want of some fixed principle of selection among imperfect foci. I have found the results of assuming a law which answers the obvious requirements of the case, as follows:—If that be called the best section (focus) of a pencil which gives the sum of the squares of the distances from their centre of intensity of the points where the several rays meet the plane of the section, we find that the circle of least confusion conforms accurately to this condition, and that for a large direct pencil the distance of the best focus from the least circle of aberration is $\frac{1}{9}$th of the whole longitudinal aberration. I think some satisfactory conclusion on this point might be attained by careful experiment.

L. *Examinations of some Felspathic Rocks.*

By T. S. Hunt, *of the Geological Commission of Canada**.

THE oldest known group of rocks in North America is that to which the name of the Laurentian System has been given in the Reports of the Geological Survey of Canada, and i made up of very much disturbed and highly crystalline strata which are in great part gneissoid and quartzose. Interstratifie with these, however, are beds of crystalline limestone, often o great thickness, and holding scapolite, pyroxene, phlogopite, serpentine, fluor, apatite, spinel, corundum, condrodite, sphene graphite, and other characteristic minerals. Associated with these limestones, there is another rock forming an important member of the group, and consisting chiefly of felspar, with small portions of black mica, green pyroxene, and occasional grains of garnet, epidote, and more rarely of quartz. This rock is often marked by the presence of small portions of hypersthene; from the presence of this mineral, these masses have been described by the New York geologists as *hypersthene rock*. In addition to these minerals we may add ilmenite, or titaniferous iron ore, which occurs sometimes in large masses, and at other times in small disseminated grains, which like the hypersthene appear to mark the planes of stratification. If to these we add small portions of iron pyrites and a little disseminated carbonate of lime, we have the mineralogy of the rock so far as yet known.

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* Communicated by the Author.

The texture of these felspar rocks is varied; sometimes the mass is a confusedly crystalline aggregate, exhibiting cleavage surfaces three or four lines in diameter, with a fine-grained, somewhat calcareous paste in the interstices. Sometimes the whole rock is uniformly granular, while more frequently a granular base holds at intervals cleavable masses of felspar, often several inches in diameter. The colours of these rocks vary from grayish and bluish-white to lavender and violet-blue; flesh-red, greenish and brownish tints are also met with; the colours are rarely brilliant. These felspars seldom occur in distinct crystals, but their cleavage is triclinic, a fact which, coupled with their densities varying from 2·66 to 2·73, shows them to belong to the group of which albite and anorthite may be taken as the representatives. The bluish cleavable varieties often exhibit the opalescence of labradorite, to which species American mineralogists have hitherto referred them; but with the exception of a few analyses by myself, we have no published analyses of any of these felspars. My investigations show, that while all of them are felspars with a base of lime and soda, the composition varies very much, being sometimes that of labradorite, andesine or intermediate varieties, and at other times approaching to that of anorthite. The results of these examinations, as far as yet completed, I propose to give in the present paper, as the first part of the history of these felspathic rocks.

One of the most interesting localities of these felspars is in the parish of Château Richer, in the county of Montmorenci near Quebec, where they cover a breadth of two or three miles across the strike, bounded by crystalline limestone on one side, and a quartzo-felspathic rock on the other, and rising into small hills. In this region several varieties of the rock appear, but the most interesting is made up of a finely granular base, greenish or grayish-white in colour, holding masses of a reddish cleavable felspar, which are sometimes from one-tenth to one-half an inch in diameter, but often take the form of large imperfect crystals, frequently 12 inches long, and 4 or 5 inches wide. These dimensions correspond to the faces M and T, while the face P, characterized by its perfect cleavage, is from half an inch to 2 inches broad. Twin crystals sometimes occur having a composition parallel to M.

Hypersthene is met with throughout the rock in flattened masses, which although variable and irregular in their distribution, exhibit a general parallelism; they are occasionally 4 or 5 inches in length and breadth, by an inch or more in thickness, and are separated from the granular felspathic rock by a thin film of brownish-black mica. Titaniferous iron ore is also found in grains and lenticular masses, occasionally an inch or two in thickness; these occur in the granular base, and generally near the hypersthene, but grains of the mineral are occasionally found in the crystalline felspar. Quartz in small grains is imbedded in the titaniferous iron, but was not observed elsewhere in the rock, nor have any other minerals than these been detected. In the specimens of the rock which I selected on the spot for examination, the crystalline felspar constitutes from one-half to seven-eighths, while the hypersthene does not equal more than $\frac{2}{100}$dths, and the titaniferous iron more than $\frac{1}{100}$dth of the whole; the amounts of quartz and mica are insignificant. In other portions of the rock, however, the proportion of the ore may equal 5 per cent., and in some parts the amount of hypersthene is nearly as great. By the action of the elements, the surface of the rock becomes of a dull opake white; the cleavable masses of felspar are, however, less affected than the granular portion, and by their obscure reddish colour are distinctly visible on the weathered surfaces; this change extends but a very little distance into the rock. The iron ore of course remains unaltered, but the dark brown hypersthene becomes lighter, and inclines to a pinchbeck-brown.

The felspar is triclinic in cleavage; the angle of P : M = about 80° 80′. Cleavage with P perfect, with the other planes distinct; P is often delicately striated, and sometimes curved. Hardness =6, and density 2·667 to 2·674. Lustre vitreous, sometimes pearly on P; colour flesh-red, passing into reddish, greenish and grayish-brown; the surfaces sometimes mottled, but the red always predominating.

The following analyses were made of three different specimens, which were carefully selected, and after being pulverized, dried at 212° F. The earthy ingredients were determined after fusion with carbonate of soda, and the alkalies by the method of Dr. J. L. Smith, which consists in igniting for some time the finely levigated mineral with five or six parts of carbonate of lime and three-fourths its weight of sal-ammoniac. The agglutinated mass slakes by the action of water, and yields to that liquid its alkalies in the form of chlorides, with a mixture of chloride of calcium. A second ignition of the undissoved residue with two-thirds the first amount of sal-ammoniac ensures the separation of the last portions of alkali. These processes were adopted in all the analyses here given, with some exceptions to be noticed in their places.

	I.	II.	III.
Silica	59·55	59·85	59·80
Alumina	25·62	25·55	25·39
Peroxide of iron .	·75	·65	·60
Lime	7·73	6·94	7·78
Magnesia	traces	·11	·11
Potash	·96	·96	1·00
Soda.	5·09	5·09	5·14
Loss by ignition .	·45	·30	·00
	100·15	99·45	99·82

In another specimen the amount of lime was found to equal 7·89 per cent. The composition of this felspar is very nearly that of andesine, which, according to Abich, yields silica, 59·60; alumina, 24·18; peroxide of iron, 1·58; lime, 5·77; magnesia, 1·08; potash, 1·08; soda, 6·53=99·82.

The greenish base of the rock is generally finely granular and strongly coherent; the grains possess the cleavage, lustre, and hardness of felspar; the density of carefully chosen fragments was from 2·665 to 2·668. The greenish-white of the powder is changed to a fawn colour by ignition. When pulverized and digested with acetic acid, the mineral loses two or three thousandths of carbonate of lime, with traces of magnesia, iron oxide, and alumina. A portion which had been thus treated and carefully dried gave the following results:—

	IV.
Silica	58·50
Alumina	25·80
Peroxide of iron .	1·00
Lime	8·06
Magnesia	·20
Potash	1·16
Soda	5·45
Loss by ignition .	·40
	100·57

It is therefore a felspar differing but little from the crystalline andesine in composition.

The hypersthene occurs in foliated masses with curved surfaces. Besides the basal cleavages thus exhibited, it cleaves readily with the sides of an oblique prism of 87°, and with its longer diagonal. The hardness of the mineral is 6, and its density 3·409 to 3·417. Lustre vitreous, submetallic; colour blackish-brown, in thin laminæ yellowish-brown; streak and powder ash-gray, the powder turning reddish-gray on ignition. Subtranslucent, brittle, fracture uneven. The fragments which had served to determine the density still contained flakes of felspathic matter between the laminæ; these were, as far as possible, removed in breaking up the hypersthene for analysis. The results of two analyses by fusion with carbonate of soda were as follows:—

	V.	VI.
Silica	51·85	51·35
Alumina	3·90	3·70
Protoxide of iron	20·20	20·56
Lime	1·60	1·68
Magnesia	21·91	22·59
Manganese	traces	
Loss on ignition	·20	·10
	99·66	99·98

It is almost identical in composition with the hypersthene from Labrador, analysed by Damour.

The accompanying ilmenite was more or less interpenetrated with felspar and quartz, which could not easily be separated. Its hardness was 6, and the density of selected fragments from 4·65 to 4·68. Colour and streak iron-black, lustre submetallic; not magnetic. When decomposed by fusion with bisulphate of potash, it gave,—

	VII.
Titanic acid	39·86
Peroxide of iron	56·64
Magnesia	1·44
Insoluble quartz, &c.	4·90
	102·84

A large portion of the iron is to be regarded as protoxide.

Another variety of the lime-felspar rock from Château-Richer is pale greenish or bluish-gray, with occasional reddish grains, and is finely granular. The lustre is vitreous upon the cleavages, but waxy elsewhere. The only foreign mineral observed in the rock was brownish-black mica in small scattered patches. The density of the greenish-gray portion was 2·681, and its analysis gave,—

	VIII.
Silica	55·80
Alumina	26·90
Peroxide of iron	1·53
Lime	9·01
Magnesia	·27
Potash	·86
Soda	4·77
Loss by ignition	·45
	99·59

In Château-Richer and its vicinity there are found boulders of a well-defined variety of the felspar rock, which has not yet been met with in place. The base is a coarsely granular felspar of a light reddish-gray colour and vitreous lustre, exhibiting everywhere distinct cleavages, and holding imbedded small bright grains of ilmenite, surrounded with thin films of brownish mica. The imbedded crystals of felspar are numerous, and are often 3 or 4 inches in length and breadth by an inch in thickness. The faces of perfect cleavage are beautifully striated, and the smaller crystals, which are often slender and well defined, are sometimes curved. Hardness, 6; density, 2·680 to 2·692; lustre, vitreous; colour, pale lavender blue, with pearly opalescence. Semitransparent, fracture conchoidal. Analysis IX. is a cleavable fragment from a boulder found at Château-Richer, and X. and XI. are from a similar and larger mass in the neighbouring parish of St. Joachim.

	IX.	X.	XI.
Silica	57·20	57·55	57·35
Alumina	26·40	27·10	27·30
Peroxide of iron	·40		
Lime	8·34	8·73	
Potash	·84	·79	
Soda	5·83	5·38	
Loss by ignition	·65	·20	·25
	99·66	99·75	

The district of Montreal also affords extensive exposures of these same felspar rocks, associated with crystalline limestone, in the counties of Leinster and Teneboune. In the townships of Rawdon and Chertsey they are often fine-grained and homogeneous, and constitute an exceedingly tough rock, with an uneven subconchoidal fracture, and a feeble vitreous lustre; this variety is bluish or grayish-white in colour, somewhat translucent, and exhibits here and there the cleavage of grains of felspar. Great bodies of this rock are almost free from foreign minerals, while other portions abound in a green granular pyroxene, arranged in thin interrupted parallel layers with ilmenite. These layers of pyroxene are seldom more than 4 or 5 lines in thickness, and may be an inch or two apart, while those of ilmenite are still thinner, and often enclosed in the pyroxene, along the surfaces of which deep red grains of garnet are occasionally seen. These different minerals appear in relief upon the white weathered surface of the rock, and give a picture of its stratified structure, which is, however, not less apparent upon the surfaces of recent fracture. Small, rounded, bluish masses of cleavable felspar are frequently disseminated in the same planes as the other minerals. In some places the pyroxene appears to pass into, or is replaced by, foliated hypersthene. A homogeneous fragment of this rock from Rawdon had a density of 2·691. It was bluish-white, granular and translucent, and gave by analysis,—

	XII.
Silica	54·45
Alumina	28·05
Peroxide of iron	·45
Lime	9·68
Potash	1·06
Soda	6·25
Loss by ignition	·55
	100·49

In connexion with this variety, which has nearly the composition assigned to labradorite, I may give the analyses of two felspars which differ from it principally in their greater amount of lime. No. XIII., from the township of Morin, belongs to the same area as the last, and forms large cleavable masses in a fine-grained base. Its density is 2·684 to 2·695, and its colour greenish-gray, passing into bluish-gray, with an occasional pearl-gray opalescence. The greenish paste of this felspar is softer than the others, and yields with effervescence about four hundredths of carbonate of lime to dilute acids; the insoluble residue has nearly the composition of the felspar. No. XIV. is a cleavable, lavender-blue, opalescent variety, from a rolled mass found in Drummond, Canada West, and having a density of 2·697. The determination of the alkalies in this was not made by the same process as in the other analyses, and probably does not give the full amount.

	XIII.	XIV.
Silica	54·20	54·70
Alumina	29·10	29·80
Peroxide of iron	1·10	·36
Lime	11·25	11·42
Magnesia	·15	traces
Potash } by diff.	3·80	·23
Soda } by diff.	...	2·44
Loss by ignition	·40	·40
	100·00	99·35

At La-Chute on the Rivière du Nord, there is a felspar rock associated like the others with crystalline limestone, and holding in a greenish granular base a cleavable felspar resembling andesine in composition. Its lustre is vitreous, and the face of perfect cleavage, as in all these felspars, is finely striated. Density, 2·687; colour, lavender-blue, passing into sapphire-blue; semitransparent. Its analysis gave—

	XV.
Silica	58·15
Alumina	26·09
Peroxide of iron	·50
Lime	7·78
Magnesia	·16
Potash	1·21
Soda	5·55
Loss by ignition	·45
	99·89

The bytownite of Thompson appears to be one of these granular felspar rocks, and can scarcely be distinguished from some of the varieties just described. In 1850 I examined an authentic specimen of the mineral, and found it to have a hardness of 6·5, and a density of 2·732; it gave by analysis,—

	XVI.	
Silica	47·40	47·30
Alumina	30·45	
Peroxide of iron	·89	
Lime	14·24	
Magnesia	·87	
Potash	·38	
Soda	2·82	
Loss by ignition	2·00	
	99·05	

I remarked at the time, the undoubted felspathic character of the mineral, which I described as corresponding to the thiorsanite of Genth, and as probably anorthite with an admixture of quartz*.

The frequent association of ilmenite with these felspars, derives additional interest from the fact, that the immense deposits of this ore at Bay St. Paul are accompanied with a lime felspar. Here, besides many smaller masses, a body of titaniferous iron ore, 300 feet long by 90 feet wide, is exposed on the side of a hill, and a still larger mass is said to occur in the vicinity. The ore is coarsely crystalline; its colour and streak are iron-black, and its lustre submetallic; it affects the magnetic needle very feebly. Hardness, 6; density, 4·56 to 4·66. Its analysis gave me titanic acid, 48·60; protoxide of iron, 37·06; peroxide of iron, 10·42; magnesia, 3·60=99·68. Disseminated through portions of the ore, are small, garnet-red, translucent grains, which have an adamantine lustre, a conchoidal fracture, and a hardness of 6. They are found by analysis to be pure oxide of titanium, and are to be referred to the species rutile or brookite.

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* See this Magazine, S. 4. vol. i. p. 324. Also Report of the Geological Survey of Canada for 1850–51, p. 38, where analysis XIV. has also appeared.

We have in the rocks which have been the subject of these examinations, a series of felspars in which the amount of silica varies from 47·40 to 59·80 per cent., and that of the lime from 7·73 to 14·24 per cent., the amount of the alkalies decreasing as that of the lime augments. These results only help to confirm the conclusion which may be drawn from all the previous analyses of triclinic felspars, that there are no defined limits for those species which, like vosgite, labradorite, andesine, and oligoclase, have been created between albite on the one hand, and anorthite on the other. I therefore proposed some time since to regard all of the intermediate felspars as mixtures of these two species, which, being homœomorphous, may be supposed to crystallize together in indefinite proportions. Multiplying and expanding the received formulæ of albite and anorthite, I represented them as follows (silica being SiO, and alumina alO$=(\text{Al}^2\,\text{O}^3)\div3$)* :—

	Formula	Eq. wt.	Density.	Eq. vol.
Albite . .	$(\text{Si}^{48}\,\text{al}^{12}\,\text{Na}^4)\text{O}^{64}=$	1054·4 ÷	2·62 =	402·4
Anorthite .	$(\text{Si}^{32}\,\text{al}^{24}\,\text{Ca}^8)\text{O}^{64}=$	1118·4 ÷	2·72 =	405·0.

The composition and density of the intermediate felspars permit us to regard them, for the most part, as mixtures of a soda-albite and a lime-anorthite. In the analyses of many albites and anorthites, however, we have evidence of similar admixtures; for some albites contain from 1 to 2·5 per cent. of lime, and anorthites from 3 to 4 per cent. of alkalies. Of a like significance is the constant presence of a small amount of potash with the soda of these felspars, and the magnesia, sometimes amounting to 5 per cent. in anorthite, leading us to infer the existence of lime and potash-albites (orthoclase?), and soda and magnesia anorthites. The difficulties presented by the varying composition of these felspars are obviated by admitting such a mixture of species as constantly takes place in the crystallization of homœomorphous salts from mixed solutions, and this consideration should never be lost sight of in the study of mineral chemistry.

It was not until after I had published this view of the constitution of the triclinic felspars (a view which must also be extended to the scapolites), that I became aware that Th. Scheerer had recently proposed to regard all felspars as combinations of anorthite with labradorite, anorthite with albite (orthoclase), or labradorite with albite*. It was gratifying to find that this distinguished chemist had already arrived at a solution of the problem of the felspars not unlike my own, but I must object to admitting labradorite as a distinct species, or as having any higher value than oligoclase, andesine, or the felspars X. and XV. in the present paper.

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* American Journal of Science, 2nd series, vol. xviii. p. 270.

Montreal, Canada,
March 1855.

LI. *Note on a doubtful point in Climatology.*
By JOHN BALL, *Esq., M.P.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN common with many persons who have paid attention to glacier phænomena, I have often found it difficult to reconcile the mass of testimony which seems to indicate the continued increase and accumulation of ice and snow in certain regions of the earth, with the equally strong evidence which goes to prove that the general climate of the inhabited portions has not sensibly varied throughout the historic period. Having lately had occasion to discuss the subject with somewhat stricter accuracy, I think that I am enabled to offer an explanation of the difficulty which I am desirous of submitting to the judgement of your readers.

I may commence by observing, that in the course of frequent visits to the higher regions of the European Alps, I had been struck by appearances which indicate that, even at the present time, there are certain spots of limited extent in which this process of accumulation still proceeds, and where, if we only admit that at some period since the glacial epoch the mean temperature of this portion of the earth was slightly greater than it now is, simple physical considerations show that such accumulation must have ensued. This, in fact, merely amounts to the assertion, that there are natural reservoirs or plateaux in the upper regions of the Alps where the annual increase of *névé* or consolidated snow is greater than the annual loss by melting, evaporation, and outflow of glacier towards the lower regions. Several such instances might, I think, be pointed out in the central chain, and elsewhere in the Alps; as, for instance, the plateau, about 13,000 English feet above the sea, which lies between the highest ridge of the Monte Rosa, the Zumstein Spitz, and the eastern end of the Lys Kamm, or the extensive and slightly inclined slopes which lie to the east of the Pass of St. Théodule. It would seem as if the existence of such centres of accumulation would alone serve to explain the fact established by the unanimous current of history and tradition, which affirm the gradual extension and advance of the great glaciers throughout the entire chain of the Alps, simultaneously with a general constancy in the climatal conditions of the surrounding countries. A few unimportant exceptions may be pointed out, such as that of the glacier of Findelen near Zermatt, which has retrograded for the last forty or fifty years, while the Gorner glacier originating in the same ice-fields has advanced to more than a proportionate extent. These cases may probably be explained by landslips altering the direction of the upper portion of certain glacier streams, but they cannot invalidate the universal consent of all available evidence as to the general extension of the glaciers. For this we rely, not merely upon concurrent tradition and documentary evidence, but on the unimpeachable declarations of those ancient tracks and paved footways which we now find invaded by ice and *névé*.

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* Poggendorff's *Annalen*, vol. lxxxix. p. 19, cited in Liebig and Kopp's *Jahresbericht*, 1853, p. 805.

But if I do not mistake, there is evidence of a similar phænomenon on a much larger scale in the polar regions. Some of this evidence afforded by vegetable and animal remains in regions now chiefly covered with snow and ice may be open to question as to its true date, but it appears to me that the remarkable and well-known facts proving a local diminution of temperature in the plains of Iceland and Greenland, extending over the last 700 or 800 years, may far more easily be explained by the continuous accumulation of ice in the upper portions of those and the adjoining countries, than by any other hypothesis with which I am acquainted. It is true that, in regard to Iceland, the change of climate which has gradually driven out the cultivation of grain and reduced the tree vegetation to the dwarf birch scrub which alone now remains, may be in some degree connected with the enormous outburst of lava in the eruption of the Skaptar Yokul, substituting in the interior of the island a vast tract of surface of much greater radiating power; but it is questionable whether, under skies so charged with clouds as those of Iceland are in summer, this explanation can serve much, while the arbitrary assumption of a change in the force and direction of the Gulf-stream is scarcely reconcileable with the absence of any sensible change in the coast climate of north-western Europe. This latter explanation, again, can still less easily be applied to account for the change of temperature in Greenland and in the northern part of Baffin's Bay.

But then comes the question, how it is possible to reconcile this extension of ice and snow in certain regions with the general constancy of the mean temperature of the earth, which may be inferred from historical evidence proving that the limits of cultivation of the best known vegetable products have not perceptibly altered for the last 3000 years, or even a much longer period. If no cosmical causes have interfered to alter the annual supply of heat received by the earth, and if no important change has occurred in the distribution of land and sea, it may be thought impossible that any reduction should occur in the general mean temperature, such as would be implied in the asserted cooling down of some districts, without a corresponding increase of temperature elsewhere. To this I venture to reply, that the increase of alpine glacier and polar ice does imply *an increase of mean temperature in those spots where the accumulation originates.* To return to the Alps from whence I started, I assert that a reservoir wherein the annual deposit of snow exceeds the annual melting and outflow, must have a mean temperature below the freezing-point, that before the rock or soil was constantly covered with snow or ice its mean temperature must have been still lower, and that the result of the accumulation is to raise that mean temperature.

To illustrate this, let us suppose that, instead of the intense dryness that prevails in the frigid regions of northern Siberia, between the Lena and the Jenisei, the prevalence of moist winds should determine a great annual fall of snow, we cannot doubt but that an enormous accumulation of *névé* and ice would proceed for ages until a condition was produced similar to that of the antarctic polar lands, the mean temperature of the region itself would be considerably increased, while that of some neighbouring districts would be diminished, the general mean for the whole earth and for countries not within reach of the immediate operating causes remaining unaltered.

Not having seen this question anywhere satisfactorily discussed, I have offered these somewhat crude considerations to the examination of those interested in its decision, and have the honour to remain, Gentlemen,

Your obedient Servant,

JOHN BALL.

LII. *Further Researches into the Properties of the Sulphate of Iodo-quinine or Herapathite, more especially in regard to its Crystallography, with additional facts concerning its Optical relations.* *By* WILLIAM BIRD HERAPATH, *M.D.*[*]

[With a Plate.]

AFTER referring to the observations of Professors Stokes[†] and Haidinger[‡], as well as to papers already published by himself on this subject in the Philosophical Magazine, the author gives an account of a set of prisms perfectly complementary in their optical characters to those previously described by him, and proves this fact by an elaborate comparison of their various optical relations; from which it appeared, that whilst the α-prisms (those described in the Philosophical Magazine for March 1852) were totally impervious to a beam of polarized light, reflected from glass plates, when the plane of the length of the prism was at right angles to the plane of primitive polarization, the β-prisms (those now examined) were equally absorbent of a vertically polarized beam, when the plane of their length lay parallel to that of primitive polarization. See Plate III. diagram II.

These prisms may be readily made by the following process:—

Dissolve 10 grains of disulphate of quinine in half a fluid ounce of spirit wine, having 3 grains of benzoic acid dissolved in it also; add 2 drachms of water and warm the whole to complete solution, then upon adding a few drops of spirituous solution of iodine and placing in repose, prismatic crystals having the following properties are produced:—

1. When two are crossed in superposition at right angles to each other or even at 30°, the overlapped space is "black" if the crystals are sufficiently thick, but reddish-brown or violet-red if very thin; in these respects they perfectly coincide with the α-prisms. Pl. III. diagram III.

2. When examined by polarized light reflected from glass, the *vertical* crystals now appear black, whilst the horizontal prisms are almost perfectly transparent to the polarized beam. Pl. III. diagram II. β.

3. Upon introducing the selenite stages beneath the prisms, those which previously obstructed the polarized beam and appeared "black," transmitted the "red" or "blue" colours of the selenite films, whilst the transparent, horizontal prisms exhibited the complementary colours, viz. the green or yellow. Pl. III. diagram II. fig. β.

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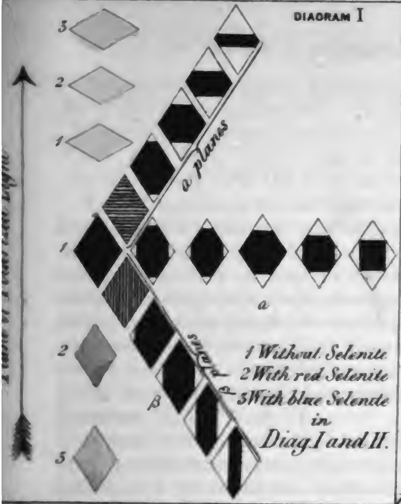
[*] Reprinted from the Abstracts of the Papers communicated to the Royal Society of London.

[†] In a paper read at Belfast before the British Association (Athen. Sept. 1852) by Prof. Stokes.

[‡] In Poggendorff's *Annalen*, June 1853, and Phil. Mag. Oct. 1853, by M. Haidinger.

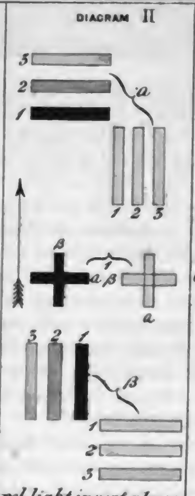
res mag. 200 diam. Arrows shew where polarized Light is employed & its plane also.

DIAGRAM I



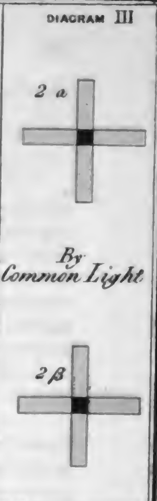
1 Without Selenite
2 With red Selenite
3 With blue Selenite
in
Diag. I and II.

DIAGRAM II

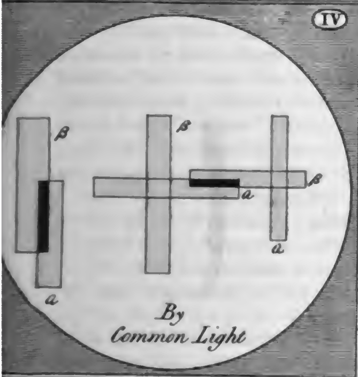


pol. light invert. plane.

DIAGRAM III

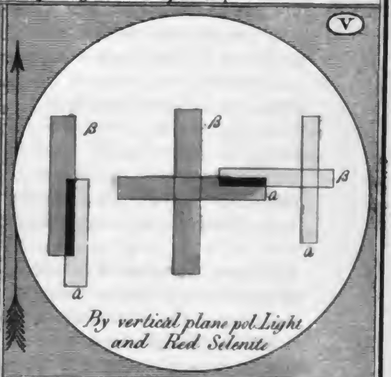


(IV)



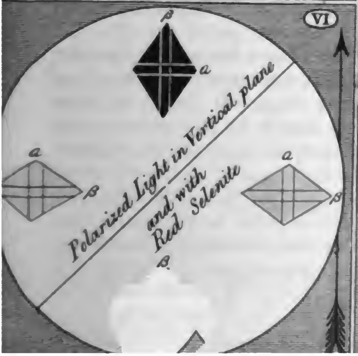
By
Common Light

(V)



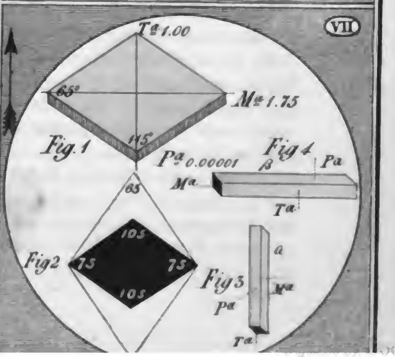
By vertical plane pol. Light
and Red Selenite

(VI)



Polarized Light in Vertical plane
and with
Red Selenite

(VII)



The last two completely decided their complementary character, for whilst the β-prisms were "black," "red" or "blue," when the planes of their length were *vertical*, the α-prisms were "black," "red" or "blue" whilst the planes of their length were *horizontally* placed. Pl. III. diagram II. figs. 1, 2, 3, α & β.

4. But by mixing the two prisms upon one slide their perfectly complementary character was completely demonstrated, for when an α-prism was crossed at right angles by a β-prism, the square space where they overlapped was still transparent to common light; but if a β-prism were overlapped by an α-prism, the length of the two being parallel to each other, the oblong space where they overlapped became black from their doubly absorbent properties; in short, the optical examination fully proved the complementary character of these crystals length for length. Pl. III. diagrams II. IV. V. & VI.

It was further shown that two varieties of hexagons existed; those produced from strong spirituous solutions were longer than broad, and possessed two acute angles of 65° and four equal angles of 147° 30′. Examined by a vertically plane-polarized beam, these were "black" when a line passing through the long diameter or acute angles was parallel to the plane of primitive polarization: these the author called "acute" or β-hexagons. The hexagons produced from acetic acid solutions, on the other hand, were "obtuse" in their character, and had two angles of 115° and four equal angles of 122° 30′. These, when examined by vertically plane-polarized light, were always "black" when a line passing through the two angles of 115° was perpendicular to the plane of primitive polarization: the author called these "obtuse" or α-hexagons. Pl. III. diagram I. α, β.

The rhombic crystals which presented themselves were also "black" (examined in the same way) when their long diameters lay parallel to the plane of primitive polarization, but transparent when they were rotated 90°. Pl. III. diagram I. figs. 1,2,3,&VI.

It was also shown that the doubly absorbent powers of the primary rhombic crystal extended more or less on each side of the vertical position, through an arc of 60°, so that upon rotating a crystal through the whole circle, there were two arcs of 120°, in which the crystals were more or less darkened, and two of 60° each, in which the light passed through without loss; this transparency was of course at its maximum when the longer diameter of the rhombic plate lay in a horizontal position, and gradually became less evident through an arc of 30° above and below this line. By a careful measurement of the angles of the various crystalline forms and by the results of their optical examination, it appeared that they may all be obtained from the right rhombic prism, the acute angle of which is 65° and the obtuse

115°, the major axis being to the minor as 1·57 to 1, whilst the prism itself had scarcely appreciable length. It was a mere rhombic scale; but considered as a short prism, the axes were $P_{\frac{a}{0\cdot00001}}$, $M_{\frac{a}{1\cdot57}}$, $T^{a}_{1\cdot00}$ the quantity for P^a not being absolutely determinable in consequence of its variability and minuteness. Pl. III. diagram VII. fig. 1.

It appeared also that the *α*-prism and obtuse hexagon were the results of truncation of the *acute* angles of the primary rhombic plate or prism by planes at right angles to the plane of primitive polarization, when the crystals were "black," when examined by vertically plane-polarized light; whilst the *β*-prism and acute hexagon resulted from truncation of the *obtuse* angles of the same primary form by planes parallel to that of primitive polarization, the crystals being "black" when examined by a vertically plane-polarized beam; and that the octagonal and square plates, and rectangular parallelogram resulted from the coincidence of these truncation planes in the same individual form. Pl. III. diagrams I. II. & VI.

It was shown that the solvent medium had the power of developing these truncation planes, and it appeared that *water* produced the *α*-truncation, and *spirit* the *β*-truncation; and if the two opposing forces of water and spirit were made equal in intensity, they neutralized each other, so that the pure primary rhombic prism resulted without truncation.

It was further shown that hyponitric æther developed other crystalline forms, converting the rhombic plate into one of 75° and 105° by truncation planes upon the acute angles of the primary rhombic plate, cutting off portions equal to half the long axis, and leaving the shorter or transverse axis untouched. By this means the new rhombic crystal appeared "black" when the longer diameter was at right angles to the plane of vertically polarized light, as if it had been rotated through 90°, whilst it absorbed the polarized light as before. Pl. III. diagram VII. fig. 2.

As from the examination of certain rectangular quadrilateral prisms of the *α* and *β* varieties, it appeared that Herapathite possesses doubly absorbent powers of nearly, if not perfectly equal intensity, in directions coincident with all three rectangular axes P^a, M^a and T^a, the author inferred that the development of their optic axes could not be reasonably expected in these directions. He considered that the biaxial systems of rings would be found to exist in thicker crystals (when discovered), having triangular replacement planes upon the solid angles, either of the shorter or longer oblique diameters of the right rhombic prism, and the results of these optic axes would be seen in a direction perpendicular to these surfaces of replacement. Pl. III. diagram VII. figs. 3 & 4.

The various formulæ necessary to produce different crystalline forms are given in detail.

The author enters into a re-discussion of the double refractive powers, and attempts to show that Herapathite possesses a principal axis having "positive" characters. He also endeavoured to measure the thickness of certain very thin plates, by the differential tints produced by the exercise of their doubly refracting powers upon the colours of the selenite stages, and found that these thin plates raised the blue of the second order to the yellow and violet; the red of the second to the violet and blue-gray; and the green of the same to orange and violet of the second order. Assuming, therefore, this substance to have similar doubly refracting powers to selenite, and of equal intensity, and also the same index of refraction, their thickness may be estimated as equal to that of a plate of selenite necessary to give the same difference in tint, viz. ·000517 of an inch.

These very thin plates no longer possessed the Cantharides-like or brilliant metallic green colour when examined by reflected light. They appeared more like portions of the elytra of the *Blatta germanica*; still reflecting a polarized beam, but of a brown colour, and also retaining their doubly absorbent powers when examined by transmitted light.

The author considers that his recent investigations indicate that whatever properties Professor Stokes may have assigned to the ray reflected from the α-prisms (which alone were experimented on by him) in the principal plane of the breadth, must be equally true in that reflected from the primary rhombic prism in the principal plane of its length, *i. e.* its long diameter; and whatever properties he may have assigned to the reflected ray from the principal plane of the length of the α-prism, will be equally true of that reflected from the primary rhombus in the principal plane of the breadth, *i. e.* its short diameter.

The paper concludes with some recently determined facts relating to the chemical characters of this peculiar substance.

Its sp. gr. at 60° Fahr. was found to be 1·895.

It is very sparingly soluble in æther, turpentine or water at the ordinary temperatures, scarcely more than $\frac{1}{2000}$dth part; boiling water does not dissolve $\frac{1}{1000}$dth part. It does not appear more soluble in æther or turpentine by boiling. Chloroform does not dissolve it, but has a great attraction of surface for it.

Alcohol of ·837 dissolves $\frac{1}{650}$th part at 57° Fahr., and by boiling, it takes up $\frac{1}{30}$th part, which readily crystallizes on cooling in β-hexagons.

Acetic acid, sp. gr. 1·042, dissolves $\frac{1}{750}$th part at 60° Fahr., and $\frac{1}{60}$th part at boiling temperature—iodine volatilizing if the heat is prolonged.

Diluted sulphuric acid, sp. gr. 1·0682, does not dissolve it at the ordinary temperatures, but does so readily with the aid of heat. *Sulphuric acid*, sp. gr. 1·845, rapidly dissolves it.

Diluted hydrochloric acid has but little action on it, but when concentrated, it almost instantly reddens, without dissolving it; upon boiling, it forms a yellow solution, from which minute, dark, opake, radiating aciculæ deposit on cooling.

Nitric acid immediately decomposes it, even in the cold; upon raising the temperature, iodine at first volatilizes; then nitrous acid vapours are evolved. Iodine is probably partially converted into iodic acid.

Hydro-sulphuric acid passed through its alcoholic or acetic acid solution at once decomposes it, converting the iodine into hydriodic acid, with separation of sulphur.

Alkalies and alkaline earths in solution at once decompose it, removing the sulphuric acid and leaving a Naples yellow residue containing the quinine and a portion of the iodine; a soluble iodide of quinine is also formed in ammoniacal liquids.

LIII. *The Theory of the Moon's Motion.—Results of a Third Approximation. By the* Rev. Professor CHALLIS, *M.A., F.R.S.*[*]

IN two previous communications I have shown in what manner terms that may increase indefinitely with the time are introduced into the development of the radius-vector, and what processes are proper for avoiding them. That this is an important point in the lunar theory is evident from the circumstance, that the occurrence of such terms has in a great measure regulated the course which the solution of the problem has taken. It has been thought necessary, in order to get rid of them, to introduce *à priori* into the investigation expressions suggested by the observed motions of the nodes and apses of the moon's orbit. (See Pontécoulant, *Théorie de la Lune*, Chap. I. No. 5.) But it is certain on general principles that there must be a course of reasoning by which the expressions appropriate to the problem may be developed without the necessity of any assumption respecting their form; and as the solution I have commenced in this Magazine appears to be of this kind, I propose now to carry it on to a higher approximation. Although this method may lead to no numerical results differing in any sensible degree from those already obtained, I cannot but think that it is worth while to give to a problem of so great interest and importance as logical a solution as possible.

For the sake of clearness, I shall first recapitulate the previous part of the argument, and add such additional elucidation as it may seem to require. In the Supplement to the Philosophical Magazine for December 1854 (p. 521), an exact first integral of the differential equations is obtained by neglecting the eccentricity of the sun's orbit. Then, after neglecting also the inclination of the moon's orbit to the plane of the ecliptic, two equations (a) and (b) are found, which, if we omit terms multiplied by the first and higher powers of the ratio of the moon's radius-vector to the sun's mean distance, reduce themselves to the following:—

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[*] Communicated by the Author.

$$\frac{dr^2}{dt^2}+r^2\frac{d\phi^2}{dt^2}+\mathrm{C}_1-\frac{2m'}{a'}=\frac{2\mu}{r}+\frac{3n'^2r^2}{2}(1+\cos 2\phi) \quad . \quad (\mathrm{A})$$

$$d\,.\,r^2\left(\frac{d\phi}{dt}+n'\right)=-\frac{3n'^2r^2}{2}\sin 2\phi\, dt. \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{B})$$

In these equations r is the moon's radius-vector at the time t, and, θ being the moon's true longitude at the same time, ϕ is the excess of θ above $n't+\epsilon'$ the sun's mean longitude; μ is the sum of the attractions of the earth and moon, and m' is the sun's attraction, at the unit of distance; a' is the sun's mean distance, so that $a'^3n'^2=m'$; and C_1 is the constant introduced by the integration above mentioned.

The above limitations are made for the sake of simplicity, my object being to exhibit a method of solution rather than to obtain numerical results. By taking account of the eccentricity of the sun's orbit and terms of the fourth order containing the ratio of r to a', the investigation becomes much more complicated, but involves no processes differing in principle from those required under the proposed limitations. The terms depending on the moon's latitude may be considered separately on a future occasion.

The equations (A) and (B) not admitting of exact integration, are to be integrated approximately, and the approximation is to proceed both according to the eccentricity of the orbit and the disturbing force. Hence the first approximation to the orbit is a circle described by the action of the central force $\frac{\mu}{r^2}$. If a be the radius of the circle, it will be readily seen that

$$a=\frac{\mu}{\mathrm{C}_1}, \quad n^2=\mu a^3, \quad \theta=nt+\epsilon.$$

These values are to be used for substitution in the small terms of (A) and (B) in order to proceed to the next approximation, which I have hitherto called the *first* approximation, and for the sake of uniformity shall continue to designate in the same way. In this approximation it is proposed to obtain values of the radius-vector and true longitude to the first order of small quantities.

Consequently the value of $\frac{dr^2}{dt^2}$ must include all small quantities of the second order. I have shown that by merely making use of the equation $\theta = nt + \epsilon$, the following equation may be obtained,

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{n'^2 r^2}{2} + \mathrm{C} = 0, \quad . \quad . \quad . \quad . \quad (\mathrm{C})$$

in which C is put for $\mathrm{C}_1 - \frac{2m'}{a'} - 2n'h$, C_1 and h being the arbitrary constants introduced by the integration. It may here be remarked, that as this equation was obtained by proceeding to small quantities of the second order, it is needless to retain the general value of r in the term $-\frac{n'^2 r^2}{2}$. Putting a for r in that term, and substituting C' for $\mathrm{C} - \frac{n'^2 a^2}{2}$, we have

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} + \mathrm{C}' = 0.$$

This equation gives by integration,

$$r = a(1 - e\cos\psi), \qquad \frac{\mathrm{C}'^{\frac{3}{2}}}{\mu}(t + \mathrm{T}) = \psi - e\sin\psi,$$

$$a = \frac{\mu}{\mathrm{C}'}, \qquad e^2 = 1 - \frac{\mathrm{C}'h^2}{\mu^2}.$$

Hence by substituting for C' its value, and putting m for the ratio of n' to n, it will be found that

$$a = \frac{\mu}{\mathrm{C}}\left(1 + \frac{m^2}{2}\right), \quad e^2 = 1 - \frac{\mathrm{C}h^2}{\mu^2} + \frac{m^2}{2}.$$

But as the approximation gives the values of r only to the first order of small quantities, the above value of a is not true to small quantities of the second order, and in fact is not confirmed by the next approximation. Hence we have simply $a = \frac{\mu}{\mathrm{C}}$.

It is evident that the orbit, as determined by this first approximation, is a fixed ellipse of arbitrary eccentricity e, and that if ϖ be the longitude of the perihelion, the values of the radius-vector and true longitude are the following:

$$r = a\big(1 - e\cos(nt + \epsilon - \varpi)\big)$$
$$\theta = nt + \epsilon + 2e\sin(nt + \epsilon - \varpi).$$

These values are to be substituted in the small terms of the equations (A) and (B) to proceed to the second approximation.

In the Supplement to the Philosophical Magazine for December

1854 (p. 527), I have shown how by including small quantities of the third order, the following equation is obtained:

$$\frac{dr^2}{dt^2}+\frac{h^2}{r^2}-\frac{2\mu}{r}-\frac{n'^2r^2}{2}+\mathrm{C}=$$

$$n'^2a^2e\big(3\cos(2q-p)-\cos(2q+p)\big), \quad . \quad (\mathrm{C}')$$

q being put for $nt+\epsilon-\overline{n't+\epsilon'}$, and p for $nt+\epsilon-\varpi$. Now the mean apsidal distances may be defined to be the values of r for which the part of $\frac{dr^2}{dt^2}$, which is independent of the sun's longitude, vanishes, the mean distance to be the mean between the two apsidal distances, and the mean eccentricity to be the ratio of the difference between the mean apsidal distances to their sum. Hence by equating to zero the left-hand side of the above equation, we have the equation of an orbit in which the mean distance and eccentricity are the same as the mean distance and mean eccentricity of the actual orbit, and which is proper for determining the motion of the apses. Putting the equation under the form

$$r^2\frac{dr^2}{dt^2}+h^2-2\mu r-\frac{n'^2r^4}{2}+\mathrm{C}r^2=0,$$

it will be seen that the integration cannot be exactly performed if the general value of r be retained in the small term. The equation might, however, be integrated by conducting the approximation solely according to the disturbing force, in the manner I have exhibited in the Philosophical Magazine for last February (p. 132). But that process would here be illegitimate, because the equation (C′) was obtained by approximating according to the eccentricity as well as the disturbing force. The following is the process required by the strict rules of approximation. If a be the mean between the two apsidal distances, and $r=a+v$, by hypothesis v will be a small quantity. Hence put $a+v$ for r in the foregoing equation, and in the small term expand to the first power of v, because the approximation includes small quantities of the third order. Or, what is equivalent, simply put $a+\overline{r-a}$ for r in the small term, and expand to the first power of $r-a$. The equation will then become

$$r^2\frac{dr^2}{dt^2}+h'^2-2\mu' r+\mathrm{C}r^2=0, \quad . \quad . \quad . \quad . \quad (\mathrm{D})$$

where $h'^2=h^2+\frac{3n'^2a^4}{2}$, and $\mu'=\mu+n'^2a^3$. By integration this equation gives

$$r=a(1-e\cos\psi),\qquad \frac{\mathrm{C}^{\frac{3}{2}}}{\mu'}(t+\mathrm{T})=\psi-e\sin\psi,$$

$$a=\frac{\mu'}{\mathrm{C}},\qquad e^2=1-\frac{\mathrm{C}h'^2}{\mu'};$$

and if $cn=\frac{\mathrm{C}^{\frac{3}{2}}}{\mu'}$, the apsidal motion in one revolution of the moon from an apse to the same apse again is $2\pi(1-c)$.

These inferences having been drawn from the mean orbit defined by the equation (D), we may proceed to integrate the equation (C′) in the manner I have employed in the Supplement to the Philosophical Magazine for December 1854, the quantity p now representing $cnt+\epsilon-\varpi$. The resulting value of r to small quantities of the second order is as follows:—

$$r=a\left(1-e\cos p+\frac{e^2}{2}-\frac{e^2}{2}\cos 2p-m^2\cos 2q\right).$$

Also since to the same approximation

$$\frac{d\theta}{dt}=\frac{h}{r^2}+\frac{3n'^2}{4n}\cos 2q,$$

we have by integration,

$$\theta=\epsilon+\frac{h}{a^2}\left((1+e^2)t+\frac{2e}{cn}\sin p+\frac{5e^2}{4cn}\sin 2p+\frac{m^2}{n}\cos 2q\right)+\frac{3m^2}{8}\cos 2q.$$

Hence by the definition of n,

$$n=\frac{h}{a^2}(1+e^2).$$

Also

$$\mu'=\mu\left(1+\frac{n'^2a^3}{\mu}\right)=\mu(1+m^2)$$

$$h'^2=h^2\left(1+\frac{3n'^2a^4}{2h^2}\right)=h^2\left(1+\frac{3m^2}{2}\right);$$

because by the former approximation $n^2a^3=\mu$, and $h=na^2$. Consequently to small quantities of the second order,

$$a=\frac{\mu'}{\mathrm{C}}=\frac{\mu}{\mathrm{C}}(1+m^2)$$

$$e^2=1-\frac{h'^2\mathrm{C}}{\mu'^2}\doteq 1-\frac{h^2\mathrm{C}}{\mu^2}+\frac{m^2}{2}$$

$$c=\frac{\mathrm{C}^{\frac{3}{2}}}{\mu' n}=\frac{a\mathrm{C}^{\frac{1}{2}}}{h\left(1+\frac{e^2}{2}\right)}=\frac{\mu}{h\sqrt{\mathrm{C}}}\cdot\frac{1+m^2}{1+\frac{e^2}{2}}.$$

But by the above value of e^2, $\frac{h\sqrt{C}}{\mu}=1-\frac{e^2}{2}+\frac{m^2}{4}$ nearly. Hence it follows that

$$c=1+\frac{3m^2}{4}.$$

Here it is to be remarked, that we have arrived at a value of c which is not true to the second order of small quantities, the true value, as is well known, being $1-\frac{3m^2}{4}$. The explanation of the above result is, that the approximation has not been carried far enough to determine the value of c to the second order of small quantities. The first term in the development of the radius-vector which contains c is $e\cos(cnt+\epsilon-\varpi)$; and if, after putting $1+\alpha$ for c, this term be expanded according to powers of α, the second term of the expansion contains the factor $e\alpha$, which if α be of the second order, is of the *third* order. Consequently, as our approximation only extends to quantities of the second order in the development of r, it does not embrace the value of c to quantities of the second order; it only proves that that quantity contains no term of the first order. Hence, as far as this approximation shows, $c=1$.

In the paper already referred to (p. 528), I have shown how, in the second approximation, to take account of terms which rise in value one order by integration, and have obtained the following complete values of r and ϕ to the second approximation:

$$\frac{r}{a}=1-e\cos p+\frac{e^2}{2}-\frac{e^2}{2}\cos 2p-m^2\cos 2q-\frac{15me}{8}\cos(2q-p)$$

$$\phi=q+2e\sin p+\frac{5e^2}{4}\sin^2 p+\frac{11m^2}{8}\sin 2q+\frac{15me}{4}\sin(2q-p).$$

These are the values to be employed in proceeding to the third approximation.

Before entering upon the next approximation, I propose to indicate another method of obtaining the above values of r and ϕ, which will be found to be convenient in the higher approximations, and adapted to the consideration of the terms that rise in order of value by integration. In this method it will first be necessary to inquire in what way terms of that kind are introduced. It is plain that they must be terms the argument of whose circular function is a multiple of $q-p$, and that, as shown by equations (A) and (B), such terms rise in value by integration when they occur either in the expression for $r^2\sin 2\phi$, or in that for $\frac{d\theta}{dt}$. It may be readily shown that terms of the form $A\cos 2p$ in the value of r, and of the form $A'\sin 2p$ in the value of ϕ, give rise to terms of the form $M \cos 2(q-p)$ in the value of $r^2 \sin 2\phi$. Also a term of the form $B \cos 2(q-p)$, and of the third order in r, will produce a term of the same form and of the second order in the expansion of θ. Let us, therefore, suppose the values of r and ϕ, which are to be substituted in the small terms of the equations (A) and (B) for a second approximation, to be the following:

$$\frac{r}{a}=1-e\cos p+A\cos 2p+B\cos 2(q-p)$$

$$\phi=q+2e\sin p+A'\sin 2p.$$

On going through the calculation, and retaining terms of the *fourth* order which involve the argument $q-p$, the following result is found for the equation C':

$$\frac{dr^2}{dt^2}+\frac{h^2}{r^2}-\frac{2\mu}{r}-\frac{n'^2r^2}{2}+C=$$

$$n'^2a^2\left\{3e\cos(2q-p)-e\cos(2q+p)+\right.$$

$$\left.+\left(\frac{51e^2}{8m}+\frac{3(A-A')}{2m}+\frac{57e^2}{8}\right)\cos 2(q-p)\right\}.$$

In order to integrate this equation, assume

$$\frac{r}{a}=1-e\cos p+\Sigma\,.\,P\cos(\alpha q+\beta p),$$

and substitute in the first side of the equation. Then by making the terms of like argument identical, different corresponding values of P, α, and β are found, which determine the different terms of the development of r. Those of the development of θ may then be obtained by integration. For the values $\alpha=2$, $\beta=-1$, there results

$$P=-\frac{3m^2}{4e}\left(\frac{17e^2}{4m}+\frac{A-A'}{m}\right);$$

and as A is the coefficient of $\cos 2p$ in the development of r, and A' the coefficient of $\sin 2p$ in that of θ, we have

$$A=-\frac{e^2}{2},\quad A'=\frac{5e^2}{4},\quad \text{and } P=-\frac{15me}{8}.$$

The corresponding coefficient in the development of θ is plainly $+\frac{15me}{4}$. It appears also that the coefficient B is $-\frac{15me^2}{16}$, and that the coefficient of the term containing the argument $2(q-p)$ in the expansion of θ is reduced to zero.

Third Approximation.

Small quantities of the *fourth* order are now to be included. Hence from the complete values of r and ϕ given by the second approximation, it is required to obtain values of $r^2 \cos 2\phi$ and $r^2 \sin 2\phi$ to the second order of small quantities for substitution in the right-hand sides of the equations (A) and (B). By the ordinary processes of approximation the following results will be obtained:

$$\begin{aligned}\frac{r^2}{a^2}\cos 2\phi = {} & \cos 2q + e\big(\cos(2q+p) - 3\cos(2q-p)\big) \\ & + e^2\left(-\frac{5}{2}\cos 2q + \cos 2(q+p) + \frac{5}{2}\cos 2(q-p)\right) \\ & + me\left(\frac{15}{8}\cos(4q-p) - \frac{45}{8}\cos p\right) \\ & + m^2\left(-\frac{19}{8} + \frac{3}{8}\cos 4q\right)\end{aligned}$$

$$\begin{aligned}\frac{r^2}{a^2}\sin 2\phi = {} & \sin 2q + e\big(\sin(2q+p) - 3\sin(2q-p)\big) \\ & + e^2\left(-\frac{5}{2}\sin 2q + \sin 2(q+p) + \frac{5}{2}\sin 2(q-p)\right) \\ & + me\left(\frac{15}{8}\sin(4q-p) - \frac{45}{8}\sin p\right) + \frac{3m^2}{8}\sin 4q.\end{aligned}$$

By substituting for $r^2 \sin 2\phi$ in the equation (B), and then after integrating and squaring the result, replacing $\frac{h}{r^2} - n'$ by its approximate value $\frac{d\phi}{dt} - \frac{3n'^2}{4n}\cos 2q$, the following equation is obtained:

$$r^2\frac{d\phi^2}{dt^2} - \left(\frac{h}{r} - n'r\right)^2 =$$

$$\begin{aligned}\frac{3n'^2a^2}{2}\cdot\Big\{ & +\cos 2q + e\left(\frac{5}{3}\cos(2q+p) - 5\cos(2q-p)\right) - \frac{5e^2}{2m}\cos 2(q-p) \\ & + e^2\left(-\frac{47}{6}\cos 2q + \frac{29}{12}\cos 2(q+p) - \frac{9}{4}\cos 2(q-p)\right) \\ & + em\left(\frac{7}{9}\cos(2q+p) - 5\cos(2q-p) + \frac{25}{8}\cos(4q-p) - \frac{75}{8}\cos p\right) \\ & + \frac{e^3}{m}\left(-\frac{5}{2}\cos(2q-p) - \frac{5}{2}\cos(2q-3p)\right) \\ & + m^2\left(\frac{19}{16} + \frac{11}{8}\cos 4q\right)\Big\}.\end{aligned}$$

When the value of $r^2\dfrac{d\phi^2}{dt^2}$ given by this equation and the foregoing value of $r^2\cos 2\phi$ are substituted in the equation (A), there will be found on the right-hand side of the equation the following quantity which does not contain the sun's longitude, viz.

$$\frac{3n'^2a^2}{2}\left(-\frac{57m^2}{16}+\frac{15me}{4}\cos p\right).$$

After substituting for $e\cos p$ its approximate value $1-\dfrac{r}{a}$, this quantity is to be transferred to the left-hand side of the equation. Then putting $a+\overline{r-a}$ for r in the small terms, and expanding $a+\overline{r-a}$ so as to include small quantities of the fourth order, the final result is

$$\frac{dr^2}{dt^2}+\frac{h'^2}{r^2}-\frac{2\mu'}{r}+\mathrm{C}'=$$

$$\begin{aligned}\frac{3n'^2a^2}{2}\cdot\Bigg\{&e\left(-\frac{2}{3}\cos(2q+p)+2\cos(2q-p)\right)+\frac{5e^2}{2m}\cos 2(q-p)\\&+e^2\left(\frac{16}{3}\cos 2q-\frac{17}{12}\cos 2(q+p)+\frac{19}{4}\cos 2(q-p)\right)\\&+em\left(-\frac{7}{9}\cos(2q+p)+5\cos(2q-p)-\frac{5}{4}\cos(4q-p)\right)\\&+\frac{e^3}{m}\left(\frac{5}{2}\cos(2q-p)+\frac{5}{2}\cos(2q-3p)\right)-m^2\cos 4q\Bigg\}\quad(\mathrm{C}'')\end{aligned}$$

where

$$h'^2=h^2-\frac{3n'^2a^4}{2}-\frac{45n'^2ma^4}{4}$$

$$\mu'=\mu-2n'^2a^3-\frac{135}{16}n'^2ma^3$$

$$\mathrm{C}'=\mathrm{C}-3n'^2a^2-\frac{45n'^2ma^2}{8}+\frac{171m^2n'^2a^2}{32}.$$

Consequently the equation applicable to the mean orbit is

$$\frac{dr^2}{dt^2}+\frac{h'^2}{r^2}-\frac{2\mu'}{r}+\mathrm{C}'=0,$$

which gives by integration,

$$r=a(1-e\cos\psi),\qquad \frac{\mathrm{C}'^{\frac{3}{2}}}{\mu'}(t+\mathrm{T})=\psi-e\sin\psi,$$

$$a=\frac{\mu'}{\mathrm{C}'},\qquad e^2=1-\frac{\mathrm{C}'h'^2}{\mu'^2};$$

and if $cn = \frac{C'^{\frac{3}{2}}}{\mu'}$, the apsidal motion in one revolution of the moon is $2\pi(1-c)$.

Since by the second approximation

$$\frac{a^4}{h^2} = \frac{1+e^2}{n^2}, \quad \frac{a^3}{\mu} = \frac{1-\frac{m^2}{2}}{n^2}, \quad \frac{a^2}{C} = \frac{1-\frac{3m^2}{2}}{n^2},$$

the foregoing values of h'^2, μ', and C′ become

$$h'^2 = h^2\left(1 - \frac{3m^2}{2} - \frac{45}{4}m^3 - \frac{3m^2e^2}{2}\right)$$

$$\mu' = \mu\left(1 - 2m^2 - \frac{135}{16}m^3 + m^4\right)$$

$$C' = C\left(1 - 3m^2 - \frac{45}{8}m^3 + \frac{459}{32}m^4\right).$$

In these values the terms of the *fourth* order are to be rejected, because the present approximation extends only to quantities of the third order. Hence it will be found that

$$a = \frac{\mu}{C}\left(1 + m^2 - \frac{45}{16}m^3\right)$$

$$e^2 = 1 - \frac{Ch^2}{\mu^2} + \frac{m^2}{2}.$$

This result proves that the expression for e^2 *contains no term involving* m^3.

Again, in obtaining the value of c, terms of the *third* order are to be omitted, because, for reasons already adduced, this third approximation can give the value of c only to terms of the second order. Now to that order we have, as in the second approximation, $na^2 = h\left(1+\frac{e^2}{2}\right)$. Hence

$$c = \frac{C'^{\frac{3}{2}}}{\mu' n} = \frac{C^{\frac{3}{2}}a^2}{\mu h} \cdot \frac{1-\frac{9m^2}{2}}{\left(1+\frac{e^2}{2}\right)(1-2m^2)};$$

and as $a^2 = \frac{\mu^2}{C^2}(1+2m^2)$ and $\frac{\mu}{hC^{\frac{1}{2}}} = 1 + \frac{e^2}{2} - \frac{m^2}{4}$ nearly, it will be seen that

$$c = 1 - \frac{3m^2}{4},$$

which is the known approximation, obtained by a strictly deductive process.

It would lead me into too complicated calculations, to add to the equation (C″) the terms of the fifth order which receive small denominators by integration, and to obtain the complete values of r and θ to the third order of small quantities. I have indicated above a process proper for this purpose; and having made trial of this process to some extent, I found terms of the third order identical with those given in Pontécoulant's Lunar Theory. In fact, I have no reason to conclude that my solution differs in ultimate analytical expression from previous solutions. The order of the reasoning differs from that of any former method, and is, I think, strictly logical. There is also one important particular in which the reasoning itself is unlike that of any other method. I have commenced the process of approximation after obtaining an exact integral, which had not been previously discovered. On that integration, and on results to which it leads, depends the proof of the new theorem I have published respecting the relation of the eccentricity of the moon's orbit to the disturbing force. I propose to conclude the present communication with some further consideration of this point.

In the Philosophical Magazine for February (p. 135), I have adduced the following argument to show that the eccentricity of the moon's orbit has a special value determined by the disturbing force. The fluctuations of value of the radius-vector, as the moon returns in successive revolutions to the same longitude, depend on the disturbing force in such a manner that the change in each complete revolution would vanish if the disturbing force vanished. The total fluctuation, in the case of a uniform apsidal motion, is the same in all directions after a large number of revolutions of the moon, and depends both on the partial fluctuations and on the interval during which they are additive or subtractive, that is, on the rate of motion of the apses. Hence as the motion of the apses, as well as the partial fluctuations, would vanish with the disturbing force, it follows that the total fluctuation in any given direction, and consequently the eccentricity of the orbit, is a quantity which contains the disturbing force as a factor. This conclusion does not hold good when the force is wholly central, as the orbit in that case may be a circle, and the eccentricity is consequently not restricted to one value.

If the foregoing general argument be true, the same conclusion must be obtainable in a special manner in the course of the investigation of the moon's motion, and I am ready to admit that the new theorem is not established unless it can be so obtained. It will, therefore, be worth while to inquire how far the theorem receives support from the results of the third approximation. First, it may be remarked, that as it has been proved that the expression for e^2 contains no term involving m^3, if the arbitrary constants C and h satisfy the equation $Ch^2=\mu^2$, the consequent value of the eccentricity is the same as that given by the second approximation. Thus the third approximation is not contradictory to the second. At the same time it is incapable of establishing the truth of the theorem, as appears from the following considerations.

In the method of treating the lunar theory which I have proposed, no use is made, as in previous methods, of the radius-vector and longitude of an assumed revolving ellipse; but instead of this process, the terms containing the sun's longitude are separated from those which do not contain it explicitly, and the former being omitted in the values of $\frac{dr^2}{dt^2}$ and $\frac{d\theta}{dt}$, equations are formed which define the orbit that I have called *the mean orbit.* These equations, as given by the third approximation, are the following:

$$\frac{dr^2}{dt^2}+\frac{h'^2}{r^2}-\frac{2\mu'}{r}+C'=0, \quad \frac{d\theta}{dt}=\frac{h}{r^2}.$$

The development of $\frac{d\theta}{dt}$ was found to contain an additional term independent of the sun's longitude, but as it is of the fourth order, it is omitted in the third approximation. By the above two equations the mean orbit is *proved* to be a revolving ellipse for which $c=\frac{h'}{h}$, and the value of h'^2 already obtained shows that to the second order of small quantities $\frac{h'}{h}=1-\frac{3m^2}{4}$. It thus appears that the apsidal motion is deduced in the third approximation from equations which are the same in form as if the force were wholly central, and consequently, so far as this approximation shows, the eccentricity is arbitrary. But on proceeding to the *fourth* approximation, the term above mentioned must be taken into account, in consequence of which $r^2\frac{d\theta}{dt}$ is no longer constant for the mean orbit, and the force cannot be regarded as wholly central. The mean motion of the apse and the form of the mean orbit are now dependent in part on a *tangential* force; and it is perhaps not too much to assert, that under these circumstances the mean orbit cannot be a circle, and consequently that the eccentricity is not arbitrary. Again, the process by which I obtain the value of c appears to indicate that in succeeding approximations, this quantity will be a function of e and m. If so, it cannot be a function of m exclusively, unless the arbitrary constant C satisfy the condition $Ch^2=\mu^2$. It would be necessary, however, in order to settle these points, to complete the fourth approximation, which would require an amount of calculation that I am not prepared to undertake. I must content myself with having thrown out these suggestions.

If the equation $Ch^2=\mu^2$ be true, it follows as a consequence that the equations (C), (C′), (C″), &c. contain the disturbing force as a factor. In my first researches I endeavoured to show antecedently that this is the case. But on reviewing the reasoning on this point, given in the Philosophical Magazine for August 1854, and in the Supplementary December Number (p. 524), I find that the inconsistencies in the approximations to the mean distance and mean motion of the apse there spoken of, disappear when due regard is paid to the order of magnitudes proper to each approximation, and in consequence that the argument founded on the occurrence of those inconsistencies is invalid. This point is fully cleared up in the present communication.

Cambridge Observatory,
April 16, 1855.

LIV. *Note on the Mineral Wilsonite.*

By T. S. Hunt, *of the Geological Commission of Canada*[*].

In the Supplementary Number of this Magazine for July 1854, p. 499, I published descriptions and analyses of two minerals occurring in crystalline limestone in Bathurst, C.W., one of which I proposed as a new species with the name of Wilsonite. It was described as forming rose-red, translucent, cleavable masses, probably monoclinic in crystallization, with a hardness of 3·5, and a density of 2·765 to 2·776. Lustre vitreous, shining occasionally, pearly on the cleavage surfaces. One of three analyses gave silica, 43·55; alumina, 27·94; oxides of iron and manganese, ·20; lime, 6·50; magnesia, 3·81; potash, 8·37; soda, ·95; water (loss by ignition), 8·61=99·93. With two other specimens the amounts of silica, alumina, magnesia, and alkalies were very nearly the same, while the loss by ignition was 9·40 and 9·00, and the amounts of lime respectively 6·94 and 6·72.

The subsequent examination of more perfect specimens in the possession of Prof. E. J. Chapman has shown him that this mineral is triclinic, with inclination to the right, and he has kindly furnished the following notes:—"I give you as approximations, the following angles from cleavage faces by the common goniometer:—P : T=94°; P : *e*=145°; T : *e*=129°; P : M and T : M=110° to 115°. Cleavage with P and T perfect, less so with M; the cleavage with *e* is not easily obtained, but is very distinct and even. Hardness, 3·5 on the more facile cleavages and their combination edges; 5·5 on the ends of the prismatic concretions. Density, 2·77."

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[*] Communicated by the Author.

Prof. Chapman also pointed out to me the existence of intermingled carbonate of lime in the specimens which I had previously analysed, and showed that the loss by ignition was in part carbonic acid. I have since found that the finely pulverized mineral yields up nearly the whole of its lime with effervescence to cold dilute hydrochloric acid, which does not attack the silicate. This still retains its rose colour, and is a hydrous silicate of alumina and potash with a little magnesia. The following are the results of the analyses by myself of two specimens, purified by dilute acid (one being from the same mass as the one given above), and a partial analysis of a third specimen by Prof. H. Croft, which he has kindly communicated to me.

	I.	II.	III.
Silica	47·50	47·70	47·42
Alumina . . .	31·17	31·22	34·80
Magnesia . . .	4·25	4·14	·73
Lime	1·51	·39	·42
Potash	9·22	9·38	
Soda	·82	·95	
Water (by ignition)	5·50	5·35	
	99·97	99·13	

The mineral freed from carbonate of lime seems to be constant in composition; and from its crystallization and density, as well from its chemical constitution, to pertain to the felspars.

Montreal, March 22, 1855.

LV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 309.]

January 18, 1855.—Sir Benjamin Brodie, Bart., in the Chair.

THE following communications were read:—

"On the Dots upon the Valves of the Diatomaceæ." By J. W. Griffith, M.D., F.L.S.

In a former paper, it was stated that the markings or dots upon the valves of the Diatomaceæ, are the optical expressions of depressions existing upon the valves.

All those authors who have paid special attention to the Diatomaceæ, have considered the markings to denote cells; among these we find Ehrenberg*, Kützing†, Ralfs‡, Smith§, and Quekett‖.

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* Die Infusionsthierchen.

† Die Bacillarien, and Spec. Algarum.

‡ Annals of Nat. History, 1843.

§ British Diatomaceæ.

‖ Histological Catalogue of the College of Surgeons; and Lectures delivered before the College of Surgeons.

The evidence I adduced in regard to the more coarsely marked Diatomaceæ, as *Isthmia*, &c., being furnished with depressions and not cells, is, I believe, satisfactory and conclusive; and this view has been admitted in a paper since read before the Royal Society*.

A different view has been taken of the nature of the *finer* markings, as those upon some species of *Gyrosigma*, by the author of the paper last quoted, as by previous authors; and the object of this note is to direct attention to the support which the extended view argued for by me in the paper above referred to, viz. that the finer markings also correspond to depressions, derives from analogy.

The structure of the Diatomaceæ, and their modes of reproduction, are, as is well known, remarkable;—so much so, that these organisms have been claimed by botanists as members of the vegetable, and by zoologists as belonging to the animal kingdom. The preponderance of evidence is decidedly in favour of their vegetable nature; but, be this as it may, they must all be classed together,—they form a perfectly natural family. Hence we have a strong argument in favour of the markings upon their valves being identical, and as these are evidently depressions in the genera and species with coarsely marked valves (*Isthmia*, &c.), we should expect from analogy that the same would apply to those with finer markings. And this view receives further support, from the fact, that under varied methods of illumination, corresponding appearances are presented by the markings when viewed by the microscope, from those which are very large, as in *Isthmia*, through those of moderate and small size, as in the species of *Coscinodiscus*, down to those in which they are extremely minute, as in the species of *Gyrosigma*, &c. The angular (triangular or quadrangular) appearance assumed by the markings, arises from the light transmitted through the valves being unequally oblique. This may be readily shown in the more coarsely marked valves (*Isthmia*, *Coscinodiscus*), which present the true structural appearance when the light is reflected by the mirror in its ordinary position, and the spurious angular appearance when the light is rendered oblique by moving the mirror to one side.

"Researches on the Theory of Invariants." By William Spottiswoode, M.A., F.R.S.

Invariants may be regarded from two points of view, the permutational and the functional. According to the former they are considered as arising from a process of permutation, according to the latter as derivatives from other functions. In this paper the latter course is adopted; and the following is an outline of the method:—

Let $$u=f(x, y, \ldots a_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots)$$

be any homogeneous function of the degree n of the variables $x, y, \ldots$, in which $a_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots$ multiplied by their respective multinomial coefficients are the coefficients of $x^{\alpha} y^{\beta} \ldots, x^{\alpha_{\prime}} y^{\beta_{\prime}} \ldots, \ldots$ Let $x, y \ldots$ undergo any linear transformation, and let $l, m, \ldots, l', m', \ldots, \ldots$ be the coefficients of transformation; also let

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* Proceedings of the Royal Society, June 15, 1854.

$$\Delta = \left| \begin{matrix} l & m & .. \\ l' & m' & .. \\ . & . & .. \end{matrix} \right|$$

$$\left.\begin{aligned} \triangleright &= l'\frac{d}{dl} + m'\frac{d}{dm} + .. \\ \triangleright' &= l''\frac{d}{dl} + m''\frac{d}{dm} + .. \\ . & \quad . \quad .. \end{aligned}\right\}$$

let $\mathrm{a}_{\alpha\beta..}$, $\mathrm{a}_{\alpha_{\prime}\beta_{\prime}..}$, .. be the new values of $a_{\alpha\beta..}$, $a_{\alpha_{\prime}\beta_{\prime}..}$, ..,; also let

$$\nabla = l\frac{d}{dx} + m\frac{d}{dy} + ..$$

$$\nabla' = l'\frac{d}{dx} + m'\frac{d}{dy} + ..$$

$$. \quad . \quad . \quad ..$$

then, à une facteur près,

$$\mathrm{a}_{\alpha\beta..} = \nabla^{\alpha}\nabla'^{\beta} .. u$$

and

$$\frac{d\mathrm{a}_{\alpha\beta..}}{da_{\alpha\beta..}}, \quad \frac{d\mathrm{a}_{\alpha\beta..}}{da_{\alpha_{\prime}\beta_{\prime}..}}, ..$$

will be respectively equal to the combinations of $l, m, .. l', m', .., ..$ by which the *a*s are multiplied in $\mathrm{a}_{\alpha\beta..}$. Again, let f' represent a function similar to f, excepting that in the former the multinomial coefficients are dropped; then if

$$\square_{no..} = f'\left(l, m, .. \frac{d}{da_{\alpha\beta..}}, \frac{d}{da_{\alpha_{\prime}\beta_{\prime}..}}, ..\right)$$

$$\square_{\alpha\beta..} = \frac{\triangleright^{n-\alpha}\triangleright'^{\beta} ..}{1.2..(n-\alpha).1.2...\beta..}\square_{no}..$$

Now the fundamental condition of invariance is expressed by the following equation:—

$$\Delta^{\overline{\frac{mn}{q}}} \mathrm{F}(a_{\alpha\beta..}, a_{\alpha_{\prime}\beta_{\prime}..}, ..) = \mathrm{F}(\mathrm{a}_{\alpha\beta..}, \mathrm{a}_{\alpha_{\prime}\beta_{\prime}..}, ..),$$

$$= \mathrm{F}(a_{\alpha\beta..}\,\square_{\alpha\beta..}, a_{\alpha_{\prime}\beta_{\prime}..}\,\square_{\alpha_{\prime}\beta_{\prime}..}, ..)\mathrm{U},$$

where $\mathrm{U} = \mathrm{F}(a_{\alpha\beta..}, a_{\alpha_{\prime}\beta_{\prime}..}, ..)$,

and the problem consists in determining the coefficients of U. This gives

$$\Delta^{\overline{\frac{mn}{q}}} (\overline{\alpha\beta..}^{i}, \overline{\alpha_{\prime}\beta_{\prime}..}^{i_{\prime}}, ..) = \square^{i}_{\alpha\beta..}\,\square^{i_{\prime}}_{\alpha_{\prime}\beta_{\prime}..} .. \mathrm{U},$$

the quantity within brackets being the coefficient of

$$a^{i}_{\alpha\beta..}\,\mathrm{a}^{i_{\prime}}_{\alpha_{\prime}\beta_{\prime}..} ..$$

Then if the term be selected in which $i = i_{\prime} = .. = 1$, and if $\Delta^{\overline{\frac{mn}{q}}}$ be so transformed that it shall appear as a function linear in each of the systems

$$\frac{d\mathrm{a}_{\alpha\beta..}}{da_{\alpha\beta..}}=(00),\ \frac{d\mathrm{a}_{\alpha\beta..}}{da_{\alpha_{,}\beta_{,}..}}=(01)..$$

$$\frac{d\mathrm{a}_{\alpha_{,}\beta_{,}..}}{da_{\alpha\beta..}}=(10),\ \frac{d\mathrm{a}_{\alpha_{,}\beta_{,}..}}{da_{\alpha_{,}\beta_{,}..}}=(11)..$$

$$.\qquad\qquad .\qquad\qquad ..$$

and if

$${}_{\alpha\beta..}\Box=a_{\alpha\beta..}\frac{d}{d(00)}+a_{\alpha_{,}\beta_{,}..}\frac{d}{d(01)}+..$$

$${}_{\alpha_{,}\beta_{,}..}\Box=a_{\alpha\beta..}\frac{d}{d(10)}+a_{\alpha_{,}\beta_{,}..}\frac{d}{d(11)}+..$$

$$.\qquad\quad .\qquad\quad .\qquad\qquad ..$$

then

$${}_{\alpha\beta..}\Box_{\alpha_{,}\beta_{,}..}\Box\,..\,\Box_{\alpha\beta..}\Box_{\alpha_{,}\beta_{,}..}..=1$$

and U will be at once given by the equation

$$\mathrm{U}=(\overline{\alpha\beta..},\ \overline{\alpha_{,}\beta_{,}..},\ ..)_{\alpha\beta..}\Box_{\alpha_{,}\beta_{,}..}\Box\,..\Delta^{\frac{mn}{q}}.$$

This method has been applied in the case of two variables to the calculation of quadratic and cubic invariants.

But in the case of two variables the coefficient a may be expressed by a series of symbols with a single suffix, thus:

$$a_0,\ a_{,},\ ..\quad \mathrm{a}_0,\ \mathrm{a}_{,}\ ..,\quad \Box_0,\ \Box_{,},\ ..\quad {}_0\Box,\ {}_{,}\Box,\ ..$$

Now since Δ is of the same degree in l, m, and in l', m', the coefficients of all powers and products of a_0, $\mathrm{a}_{,}$, .. in which the degree of l, m is above or below that of l', m', will vanish in the invariant. And from this and some other considerations it is shown, that not only

$$\Box_0^m\mathrm{U}=0,\ \triangleright\Box_0^m\mathrm{U}=0,\ ..\ \triangleright^{\frac{mn}{q}-1}\Box_0^m\mathrm{U}=0,$$

but that the coefficients of the invariant may be calculated from the equations arising from equating to zero each term of the last equation above written, operated upon by the symbol $\triangleright$. This always gives a system of linear equations for finally determining the coefficients. It does not appear possible fully to explain this method without entering into more details than can be given in an abstract.

From the general equations for so determining the coefficients, the number and degrees of the distinct invariants belonging to any given function may theoretically be determined; and this has been done for the simplest case, viz. quadratic functions. But the expressions for higher degrees appear so complicated that an answer to this important question can hardly be expected from this method, in any case not already known.

The view of invariants here taken has suggested a series of other functions of which invariants form the last term. These functions, which I propose to call Variants, may be thus expressed. If functions of the degrees r, s, .. (r, s, .. being less than n) have invariants of the degree m, then writing

$$\partial=\frac{d}{dl},\quad \partial_{,}=\frac{d}{dm}$$

$$\Delta^{\frac{mr}{2}}\mathrm{F}\left(\partial^{r}\mathrm{a}_0,\ \frac{1}{r}\partial^{r-1}\partial_{,}\mathrm{a}_0,\ ..\ \partial_{,}^{r}\mathrm{a}_0\right)=\mathrm{F}_{m,\,n-r}$$

$$\Delta^{\frac{ms}{2}}\mathrm{F}\left(\partial^{s}\mathrm{a}_0,\ \frac{1}{s}\partial^{s-1}\partial_{,}\mathrm{a}_0,\ ..\ \partial_{,}^{s}\mathrm{a}_0\right)=\mathrm{F}_{m,\,n-s}$$

$$\cdot\qquad\cdot\qquad\qquad\cdot\cdot$$

$$\Delta^{\frac{mn}{2}}\mathrm{F}\left(\partial^{n}\mathrm{a}_0,\ \frac{1}{n}\partial^{n-1}\partial_{,}\mathrm{a}_0,\ ..\ \partial_{,}^{n}\mathrm{a}_0\right)=\mathrm{F}_{m,\,0}$$

of which the last is a simple invariant, since, omitting the factor $n(n-1)..2.1$,

$$\partial^{n}\mathrm{a}_0=a_0,\ \frac{1}{n}\partial^{n-1}\partial_{,}\mathrm{a}_0=a_{,},\ ..\ \partial_{,}^{n}\mathrm{a}_0=a_n.$$

With the exact relation between these functions and covariants I am not at present acquainted.

Jan. 25, 1855.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; preceded by an Account of some Experiments on Molecular Influences."—The Bakerian Lecture. By John Tyndall, Ph.D., F.R.S.

The paper commences with an introduction, in which the present aspect of the portions of science to which it refers is briefly sketched. A section is devoted to the examination of the magnetic properties of wood, which substance, the author finds, except where extraneous impurities are present, to be always diamagnetic, and to set in the magnetic field with its fibre equatorial. The influence of the shape of pointed and flat poles is studied, and those curious phenomena of rotation, first observed by M. Plücker, and attributed by him to the action of two conflicting forces, are referred to molecular structure as a cause. Between flat poles, it is proved that the line joining the centres of the two poles is the line of *minimum* force; that is to say, the force increases more quickly from the central point of the magnetic field in an equatorial direction than in an axial one. Reflecting on the great diversity of opinion at present existing with regard to the real nature of the diamagnetic force, the author deemed it necessary to commence at the foundation of the inquiry. A fundamental question in the present case is the following:—Are diamagnetic bodies repelled by a magnet in virtue of any constant property possessed by the mass? Is the force in question a mere repulsion of ordinary matter, or is the repulsion exercised in virtue of a state of magnetization into which the body is first thrown? This question is answered in a manner which admits of no doubt.

It is proved that the repulsion of diamagnetic bodies increases in a quicker ratio than the strength of the magnet which produces the repulsion. Within wide limits, indeed, the repulsion, instead of being simply proportional to the strength of the magnet, is proportional to the square of the strength, which leads inevitably to the conclusion that the body thus repelled contributes to the effect produced; that its repulsion is due to an excited condition into which it is thrown by the influencing magnet, the intensity of this excitement varying within the limits already referred to, as the strength of the magnet which produces it. This conclusion is further arrived at by a close comparison of the repulsions of diamagnetic bodies with the attraction of paramagnetic ones: both are found subservient to one and the same law.

It is next proved that the diamagnetic excitement produced by one pole of a magnet is not the state which enables a pole of an opposite quality to repel the substance:—that each pole induces a condition peculiar to itself, or, in other words, that the excitement of diamagnetic bodies in the magnetic field is of a *dual* character.

These points being established, a searching comparison is instituted between the phenomena exhibited by paramagnetic and diamagnetic bodies in three distinct cases:—first, when operated on by the magnet alone; secondly, when operated on by the current alone; and, thirdly, when operated on by the magnet and current combined. A bar of iron was, in some of these cases, compared with a bar of bismuth, but it was soon found necessary, in order to avoid the proved errors of reasoning, to take strict account of the molecular structure of the bismuth. A bar of this substance, cut in a certain manner from the crystallized mass, exhibits between the poles of a magnet precisely the same visible deportment as a bar of iron, while it is well known that the normal deportment of bismuth is opposed to that of iron. The author, in his examination of the points before us, divided paramagnetic bars into two distinct classes, and classified diamagnetic bars in the same manner; one class he called *normal*, and the other class *abnormal*. A normal paramagnetic bar is one which sets its length from pole to pole in the magnetic field, and a normal diamagnetic bar is one which sets its length at right angles to the line joining the poles. An abnormal magnetic bar, on the contrary, is one which sets its length equatorially; while an abnormal diamagnetic bar is one which sets its length axially.

In all cases, whether operated on by the magnet alone, the current alone, or the magnet and current combined, the deportment of the normal paramagnetic bar is precisely antithetical to that of the normal diamagnetic one. In the magnetic field the former sets axially, the latter equatorially. Operated on by a voltaic current, the former sets its length at right angles to the current, the latter sets its length parallel to it. When magnet and current act together on the bars, it is found that the disposition of forces which produces a deflection from right to left of the paramagnetic bar produces a deflection from left to right of the diamagnetic bar. If the position of equilibrium of the former be from N.E. to S.W., the position of equilibrium of the latter is from N.W. to S.E. In short, the position of rest for the normal magnetic bar is always at right angles to the position of the diamagnetic bar. A precisely similar antithesis is observed when we compare the abnormal paramagnetic bar with the abnormal diamagnetic one. The former, in the magnetic field, sets equatorially, the latter axially. The former sets parallel to an electric current, the latter perpendicular to the same. If the deflection of the former be from right to left, the deflection of the latter, under like conditions, is from left to right. Finally, the position of equilibrium of the former is always at right angles to that of the latter.

But if the deportment of the normal paramagnetic bar be compared with that of the abnormal diamagnetic one, it will be found that they are in all cases identical; and the same identity of deportment is exhibited when the abnormal paramagnetic bar is compared with the normal diamagnetic one. The necessity of paying attention to structure in experiments of this nature, could not, it is imagined, be more strikingly exhibited.

It is proved by these experiments that the simple substitution of an attractive force for a repulsive one would completely convert the phenomena exhibited by paramagnetic bodies into those exhibited by diamagnetic ones; that if that which Gauss has called the ideal distribution of magnetism in an iron bar be reversed, we have a distribution which would produce the phenomena of a bismuth bar of the same dimensions. All the phenomena of diamagnetic bodies become equally intelligible with those of paramagnetic ones, when we assume that the former class possess a polarity the same in kind, but the opposite in direction, to that of the latter.

It is well known that a bar of iron surrounded by a helix in which a voltaic current circulates is converted into a magnet, and exhibits that *twoness* of action,—those phenomena of attraction and repulsion at its two ends—to which we give the name of polarity. The present paper contains an account of experiments made with the view of ascertaining whether similar phenomena were exhibited by a bar of pure bismuth. A cylinder of the latter substance, 6½ inches long and 0·4 of an inch in diameter, was suspended by a suitable device within a helix of covered copper wire, so that it could vibrate freely from side to side. The ends of two electro-magnetic cores were brought to bear upon the two ends of the bismuth bar, and it was so arranged that the two magnetic poles acting upon the bar might be of the same or of opposite qualities. The helix being first excited by a strong current, a current of considerably less power was sent round the electro-magnetic cores, and their action upon the bismuth bar was observed: by means of a gyrotrope the current in the helix was reversed, and the effect of the reversion noted; permitting the current through the helix to flow in its last direction, by means of a second gyrotrope, the current which excited the cores was reversed; and finally, while the last magnetic polarity remained unaltered, the direction of the current in the helix was once more changed, and the consequent deportment of the bismuth bar was noted.

In making these experiments and exercising some judgment in the choice of the relation between the strength of the magnets and the strength of the current in the helix, the most complete mastery was attained over the motions of the bar. With one disposition of the forces the ends of the bar of bismuth were promptly repelled, with another arrangement they were just as promptly *attracted.* If, when moving towards the cores, the direction of the current in the helix was reversed, the motion of the bar was at once arrested, and attraction was converted into repulsion. If, while receding from the bar, the direction of the current in the helix was changed, the recession was stopped and the ends of the bar were attracted. The same results were obtained when, instead of changing the direction of the current in the helix, the polarity of the electro-magnetic cores was reversed. When the latter were so excited that poles of the same quality were presented to the ends of the bismuth bar, the repulsion of the one pole was balanced by the attraction of the other, and under the influence of these opposing forces the bar stood still.

Pursuing the argument further, a south pole and a north pole were caused to act simultaneously upon *each* end of the bismuth bar; supposing one end of the latter to be repelled by a south pole, then, on the assumption of diamagnetic polarity, the same end would be attracted by a north pole; and permitting both poles to act upon it simultaneously from *opposite sides*, we may anticipate that the force tending to turn the bar will be greater than if only a single pole were used. To test this conclusion, *four* electro-magnetic cores were made use of; the two poles to the right of the bismuth bar were of the same name, while the two to the left of the bismuth bar were of an opposite quality: with this arrangement the mechanical action upon the bar was greatly augmented, and the foregoing anticipation completely verified.

The bar employed in these experiments is unusually large, but it does not mark the practical limit of success. All the results obtained with this bar were obtained with another solid cylinder of bismuth 14 inches long and 1 inch in diameter. The corresponding experiments were made with bars of iron, and it was always found that the arrangement of forces which caused the attraction of the ends of the paramagnetic bar caused the repulsion of the ends of the diamagnetic one; while the disposition which caused the repulsion of the ends of the paramagnetic bar produced, in the most manifest manner, the *attraction* of the ends of the diamagnetic one. As regards the abstract question of polarity, the only difference between iron and bismuth is the comparatively intense action of the former. In the case of a magnetic body, whose capacity for magnetization does not exceed that of bismuth, and in which coercive force is equally absent, no proof can be adduced in favour of the polarity of the former body that cannot be matched by proofs of equal value in every respect of the polarity of the latter.

The objections that have been and possibly may be used against diamagnetic polarity, are next considered, and some observations are made on the constitution of the magnetic field. The relation of our present knowledge of diamagnetic action to the theory of Weber, and to Ampère's hypothesis of molecular currents is stated; and in conclusion, the author dwells briefly upon those points of diamagnetic action wherein the views of M. Matteucci differ from his own.

"On Differential Transformation and the Reversion of Serieses."

By J. J. Sylvester, Esq., F.R.S.

With a view to its publication in the Proceedings of the Society, I take occasion to communicate the result of my investigations, as far as they have yet extended, into the general theory of differential transformations, containing a complete and general solution of the important problem of expanding a given partial differential coefficient of a function in respect of one system of independent variables in terms of the partial differential coefficients thereof, in respect to a second system of independent variables, each respectively given as explicit functions of the first set.

This question may be shown to be exactly coincident with that of the reversion of simultaneous serieses proposed by Jacobi, which may be thus stated: given $(n+1)$ quantities, each expressed by rational infinite serieses as functions of n others; required to express any one of the first set in a rational infinite series in terms of the other n of the same set. This question has only been resolved by Jacobi for a particular case; the result hereunder given for the transformation of differential coefficients contains the solution of the general question. My method of investigation is entirely different from that adopted by the great Jacobi, and I hope in a short time to be able to lay it in a complete form before the Society, and probably to add a solution of the still more general question comprising the reversion of serieses as a particular case, viz. the question of expressing any one of n quantities connected by m equations in terms of any $(n-m)$ others of the same.

Let there be any number of variables, say u, v, w, of which x, y, z, ϑ are given functions, it is required to expand

$$\left(\frac{d}{dx}\right)^f \left(\frac{d}{dy}\right)^g \left(\frac{d}{dz}\right)^h \vartheta$$

in terms of the partial differential coefficients of ϑ, x, y, z in respect of u, v, w.

Form the determinant

$$\begin{matrix} \frac{dx}{du}, & \frac{dx}{dv}, & \frac{dx}{dw}, \\ \frac{dy}{du}, & \frac{dy}{dv}, & \frac{dy}{dw}, \\ \frac{dz}{du}, & \frac{dz}{dv}, & \frac{dz}{dw}, \end{matrix}$$

which call J.

The required expansion will contain in each term an integer numerical coefficient, a power of $\frac{1}{J}$, one factor of the form

$$\left(\frac{d}{du}\right)^p \left(\frac{d}{dv}\right)^q \left(\frac{d}{dw}\right)^r \vartheta,$$

and other factors of the form

$$\left(\frac{d}{du}\right)^{l}\left(\frac{d}{dv}\right)^{m}\left(\frac{d}{dw}\right)^{p}x$$

$$\left(\frac{d}{du}\right)^{l'}\left(\frac{d}{dv}\right)^{m'}\left(\frac{d}{dw}\right)^{p'}y$$

$$\left(\frac{d}{du}\right)^{l''}\left(\frac{d}{dv}\right)^{m''}\left(\frac{d}{dw}\right)^{p''}z.$$

Let the latter class of factors be distinguished into two sets, those where $l+m+n=1$,

$$\begin{pmatrix} li.i\,l=0 & m=0 & n=0 \\ \text{or } l=0 & m=1 & n=0 \\ \text{or } l=0 & m=0 & n=1 \end{pmatrix}$$

which I shall call uni-differential factors, and those in which $l+m+n > 1$, which I shall call pluri-differential factors.

First, then, as to the form of the general term abstracting from the numerical coefficient and the uni-differential factors (except of course so far as they enter into J). This will be as follows:—

$$\left(\frac{d}{du}\right)^{{}^1l_1}\left(\frac{d}{dv}\right)^{{}^1m_1}\left(\frac{d}{dw}\right)^{{}^1n_1}x\times\left(\frac{d}{du}\right)^{{}^2l_1}\left(\frac{d}{dv}\right)^{{}^2m_1}\cdot\left(\frac{d}{dw}\right)^{{}^2n_1}x\times\ldots\left(\frac{d}{du}\right)^{{}^{e_1}l_1}\left(\frac{d}{dv}\right)^{{}^{e_1}m_1}\left(\frac{d}{dw}\right)^{{}^{e_1}n_1}x$$

$$\times\left(\frac{d}{du}\right)^{{}^1l_2}\left(\frac{d}{dv}\right)^{{}^1m_2}\left(\frac{d}{dw}\right)^{{}^1n_2}y\times\ldots\ldots\times\left(\frac{d}{du}\right)^{{}^{e_1}l_2}\left(\frac{d}{dv}\right)^{{}^{e_2}m_2}\left(\frac{d}{dw}\right)^{{}^{e_2}n_2}y$$

$$\times\left(\frac{d}{du}\right)^{{}^1l_3}\left(\frac{d}{dv}\right)^{{}^1m_3}\left(\frac{d}{dw}\right)^{{}^1n_3}z\times\ldots\ldots\times\left(\frac{d}{du}\right)^{{}^{e_3}l_3}\left(\frac{d}{dv}\right)^{{}^{e_3}m_3}\left(\frac{d}{dw}\right)^{{}^{e_3}n_3}z$$

$$\times\left(\frac{d}{du}\right)^{p}\left(\frac{d}{dv}\right)^{q}\left(\frac{d}{dw}\right)^{r}\vartheta\times\frac{1}{J^{\omega}},$$

subject to the limitations about to be expressed.

Call
$$\begin{aligned} {}^1l_1+{}^2l_1+\ldots+{}^{e_1}l_1 &= L_1 \\ {}^1l_2+{}^2l_2+\ldots+{}^{e_2}l_2 &= L_2 \\ {}^1l_3+{}^2l_3+\ldots+{}^{e_3}l_3 &= L_3, \end{aligned}$$

and form the analogous quantities M_1, M_2, M_3; N_1, N_2, N_3. Then we must have

$$\begin{aligned} L_1+L_2+L_3+M_1+M_2+M_3+N_1+N_2+N_3+p+q+r \\ =f+g+h+e_1+e_2+e_3; \end{aligned}$$

and as the sum of any group of indices l, m, n must be not less than 2, we have

$$f+g+h+e_1+e_2+e_3+p+q+r, \text{ not less than } 2e_1+2e_2+2e_3,$$

so that $e_1+e_2+e_3$ must not exceed $f+g+h+p+q+r$; furthermore, $p+q+r$ must not exceed $f+g+h$; and finally,

$$\omega=f+g+h+e_1+e_2+e_3.$$

1. We may first take $e_1+e_2+e_3=E$, giving to E in succession all integer values from $f+g+h$ to $2f+2g+2h$, and find all possible so-

lutions of this equation with permutations between the values of e_1, e_2, e_3.

2. We may then take $p+q+r=s$, giving s in succession all integer values from 1 to $f+g+h$, and find all possible solutions of this equation with permutations between f, g, h.

3. We may then take $L+M+N=f+g+h+E-s$, and find all the values of L, M, N, with permutations allowable between the values of L, M, N.

4. We may then take

$$\begin{aligned} L_1+L_2+L_3&=L\\ M_1+M_2+M_3&=M\\ N_1+N_2+N_3&=N, \end{aligned}$$

and solve these several equations in every way possible, with permutations as before.

5. We must take

$$\begin{array}{lll} {}^1l_1+{}^2l_1+\ldots+{}^{e_1}l_1=L_1 & {}^1m_1+{}^2m_1+\ldots+{}^{e_1}m_1=M_1 & {}^1n_1+{}^2n_1+\ldots+{}^{e_1}n_1=N_1\\ {}^1l_2+{}^2l_2\;\ldots\ldots\;{}^{e_2}l_2=L_2 & {}^1m_2+{}^2m_2\;\ldots\ldots\;{}^{e_2}m_2=M_2 & {}^1n_2+{}^2n_2\qquad\qquad {}^{e_2}n_2=N_2\\ {}^1l_3+{}^2l_3\;\ldots\ldots\;{}^{e_3}l_3=L_3 & {}^1m_3+{}^2m_3\;\ldots\ldots\;{}^{e_3}m_3=M_3 & {}^1n_3+{}^2n_3\qquad\qquad {}^{e_3}n_3=N_3 \end{array}$$

and solve in every possible manner these equations, but without admitting permutations between the values of ${}^1l_1\ {}^2l_1 \ldots {}^{e_1}l_1$, or between the values of the members of the other of the third sets taken each *per se*, and subject to the condition that every such sum as ${}^rl_i+{}^rm_i+{}^rn_i$ must be greater than unity. Every possible system of values of these nine sets will furnish a corresponding pluri-differential part to the general term.

Next, as to the uni-differential part, we may form the quantity

$$\left(\frac{dy}{dv}\cdot\frac{dz}{dw}-\frac{dy}{dw}\,\frac{dz}{dv}\right)^{\lambda_1}\left(\frac{dy}{dw}\cdot\frac{dz}{du}-\frac{dy}{du}\,\frac{dz}{dw}\right)^{\mu_1}\left(\frac{dy}{du}\,\frac{dz}{dv}-\frac{dy}{dv}\,\frac{dz}{dw}\right)^{\nu_1}$$

$$\left(\frac{dz}{dv}\,\frac{dx}{dw}-\frac{dz}{dw}\,\frac{dx}{dv}\right)^{\lambda_2}\left(\frac{dz}{dw}\,\frac{dx}{du}-\frac{dz}{du}\,\frac{dx}{dw}\right)^{\mu_2}\left(\frac{dz}{du}\,\frac{dx}{dv}-\frac{dz}{dv}\,\frac{dx}{dw}\right)^{\nu_2}$$

$$\left(\frac{dx}{dv}\,\frac{dy}{dw}-\frac{dx}{dw}\,\frac{dy}{dv}\right)^{\lambda_3}\left(\frac{dx}{dw}\,\frac{dy}{du}-\frac{dx}{du}\,\frac{dy}{dw}\right)^{\mu_3}\left(\frac{dx}{du}\,\frac{dy}{dv}-\frac{dx}{dv}\,\frac{dy}{dw}\right)^{\nu_3},$$

where

$$\begin{aligned} \lambda_1+\lambda_2+\lambda_3&=L+p\\ \mu_1+\mu_2+\mu_3&=M+q\\ \nu_1+\nu_2+\nu_3&=N+r. \end{aligned}$$

These equations are to be solved in every possible manner with permutations between the members of the λ set, the μ set, and the ν set. Finally, we have to consider the numerical coefficient. To give a perfect representation of this, we must ascertain what identities exist in the factors of the pluri-differential part. Let us suppose that one set of operators upon x is repeated θ_1 times, another θ_2 times, and so on, giving rise to the powers $\theta_1, \theta_2, \ldots\ldots \theta_\alpha$ in the x line. Similarly, form $\phi_1, \phi_2, \ldots \phi_\beta$ from the y line, and $\psi_1, \psi_2 \ldots \psi_\gamma$

from the *z* line. Then the numerical part of the general term will be

$$\frac{\Pi(\lambda_1+\mu_1+\nu_1)\Pi(\lambda_2+\mu_2+\nu_2)\Pi(\lambda_3+\mu_3+\nu_3)}{\Pi\lambda_1\Pi\mu_1\Pi\nu_1\Pi\lambda_2\Pi\mu_2\Pi\nu_2\Pi\lambda_3\Pi\mu_3\Pi\nu_3}$$

$$\times\frac{\Pi(\mathrm{L}+p)\Pi(\mathrm{M}+q)\Pi(\mathrm{N}+r)}{\left\{\begin{array}{l}\Pi^1 l_1\Pi^1 m_1\Pi^1 n_1\Pi^2 l_1\Pi^2 m_1\Pi^2 n_1\ \ldots\ldots\\ \Pi^1 l_2\Pi^1 m_2\Pi^1 n_2\Pi^2 l_2\Pi^2 m_2\Pi^2 n_2\ \ldots\ldots\\ \Pi^1 l_3\Pi^1 m_3\Pi^1 n_3\Pi^2 l_3\Pi^2 m_3\Pi^2 n_3\ \ldots\ldots\end{array}\right\}}$$

$$\times\frac{\mathrm{D}}{\Pi\theta_1\Pi\theta_2\ldots\Pi\theta_\alpha\Pi\phi_1\Pi\phi_2\ldots\Pi\phi_\beta\Pi\psi_1\Pi\psi_2\ldots\Pi\psi_\gamma},$$

where in general Πm means $1.2.3\ldots m$: as regards D, it is the following determinant, viz.

$$\begin{array}{cccccc}
\lambda_1+\mu_1+\nu_1 & \nu & \nu & \mathrm{L}_3 & \mathrm{M}_3 & \mathrm{N}_3\\
\nu & \lambda_2+\mu_2+\nu_2 & \nu & \mathrm{L}_2 & \mathrm{M}_2 & \mathrm{N}_2\\
\nu & \nu & \lambda_3+\mu_3+\nu_3 & \mathrm{L}_1 & \mathrm{M}_1 & \mathrm{N}_1\\
\lambda_1 & \lambda_2 & \lambda_3 & \mathrm{L}_1+\mathrm{L}_2\ \mathrm{L}_3+p & \nu & \nu\\
\mu_1 & \mu_2 & \mu_3 & \nu & \mathrm{M}_1+\mathrm{M}_2+\mathrm{M}_3 & \\
\nu_1 & \nu_2 & \nu_3 & \nu & \nu & \mathrm{N}_1+\mathrm{N}_2+\mathrm{N}_3+r
\end{array}$$

The result, for greater brevity, has been set out in the above pages for the case of ϑ, a function of three variables, but the reader can have no difficulty in extending the statement to any number. In the case of a single variable, the formula can easily be identified with that given by Burman's law. It is noticeable that the determinant above written is of the form

$$\mathrm{A}pqr+\mathrm{B}pq+\mathrm{C}qr+\mathrm{D}qrp+\mathrm{E}pe+\mathrm{F}q+\mathrm{G}r,$$

the part independent of p, q, r being easily seen to vanish. Moreover, A, B, C, D, E, F, G, H are all essentially positive, so that D can only vanish (except for $p=0$, $r=0$, $q=0$) by virtue of one condition at least more than the number of the variables.

Feb. 15, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read:—

"On the Computation of the Effect of the Attraction of Mountain-masses, as disturbing the apparent astronomical latitude of stations in Geodetic Surveys." By George B. Airy, Esq., F.R.S., Astronomer Royal.

The author commences with remarking that his surprise had been excited by the result obtained by Archdeacon Pratt*, namely, that the computed attraction of the elevated country north-east of India considerably exceeds the disturbance which it was sought to explain. But on consideration the author perceived that this result might have been anticipated, on the extensively received supposition that the interior of the earth is a dense fluid or semi-fluid (which for convenience he calls *lava*), and that the exterior crust floats upon it. For, he remarks, this crust cannot be supposed at any part to be very high upwards (as in mountains), at least to any great horizontal extent, unless there is a corresponding projection downwards into the lava. Upon making a numerical calculation, even with the crust 100 miles thick, it was shown that there would be such a tendency of the table-land to crack and sink in the middle as no cohesion of rocks can resist. He conceives that the state of the ground may be properly illustrated by a raft of timber floating on water: if one piece of timber projects higher into air than the others, we are certain that it also projects lower into water than the others. Assuming this as established, then it is evident that the horizontal attraction of a mountain-mass on a point at a considerable distance is nearly evanescent, because the increase of attraction of the part which is above the general level is sensibly neutralized by the deficiency of attraction below it where the lighter crust displaces the heavier lava. In like manner, the horizontal attraction of a ship or other floating body is nothing. But the horizontal attraction upon a near point on the earth's surface will not vanish, because the mountain which produces the positive attraction is nearer than the lava-displacement which produces the negative attraction: even here, however, the efficient disturbing attraction will be much less than that computed by considering the dimensions of the mountain only.

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* Proceedings of the Royal Society, December 7, 1854.

Note to a paper entitled "Contributions to the Anatomy of the Brachiopoda," read June 15, 1854. By Thomas H. Huxley, Esq., F.R.S.

My attention having been called within the last two or three days, to an error in my paper on the Anatomy of the Brachiopoda, published in No. 5 of the Royal Society's Proceedings, I beg to be allowed to take the earliest opportunity of correcting it. At p. 111 of that paper the following paragraph will be found:—

"In 1843, however, M. Vogt's elaborate Memoir on *Lingula* appeared, in which the true complex structure of the 'heart' in this genus was first explained and the plaited 'auricle' discriminated from the 'ventricle;' and in 1845, Professor Owen, having apparently been thus led to re-examine the circulatory organs of the Brachiopoda," &c. &c.

Now, in point of fact, though M. Vogt *does* describe and accurately figure the structures called 'auricle' and 'ventricle' in *Lingula*[*], yet he has not only entirely omitted to perceive their connexion, or to indicate the 'auricular' nature of the former, but he expressly states that the so-called 'hearts' are "simple, delicate, pyriform sacs" (p. 13).

I presume that my recollection of M. Vogt's figures was more vivid than that of his text; for having been unable, notwithstanding repeated endeavours, to re-obtain the memoir when writing my paper, I felt justified in trusting to what seemed my very distinct recollection of its sense. I had the less hesitation in doing this, as in M. Vogt's subsequently published 'Zoologische Briefe*,' he gives the received interpretation to the parts of the so-called 'hearts' without any indication of a change of opinion.

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[*] Neue Denkschriften der allgemeinen Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. Band VII.

I make this statement in explanation of what might otherwise seem to be great carelessness on my part, and for the purpose of further pointing out that M. Vogt not having made the supposed discovery, it is quite impossible that Professor Owen's researches should have been suggested by it.

Feb. 22, 1855.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Temperature and Density of the Seas between Southampton and Bombay *viâ* the Mediterranean and Red Seas." By MM. Adolphe, Hermann, and Robert Schlagintweit.

In this communication the authors give the results of the observations they had made during their voyage, relative to the temperature and specific gravity of the sea-water, both near the surface and at depths ranging from about 18 to 30 metres, the latter being nearly the greatest depth which the motion of the vessel permitted them to reach. They reserve for a future report their observations on the temperature and moisture of the air, as well as the results of two experiments on the quantity of carbonic acid contained in the air on the Mediterranean and Red Seas.

The instruments employed in the observations here described were as follows:—

(1.) Four thermometers which had been carefully compared at the Kew Observatory previous to the authors' departure. At Bombay they repeated the determination of the zero-point and of another standard point, and found that the thermometers had not varied.

(2.) A dipping apparatus constructed by Mr. Adie. This apparatus, which held 5 or 6 litres, was furnished with two valves, so arranged that as it descended the water passed freely through, but as soon as a commencement was made of drawing it up the valves closed and rendered it water-tight. The authors assured themselves that the temperature of the enclosed water did not sensibly change during the process of drawing it up.

(3.) An areometer from Mr. T. G. Greiner at Berlin. This instrument permitted the specific gravity to be read off directly to three places of decimals, and the fourth could be supplied by estimation.

To render the observations of specific gravity comparable with one another, it was necessary to reduce them to a common temperature, which occasioned some difficulty, as the exact expansion of sea-water between the limits 20° and 25° C. was not accurately known. By means of a delicate voluminometer, constructed for the purpose by M. Geissler of Berlin, the authors determined the expansion to be

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* Frankfort, 1851, vol. i. p. 285.

0·000271 for 1° C. For distilled water Halström had found 0·000219. Another set of more direct experiments, made at Bombay, gave for the expansion of sea-water 0·000337. The difference between this value and the preceding the authors refer to a change of volume of the voluminometer itself, and they prefer the latter, which accordingly they use in their reductions.

The authors then give tables of the results of their observations, which are followed by some general remarks.

Atlantic.—The temperature of the Atlantic was found to be—

From lat. 46° to 41° N.	17°·5 to 18°·5 C.
From lat. 39° to 37°	20° to 21° C.

Mean specific gravity reduced to 17°·5 C.=1·0277.

The temperature and specific gravity showed very little variation in the open sea, so long as no currents were met with, but in the vicinity of land, disturbances of various kinds were noticed. In harbours and in small bays the temperature of the water was found to diminish sensibly at a depth of from 15 to 20 metres, but in the open sea the temperature at the very surface was generally found somewhat lower than at a depth of 30 metres, which no doubt was due to evaporation.

Straits of Gibraltar.—A current with a mean velocity of from three to six miles an hour usually flows through the Straits from the Atlantic into the Mediterranean. A counter current is supposed to exist underneath, but the great depth of the Straits prevented the authors from reaching any such current with their dipping apparatus.

East of the Straits the water of the Atlantic was met with in several places, in close proximity with water of the Mediterranean, from which it was distinguished by its temperature and colour. The stream from the Atlantic on passing the Straits seems to divide itself into several branches.

In connexion with the variability of the currents in the Straits, it is worthy of remark that the *unreduced* specific gravity of the water of the Mediterranean and of the Atlantic is nearly the same.

Mediterranean :—

From the Straits of Gibraltar to Malta—

Temperature of the water	21°·7 to 22° C.
Specific gravity reduced to 17°·5 C.	1·0287.

From Malta to Alexandria—

Temperature	23° to 24° C.
Reduced specific gravity	1·0298.

Red Sea.—The maximum of specific gravity found during the voyage was in the northern end of the Gulf of Suez, 1·0393 (reduced).

From lat. 27° to 23° N., temperature 24° to 28° C.: reduced sp. gr. 1·0315.
From lat. 22° to 14° temperature 30° to 31°·5 C.: reduced sp. gr. 1·0306.

Straits of Bab-el-Mandeb.—The water of the Gulf of Aden being less dense, though colder, than that of the Red Sea, flows into the latter on both sides of the Island of Perim. This colder water could be detected half a degree north of the Straits. After some further remarks about this current, the authors pass on to the

Arabian Sea—in which they found

From long. 44° to 50° East from Greenwich, temp. 28°·8, reduced sp. gr. 1·0275.
From the merid. of Cape Guardafui to Bombay, temp. 27° to 28°, red. sp. gr. 1·0278.

LVI. *Intelligence and Miscellaneous Articles.*

ON SENSITIVE COLLODION. BY THOMAS WOODS, M.D.

IN the Number of the Photographic Journal for December last, Mr. Doyle asked me a question about the quantity of the gun-cotton which is precipitated by the solution of salt I recommend to be added to the collodion to prepare it for my process. I sent an answer to the succeeding Number of the Journal, but I was not aware at that time how much I ought to have explained. Questions since put to me on the same subject caused me to read over carefully the paper originally sent to the Philosophical Magazine of July last, and I find I have in it made an omission which no doubt has caused the process to be unsuccessful. When I recommended a saturated solution of salt to be used, I meant the solution to be made with alcohol, not with water. If made with the latter, it not only would precipitate too largely, but otherwise destroy the mixture; and I am afraid such has been the case with the operators who have tried the process. As my experience of the process with the iodide of iron has quite answered my own expectations, and, when the proper solutions have been used, in the hands of others the promises I have made for it, I am tempted to rewrite the formula I use, exactly as it should be, at the risk of occupying too much space by a repetition. It may not, however, be without its value just now, as the season is becoming more favourable for photographic operations, and I know of no process more sensitive or certain than that I describe.

Take of—

Sulphate of iron	40 grs.
Iodide of potassium............	24 grs.
Chloride of sodium (common salt)	6 grs.
Alcohol	2 ozs.
Strong water of ammonia	3 drops.

Mix the powdered salts together and add them to the alcohol, then the water of ammonia. A few pieces of iron wire must be kept in the mixture to prevent the iron becoming peroxidized.

One part of this mixture is to be added to three parts of collodion holding in solution an *alcoholic* solution of common salt in the proportion of 1 fluid drachm of salt to 4 ozs. of collodion. Or, neglecting the salt solution, 5 drops of *chloroform* may be added to 1 drachm of the solution of iodide of iron and 3 drachms of *plain* collodion. The mixture of collodion and iodide of iron ought to be used shortly after having been made, as the iron becomes peroxidized and spoilt by a prolonged contact with the collodion.

The nitrate of silver solution for exciting the plate should be of the strength of 30 grs. to the ounce of water.

The developing solution may be either the ordinary sulphate of iron, or the pyrogallic acid.

With a good lens and a good light an almost instantaneous picture may be produced by this process.

A little water of ammonia added to the hyposulphite bath brings out the picture more fully when a very short exposure is given.

Sometimes the common table salt contains a little *carbonate* of soda; if such be used in the process, it causes cloudiness.—*From the Journal of the Photographic Society,* April 21, 1855.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1855.

Chiswick.—March 1. Heavy rain: very fine: halo round the moon in the evening. 2. Cloudy: rain. 3. Low white clouds: heavy rain: clear and fine. 4. Clear: cloudy and fine. 5. Slight fog: very fine. 6. Cloudy and fine: foggy. 7. Frosty: fine. 8. Very clear: fine: sharp frost. 9. Frosty: few snow-flakes: overcast. 10. Snowing: sharp frost at night. 11. Cloudy: rain at night. 12. Cloudy: rain. 13. Fine. 14. Fine: cloudy. 15. Cloudy: rain. 16. Densely overcast: fine. 17. Fine: rain: clear. 18. Cloudy and boisterous. 19. Showery. 20. Foggy: very fine. 21. Cloudy and cold: hail-shower. 22. Sleet: cold rain: overcast: boisterous. 23, 24. Cloudy and cold. 25. Fine: sharp frost at night. 26. Clear: cloudy: fine. 27. Very fine. 28. Drizzly: heavy rain. 29. Cloudy: clear and fine. 30. Cloudy. 31. Heavy clouds: clear and fine.

Mean temperature of the month	37°·61
Mean temperature of March 1854	42 ·54
Mean temperature of March for the last twenty-nine years...	42 ·24
Average amount of rain in March	1·33 inch.

Boston.—March 1. Fine: rain P.M. 2. Fine. 3. Fine: stormy and rain A.M. 4, 5. Fine. 6, 7. Foggy. 8. Fine. 9. Cloudy. 10. Fine. 11. Fine: snow P.M. 12. Cloudy: rain P.M. 13. Rain: rain A.M. 14. Cloudy. 15. Fine. 16. Fine: rain A.M. 17. Cloudy: rain A.M. and P.M. 18, 19. Fine. 20, 21. Cloudy. 22. Cloudy: snow A.M. 23—25. Cloudy. 26. Fine. 27, 28. Cloudy. 29—31. Fine.

Sandwick Manse, Orkney.—March 1. Showers A.M. and P.M. 2. Rain A.M. and P.M. 3. Rain A.M.: drizzle P.M. 4. Clear A.M.: cloudy, large solar halo P.M. 5. Clear A.M.: cloudy P.M. 6. Bright A.M.: clear, fine P.M. 7, 8. Bright, hoar-frost A.M.: clear, frost P.M. 9. Bright, hoar-frost A.M.: clear, drops P.M. 10. Snow-showers A.M.: cloudy P.M. 11. Cloudy A.M.: snow-showers P.M. 12. Snow-showers A.M.: snow-drift P.M. 13. Snow A.M.: cloudy P.M. 14. Bright A.M.: clear, aurora P.M. 15. Bright A.M.: cloudy P.M. 16. Rain A.M.: cloudy P.M. 17. Clear A.M.: damp P.M. 18. Rain A.M.: showers, aurora P.M. 19. Bright A.M.: clear, aurora P.M. 20. Bright A.M.: clear P.M. 21. Clear A.M.: clear, aurora P.M. 22—24. Snow-showers A.M.: clear P.M. 25. Snow-showers A.M. and P.M. 26. Snow A.M.: clear, frost P.M. 27. Snow, clear A.M.: cloudy P.M. 28. Cloudy A.M.: clear P.M. 29, 30. Clear A.M. and P.M. 31. Bright A.M.: clear P.M.

Mean temperature of March for twenty-eight previous years...	40°·53
Mean temperature of this month	36 ·61
Mean temperature of March 1854	45 ·14
Average quantity of rain in March for fourteen previous years	2·52 inches.

The mean temperature of this month is lower than that of March during any of the twenty-eight years of my observations, except in 1837, when it was 36°·54, and in 1839, when it was 36°·33. That of March last year was the highest during the whole period.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *and by the* Rev. C. Clouston, *at Sandwick Manse,* Orkney.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. March.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·625	29·527	29·20	28·88	28·97	53	39	37	38	40	sw.	ssw.	w.	·09	·21	·20
2.	29·355	29·001	28·95	28·77	28·71	53	38	42·5	38	38½	sw.	sw.	se.	·23	·06	·28
○ 3.	29·434	28·898	28·42	28·59	28·74	50	30	42·5	40	40	sw.	ssw.	wnw.	·06	....	·23
4.	29·576	29·573	29·26	29·24	29·36	49	27	34	39½	37	sw.	sw.	e.	....	·05	·16
5.	29·793	29·730	29·40	29·25	29·46	53	26	36	40½	40½	se.	ssw.	w.	....	....	·02
6.	29·866	29·851	29·58	29·78	29·88	51	23	33	41	34	ne.	calm	calm	....	....	....
7.	29·892	29·816	29·58	29·94	30·11	46	22	31·5	38	32	e.	ne.	ne.	....	....	....
8.	30·086	30·066	29·80	30·15	30·05	45	18	34	34	33½	ne.	n.	se.	....	....	·02
9.	30·063	29·894	29·77	29·86	29·79	41	27	34	36	36½	e.	e.	sse.	....	....	....
10.	29·838	29·754	29·58	29·65	29·58	40	17	32	37	36	se.	ne.	sse.	....	....	·44
☾ 11.	29·673	29·374	29·43	29·40	28·96	37	29	27·5	36	32	w.	se.	sse.	·33	....	....
12.	29·018	28·886	28·62	28·76	29·00	46	31	34	34½	31½	sw.	sw.	ese.	·24	·35	·62
13.	29·522	29·206	28·87	29·20	29·42	49	29	37	34	34	sw.	w.	ese.	....	·25	·06
14.	29·634	29·594	29·34	29·50	29·68	45	31	36	36½	31	s.	s.	ne.	·01	....	·02
15.	29·793	29·522	29·50	29·66	29·40	46	39	33	36½	36½	se.	sse.	sse.	·21	....	....
16.	29·707	29·518	29·17	29·26	29·24	53	32	39	35	36½	w.	sw.	sse.	....	·25	·40
17.	29·641	29·498	29·25	29·34	29·22	50	33	45	40½	37	sw.	s.	ene.	·09	....	·24
● 18.	29·730	29·667	29·25	29·21	29·58	48	34	42	38	37	w.	w.	nnw.	·02	·15	·16
19.	29·922	29·877	29·59	29·80	29·89	55	30	36·5	36	33½	sw.	w.	n.	·01	....	·11
20.	29·824	29·735	29·50	29·89	29·83	58	37	42	36	33	se.	ne.	calm	....	....	....
21.	29·391	29·107	29·20	29·66	29·60	45	34	38	38½	32½	ne.	ne.	ne.	·02	....	....
22.	28·908	28·882	28·77	29·60	29·58	36	33	34	35	32	ne.	ene.	calm	·13	....	·10
23.	29·180	29·025	28·85	29·37	29·32	38	27	32	38	33	n.	n.	n.	....	·03	·04
24.	29·301	29·283	29·08	29·35	29·40	38	25	34	36½	34½	nw.	ne.	nnw.	....	....	·09
☽ 25.	29·621	29·402	29·22	29·45	29·48	43	18	35·5	35½	35	ne.	n.	w.	....	....	·05
26.	29·662	29·645	29·40	29·55	29·70	45	29	31	34½	32½	ne.	n.	nw.	....	....	·29
27.	29·821	29·786	29·50	29·86	30·04	51	27	37·5	37½	36	s.	nw.	nw.	....	....	·02
28.	30·230	30·029	29·75	30·11	30·26	41	31	37·5	40	41	e.	......	w.	·29	....	....
29.	30·434	30·420	30·13	30·40	30·45	44	25	38·5	46	40½	ne.	n.	calm.	·02	....	....
30.	30·438	30·418	30·13	30·43	30·39	46	29	39	43	35½	ne.	n.	ne.	....	....	·03

THE
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[FOURTH SERIES.]

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LVII. *On the Action of Non-conducting Bodies in Electric Induction.* By P. Riess*.

ELECTRIC induction, that wonderful property which an electrified body possesses of eliciting two electricities in any body in its vicinity, has been the subject of many investigations which would have been quite superfluous had the clear insight into that phænomenon, already obtained, been strictly adhered to. According to the conclusion deduced from Franklin's theory, and accepted without question by Æpinus, Coulomb and Poisson, the body provided with two electricities does not act differently from what it would do if it were possessed of [illegible] the induction, and to electrify the induced body directly and in exactly the same manner, by means of a rubbed glass rod and piece of sealing-wax. According to this view, every experiment in which an induced and an inducing body are allowed to act upon a third body, becomes a particular case of so complicated a nature as to render its investigation apparently quite hopeless. If we reflect that the determination of the electric distribution in such a body subject to the simple induction of another electrified body is a problem not yet solved, nor perhaps capable of complete solution, it will be evident that to determine, according to general laws, the influence of three different quantities of electricity, whose distribution is unknown, upon any body, would be a hopeless undertaking. But, inasmuch as the true notions of induction have been departed from, it has become necessary to investigate particular cases of the above kind for the sole purpose of bringing the results of experiment into harmony with the theory above mentioned. Thus, a long time ago, I considered the case

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* From Poggendorff's *Annalen*, vol. xcv. p. [illegible]

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Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. March.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·625	29·527	29·20	28·88	28·97	53	39	37	38	40	sw.	ssw.	w.	·09	·21	·20
2.	29·355	29·001	28·95	28·77	28·71	53	38	42·5	38	38½	sw.	sw.	se.	·23	·06	·28
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4.	29·576	29·573	29·26	29·24	29·36	49	27	34	39½	37	sw.	sw.	e.	....	·05	·16
5.	29·793	29·730	29·40	29·25	29·46	53	26	36	40½	40½	se.	ssw.	w.	....	....	·02
6.	29·866	29·851	29·58	29·78	29·88	51	23	33	41	34	ne.	calm	calm	....	....	....
7.	29·892	29·816	29·58	29·94	30·11	46	22	31·5	38	32	e.	ne.	ne.	....	....	....
8.	30·086	30·066	29·80	30·15	30·05	45	18	34	34	33½	ne.	n.	se.	....	....	·02
9.	30·063	29·894	29·77	29·86	29·79	41	27	34	36	36½	e.	e.	sse.	....	....	....
10.	29·838	29·754	29·58	29·65	29·58	40	17	32	37	36	se.	ne.	sse.	....	....	·44
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12.	29·018	28·886	28·62	28·76	29·00	46	31	34	34½	31½	sw.	sw.	ese.	·24	·35	·62
13.	29·522	29·206	28·87	29·20	29·42	49	29	37	34	34	sw.	w.	ese.	....	·25	·06
14.	29·634	29·594	29·34	29·50	29·68	45	31	36	36½	31	s.	s.	ne.	·01	....	·02
15.	29·793	29·522	29·50	29·66	29·40	46	39	33	36½	36½	se.	sse.	sse.	·21	....	....
16.	29·707	29·518	29·17	29·26	29·24	53	32	39	35	36½	w.	sw.	sse.	....	·25	·40
17.	29·641	29·498	29·25	29·34	29·22	50	33	45	40½	37	sw.	s.	ene.	·09	....	·24
● 18.	29·730	29·667	29·25	29·21	29·58	48	34	42	38	37	w.	w.	nnw.	·02	·15	·16
19.	29·922	29·877	29·59	29·80	29·89	55	30	36·5	36	33½	sw.	w.	n.	·01	....	·11
20.	29·824	29·735	29·50	29·89	29·83	58	37	42	36	33	se.	ne.	calm	....	....	....
21.	29·391	29·107	29·20	29·66	29·60	45	34	38	38½	32½	ne.	ne.	ne.	·02	....	....
22.	28·908	28·882	28·77	29·60	29·58	36	33	34	35	32	ne.	ene.	calm	·13	....	·10
23.	29·180	29·025	28·85	29·37	29·32	38	27	32	38	33	n.	n.	n.	....	·03	·04
24.	29·301	29·283	29·08	29·35	29·40	38	25	34	36½	34½	nw.	ne.	nnw.	....	....	·09
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27.	29·821	29·786	29·50	29·86	30·04	51	27	37·5	37½	36	s.	nw.	nw.	....	....	·02
28.	30·230	30·029	29·75	30·11	30·26	41	31	37·5	40	41	e.	......	w.	·29	....	....
29.	30·434	30·420	30·13	30·40	30·45	44	25	38·5	46	40½	ne.	n.	calm.	·02	....	....
30.	30·438	30·418	30·13	30·43	30·39	46	29	39	43	35½	ne.	n.	se.	....	....	·03
31.	30·408	30·375	30·10	30·37	[illegible]	45	22	40	40½	37½	ne.	n.	sse.			

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* From Poggendorff's *Annalen*, vol. xcii. p. 337.

I may here be allowed to remark, that in the discussion on the non-activity of induced electricity, I have been censured for having expressed myself against the term bound or latent electricity as usually employed, and that my proposition to abandon the use of this term has hitherto met with little approval: I think, unjustly. It is not asserted that a wrong meaning is attached to a word, but that in other parts of physics a different interpretation is attached to this word. A quantity of heat is said to be latent when it has no effect upon the thermometer, but a quantity of bound or latent electricity has its full effect upon an electroscope. No doubt it is possible to attach a proper interpretation to an inappropriate term, but the necessity for doing so ought to be avoided. When, of a certain quantity of electricity, a part is said to be latent, we mean that, besides the action of the quantity under consideration, that of a smaller quantity of unlike-named electricity must be taken into account. But to require such an expression to be thus interpreted appears to me unadvisable. I proceed with the subject of the present memoir.

In the 11th, 12th, and 13th series of his imperishable Experimental Researches, Faraday has occupied himself with induction, and endeavoured to establish the notion that induction is not produced by the action of electricity across space, but that an electric body acts only on the contiguous particles of an insulating medium, and that this action is transmitted from particle to particle of that medium. Thus induction should depend essentially upon the nature of the medium which separates the inducing from the induced conductor, and on this account the medium is called a *dielectric*. A long time ago* I showed that this theory was untenable, because it was based upon the assumption, proved by experiment to be incorrect, that of the three quantities of electricity, present whenever an electrified body acts upon a neutral one, two quantities being completely occupied with one another have no further action; I have intimated that the experiments which ought to support this theory may be explained by the simultaneous action of all three electricities. Such an intimation appeared to me sufficient, inasmuch as these experiments belong to those complicated cases which were mentioned at the commencement of this paper. There still exists, however, an uncertainty as to the signification of a part of those experiments, which are the more particularly important because they appear to establish an essential difference between the inductive actions of conducting and non-conducting bodies, and on that account it will not be deemed superfluous to remove that uncertainty in an experimental manner.

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* Vide *Repertorium der Physik*, 1842, p. 129.

In order to determine what he calls the specific inductive capacities of insulators, Faraday employed an instrument called the differential inductometer*, which consists of three, or simpler of two, insulated metallic discs placed at a short distance from one another with their faces parallel; one disc was charged with electricity directly, and the electric state of the other disc, which was charged by the inductive action of the first, examined. When between both plates, but without touching them, a non-conducting plate of shell-lac or sulphur was introduced, the amount of induced electricity was found to be greater than before, *i. e.* when both discs were separated from one another by air only. It follows from other experiments made by Faraday†, that the induction would have been diminished had a conducting plate been introduced between the two; for, according to Faraday's opinion, the introduction of the conducting plate would have caused the induction to take place in curved lines around the edges of the plate, instead of in right lines through the intervening stratum of air.

The experiments with insulating intermediate plates have been repeated with various results. In accordance with Faraday, Knockenhauer‡ found, by introducing a plate of shell-lac between two discs, an augmentation of induced electricity upon the induced plate, but could not conceive the cause of this result. Müller§, who considered this result to be incompatible with the cause of the phænomenon, found on frequent repetition of the experiment a diminution of the induced electricity, and ascribed the results of both his predecessors to errors in the testing of the kind of electricity here excited.

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* Experimental Researches, 1307. † Ibid. 1218.

‡ Pogg. *Ann*. vol. li. p. 126.

§ *Bericht über die neusten Fortschritte der Phys.*, Braunschweig, 1849, p. 61.

In order to give certain and unequivocal results, the experiment in question requires some care when arranging the apparatus, which I will consequently describe at length. Two metallic discs, insulated on glass pillars, were placed vertically on a table with their faces turned towards each other, and both parallel to the edge of the table at which the experimenter sat. The second or most distant plate was connected, by means of a wire 18 inches long, with a gold-leaf electroscope; and in finer experiments, with a dry-pile electroscope, and the first or nearest disc was charged with positive electricity. The electroscope indicated positive electricity, which was removed by uninsulation. If, during the course of the experiment, the quantity of induced electricity on the disc in connexion with the electroscope was increased, in other words, if the induction of the charged disc was apparently *strengthened,* the electroscope again indicated *positive* electricity; on the contrary, if that induction was apparently *weakened,* it indicated *negative* electricity. In order to avoid all uncertainty about the result thus obtained, the electroscope was uninsulated after the change in the induced electricity, and the indication of the electroscope observed after the cause of this change had been removed: here an indication of positive electricity proved that in the original experiment the induced electricity had been diminished; of negative electricity, that the induction had been increased. After every experiment such a check was applied, so that no doubt can be entertained as to the correctness of the observations. At first two metallic discs, 7·21 inches in diameter and 0·06 inch thick, were used, whose distance asunder was 1·11 inch. A disc of shell-lac, 7·21 inches in diameter and 0·33 inch thick, which had received a smooth reflecting surface by pressure between two glass plates, was fixed by means of a clamp to a glass pillar, 5 inches long; the latter was covered with shell-lac and turned on a point, so as to admit of its being placed in any required position between the two metallic discs. When the disc of shell-lac, after being divested of all electricity by means of a spirit-lamp flame, was brought in the middle between the two discs and made concentric with them, the gold-leaf or dry-pile electroscope showed that the induced electricity upon the disc in connexion with the electroscope was considerably *increased.* On the contrary, when the disc of shell-lac only occupied a portion of the intermediate space (a spindle-shaped portion about half an inch in breadth), and its greatest part remained without that space, the induced electricity was, although only slightly, yet quite decidedly *diminished.* I mention this, because whatever may be the constitution and shape of the interposed disc, the electroscope always indicates a diminution in the amount of induced electricity at the moment when it comes between the metallic discs, a diminution which will be disregarded in the following experiments; this experiment alone, however, is necessarily at variance with the assumption, that the induction of a charged metallic disc is stronger through shell-lac than through air, inasmuch as no reason can be assigned why a small piece of shell-lac should weaken an action which a greater piece strengthens. The experiments described at the end of the present memoir will furnish the explanation of this remarkable phænomenon.

In order to operate with smaller intermediate plates, the large metallic discs were replaced by smaller ones, 4·46 inches in diameter, at a distance of 1·65 inch from one another. Here, however, an important influence on the result of the experiment was produced by changing the position of the clamp. On this account the clamp was abandoned, and above the metallic discs a metallic cylinder 3 inches long was fixed, in whose axis a metallic rod could turn softly; this rod carried at one end a round nut for adjustment, and at the other a clamp 1½ inch long. With respect to this clamp, a convenient arrangement may be here noticed; one of its legs is immoveable, and the other can be moved on a prism by means of a screw, so as to keep the two legs always parallel to one another. The handle of a plate being fixed in this clamp, a rotation of 90° given to the axis of the cylinder was sufficient either to bring the plate between the metallic discs, or to remove it sufficiently from them, without altering the position of the apparatus towards the discs, exception being made of the screw on the clamp, whose influence, when present, was taken into account. This arrangement of the apparatus enabled the observer to preserve his arm and hand unmoved during an experiment, inasmuch as a slight pressure of the finger was sufficient to bring the plate into the two required positions. The immoveability of the observer is necessary in some of the following experiments, in which importance is attached to small motions of the gold leaves on the electroscope. In the experiments immediately following, the indications of the electroscope were so decided as to render this precaution unnecessary. The following table gives the changes which, according to the two-fold indications of the electroscope, were produced in the induced electricity of the neutral metallic disc by interposing the several plates there mentioned. The interposed plates were fixed to glass or shell-lac pillars, from 4 to 6 inches long, and before the experiments were completely deprived of their electricity.

Diameter of metallic discs 4·46 inches.
Distance asunder . . . 1·65 ...

	Intermediate plate.	Form.	Linear dimensions in inches.	Thickness in inches.	Induced electricity of the neutral disc.
A.	Shell-lac.	Disc.	Diam. 4·16 4·20	0·11	Increased.
a.	„	„	„ 1·29	0·26	Increased.
B.	Paraffine*.	„	„ 4·63	0·50	Increased.
b.	„	„	„ 2·53	0·30	Increased.
β.	„	„	„ 2·36	0·11	Increased.
C.	Gutta-percha.	„	„ 5·06	0·14	Increased.
c.	„	„	„ 2·16	0·14	Increased.
	Mica.	Rectangle.	6·2 by 4·1	0·004	Decreased.
D.	Glass.	Disc.	Diam. 2·20	0·16	Increased.
d.	„	Square.	Side 4·1	0·17	Increased.
δ.	„	Rectangle.	9·60 by 10·25	0·06	Decreased.

The majority of these experiments, therefore, confirm Faraday's statement, that by placing an insulating plate between a charged and a neutral metallic disc, the induced electricity upon the latter is increased. That this increase, however, is not an essential property of the insulator is shown by the two experiments with the mica plate, and the glass plate δ, both of which caused a diminution in the induced electricity, although they are by no means inferior to gutta-percha in their insulating capacities. Further, that this increase is not exclusively produced by insulating substances is proved by the following experiments, in which, by employing perfectly conducting intermediate plates fixed to insulating pillars, the same phænomenon is apparent.

Indeterminate plate.	Diameter in inches.	Thickness in inches.	Induced electricity of the neutral disc.
Tinfoil.	1·56	0·002	Increased.
Brass.	0·67	0·25	Increased.
Silver.	1·56	0·12	Increased.
Copper.	4·03	0·03	Decreased.

It is manifest that with metals the difference between the actions of two plates is solely due to their dimensions, so that none of these results can appear strange. When both discs stand alone, the neutral one is acted upon by a stratum of positive electricity 1·65 inch distant. By the interposition of a conducting plate, two electric strata at a smaller distance are superadded, of which the positive stratum lies nearer to the excited disc than the negative. Both strata contain the same quantity of electricity; but the distribution of these quantities in the strata depends not only upon the dimensions and distance between the two discs, but also upon the form and dimensions of the intermediate plate itself. By changing the dimensions of the intermediate plate, its action upon the induced disc will be changed in a twofold manner; the two electricities upon it obtain another distribution and quantity, and the distances at which these strata act upon the induced disc are changed. We can determine two strata, such that their total action upon the induced disc shall be inappreciable, or like in kind to the action of the directly charged disc, or lastly, unlike to this action. The question, therefore, in what manner will a conducting intermediate plate change the induced electricity which a charged disc produces on a neutral one, is completely indeterminate. The question will be a determinate one if the conducting plate is not insulated, for then to the action of a charged disc that of an unlike stratum of electricity is added, which necessarily opposes that action. But to attempt to compare the action of conducting and non-conducting intermediate plates under these circumstances, would be to commit an experimental mistake not less grave, but less excusable, than the mistake of those earliest electricians, who, without insulating the conductors, compared conductors with non-conductors with respect to their capacities of becoming electric by friction.

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* Commercial paraffine from the Augustinhütte, Bonn.

The simplest form was now given to the induction experiment, and the action of intermediate non-conducting plates examined. Two metallic spheres, 0·87 inch in diameter, were fixed to thin glass rods, and so placed that their central line lay horizontal, and the distance between their nearest points amounted to a little more than 0·6 inch. One sphere was charged with electricity, the other was connected, by means of a wire 18 inches long, with a dry-pile electroscope, and observations were conducted in the manner already described. Every intermediate plate was carefully placed in the space between the two spheres, so that the line joining the centres of the spheres passed perpendicularly through the centre of the plate.

	Intermediate plate.	Induced electricity on the neutral sphere.
A.	Shell-lac.	Decreased.
a.	,,	Increased.
b.	Paraffine.	Increased.
β.	,,	Increased[*].
C.	Gutta-percha.	Decreased.
c.	,,	Increased.
D.	Glass.	Increased.
d.	,,	Decreased.

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[*] A disc of paraffine, of similar dimensions to *β*, gave a decided decrease of the induced electricity; I have not recorded the observations with this disc, because it was considerably thinner in the middle than at the edges.

From this it is manifest that intermediate plates of the same non-conducting substance sometimes decreased, and sometimes increased the induced electricity of the neutral sphere; and further, by comparison with the former experiments, that one and the same non-conducting plate, which increased the induced electricity upon a neutral disc, decreased the same when the neutral body was a sphere. Hence the important theorem :—

In experiments on induction, the effect of the action of non-conducting intermediate plates depends upon the form and dimensions, not only of those plates, but also of the conductor used in the experiment. This theorem negatives, in the simplest manner, the assumption that the intermediate plates directly influence the induction of the originally charged conductor. We cannot directly ascertain in what manner the action of insulating plates exerts itself, because the electric condition of these plates cannot be examined, and reference must be had to the analogous action of conducting intermediate plates, in which this examination is possible. The following discs, on insulating pillars, were placed between the spheres.

Intermediate disc.	Diameter in inches.	Thickness in inches.	Induced electricity of the neutral sphere.
Brass.	0·67	0·25	Increased.
"	"	0·04	Decreased.
Silver.	1·56	0·12	Increased.
Tinfoil.	"	0·002	Decreased.
Copper.	4·03	0·03	Decreased.

Here, undoubtedly, both the electricities induced upon the intermediate disc acted. In order to find the general distribution of these electricities, the discs were placed separately at a distance of a quarter of an inch from the surface of the positively charged sphere, in such a manner that the line joining the centres of the sphere and disc was perpendicular to the latter. Several points of the disc were then touched with a pin's head, insulated by shell-lac, and the electric state of the latter examined in an electroscope.

Negative electricity was present upon the whole anterior surface (turned towards the charged sphere) of the thick brass disc, and it diminished from the centre to the circumference; on the cylindrical and posterior surfaces positive electricity was found, which increased from the anterior to the posterior edge, and decreased from the latter towards the centre of the posterior surface. A similar distribution was detected upon the thin brass disc; on the greater discs of silver and tinfoil the negative electricity upon the anterior surface no longer extended to the circumference; at the edge of this surface a narrow non-electric ring could be detected. Lastly, on the greatest disc, of copper, a broader ring of positive electricity was found on the anterior surface itself. The negative electricity upon this surface decreased from the centre to a distance of about $1\frac{1}{4}$ inch; at $1\frac{1}{2}$ inch there was no electricity; and at $1\frac{3}{4}$ inch there were decided appearances of positive electricity, which increased in density towards the circumference, and from there to the centre of the posterior surface again diminished. This superficial knowledge of the electric distribution upon the disc (which would suffer some change in presence of the neutral sphere), is sufficient to explain the action of the disc as intermediate plate. Whenever a disc is placed normally between two spheres, one of which is directly charged, two equal quantities of unlike kinds of electricity are elicited upon it, of which the quantity, unlike the electricity of the charged sphere, extends at all events over a less surface than the quantity like the electricity of the sphere does. When the disc is very thin, the two electric strata lie on two planes very close to one another, both of which are perpendicular to the line joining the centre of the spheres; hence the unlike-named stratum will act more strongly upon the neutral sphere than the like-named, because it will act at a less distance. For example, let us consider the copper disc placed between the two spheres. Then, acting upon the nearest point of the neutral sphere, we have the whole negative electricity upon a circular surface at a distance of one-third of an inch, and whose diameter is $2\frac{1}{2}$ inches, together with a smaller quantity of positive electricity upon a circular surface, whose distance from the point in question is only 0·302 inch, but whose diameter is 3·958 inches. At the same time the negative electricity is most dense at the centre where it is nearest to the neutral, whilst the positive electricity is most dense at the circumference where it is furthest from the neutral sphere. The negative electricity therefore exercises a greater influence upon the neutral sphere than the positive; and the induction of the charged sphere must appear diminished, as experiment plainly shows. When the thickness of the disc is great in comparison to its diameter, an opposite effect must ensue. By placing the thick brass disc between the spheres, the whole quantity of negative electricity is confined to a circular surface $\frac{2}{3}$rds of an inch in diameter, at a distance of 0·43 of an inch from the nearest point of the neutral sphere; an equal quantity of positive electricity is spread over the cylindrical surface, and a circular surface whose diameter is also $\frac{2}{3}$rds of an inch, but whose distance from the nearest point of the sphere is only 0·18 inch. This great difference between the distances manifestly causes the action of the positive electricity upon the posterior surface to surpass that of the negative electricity upon the anterior surface; sometimes, too, because the former is supported by the action of the whole positive electricity on the cylindrical surface. Here, therefore, the influence of the intermediate plate will be alike in kind to that of the charged sphere, and the induction of the latter will be apparently increased by introducing the plate between the two spheres. It is more difficult to predict the effect upon the observed induction produced by interposing a disc of less extreme dimensions in comparison to the distance between the two spheres, but after observation this effect may be easily explained by the distribution of the electricity upon the plate.

In general broad thin intermediate discs apparently weaken, and thick narrow discs apparently strengthen the inductive effect of a charged sphere upon a neutral one. Inasmuch as no doubt can exist that the action of conducting plates depends solely upon the induced electricities upon these plates, we must conclude that the action of non-conducting intermediate plates ought to be ascribed to the same cause. It is certain that a charged body induces both electricities upon the surfaces of the most perfectly insulating body; and the following simple and remarkable experiment will show that upon every one of the interposed plates hitherto used, these electricities make their appearance during the time of an observation. The disc of shell-lac 7·21 inches in diameter and 0·33 inch thick, or the disc of paraffine 4·63 inches in diameter and $\frac{1}{2}$ an inch thick, was moved quickly to and fro between the flame of a spirit-lamp and the knob of the conductor of an electric machine slightly charged positively; the anterior surface of the disc approached within one foot of the knob, and the posterior surface within one inch of the flame. By this means the anterior surface became so strongly negatively electric as to impart a strong and durable divergence to the gold leaves of the electroscope. Here, during the moment of passage, the positively charged knob of the conductor imparted negative electricity to the anterior surface of the plate, and positive to the posterior, and the negative electricity alone remained behind because the flame had withdrawn the positive. Consequently, in all the induction experiments above described, each insulating plate was undoubtedly provided with both electricities at the moment when it was interposed between the two discs or spheres; and to explain the different actions of these plates, we have only to assume that these electricities occupied different positions in space upon the thick and upon the thin plates. If, however, the action of the insulating plates depends upon the induced electricities on their surface, it follows that their interposition between the charged and neutral conductors is unessential, and the plates must also act when they occupy a side position in the neighbourhood of those conductors. By examining the induction upon a conducting disc, we may learn what effect this action must have. A copper disc, 4·03 inches in diameter, was so placed before a positively charged sphere, 0·87 inch in diameter, that the perpendicular let fall from the centre of the sphere upon the plane of the disc intersected the horizontal diameter of the latter at a point 0·77 of an inch from its centre, the distance between the nearest point of the sphere and the disc being 0·26 of an inch. The following are the results of an examination of the disc's horizontal diameter. The negative electricity induced upon the disc's anterior surface extended to a distance of 2·57 inches from that part of the circumference nearest to the sphere; the last 0·86 of an inch extending up to the most distant part of the circumference was occupied by positive electricity, as was the whole diameter of the posterior surface. If such a disc were placed near a positively charged ball during its inductive action upon a neutral one, with its plane parallel to the line joining the centres of the spheres, and its anterior circumference equidistant from both spheres, the negative electricity upon the disc would be nearer to the induced sphere than the positive. This negative electricity, therefore, would exercise a superior influence upon the sphere, and the induction of the originally charged sphere would appear weakened. The same effect would be produced if the conducting disc were placed between the spheres perpendicular to the line joining their centres, but without allowing its plane to be intersected by that line. This last experiment was unintentionally made in all the foregoing ones, in which interposed plates were employed. It was before mentioned, that the induced electricity appeared to be diminished at the moment when a plate was interposed between the two discs or spheres. With some of the non-conducting plates this diminution of the induced electricity was even greater than the subsequent increase of the same, when the plate was completely interposed. Imagine the examined copper disc placed near the two spheres with its surface parallel to the line joining their centres, and with its centre perpendicularly opposite to the centre of that line; the positive electricity on both surfaces of the disc will be much nearer to the induced sphere than the negative electricity, so that the positive electricity will exercise a superior influence on the neutral sphere, and the induction of the directly charged sphere will be apparently increased. I have made this experiment with the non-conducting plates before mentioned. The discs were placed in the described position at the side of two spheres, 0·60 inch distant from one another, and at a distance of 0·26 inch from their surfaces; in this position

I always found that the induced electricity of the neutral sphere was increased. Consequently we can apparently change the induction of a charged body upon a neutral one in any manner we please, without introducing into the space between both, which is occupied by atmospheric air, either a conducting or a non-conducting body.

The preceding research may be considered as demonstrating the following fact. The inductive influence exercised by a charged body upon a neutral one is not changed by bringing any interposed body, conducting or non-conducting, into its neighbourhood. An increase or decrease of the quantity of electricity upon an induced body may be produced by a conducting or a non-conducting interposed body; this increase or decrease arises from two new inductions proceeding from the interposed body itself, whose total action will depend upon the distribution of the electricities upon this body, and consequently also upon its substance, form and dimensions.

Accordingly, there is no essential difference between the actions of conducting and non-conducting bodies; but inasmuch as the distribution of electricity upon them depends also in extreme cases upon their substance, interposed bodies of the same form and magnitude will act differently. This difference of action was particularly perceptible when the interposed body was placed in the space between the charged and neutral bodies, and may be explained by the well-known peculiarities of conductors and insulators. In conducting bodies the distribution of the induced electricities depends solely upon the attracting and repelling forces of these electricities, and of the inducing electricity; in non-conducting bodies the distribution depends not only upon these forces, but also upon the resistance which the substance opposes to the motion of electricity. For example, in a conducting disc, when influenced by a positively charged sphere placed perpendicularly opposite to its centre, the most positively electric places lie in all cases at the circumference; in a non-conducting disc they may be at some distance therefrom. If, as interposed body between a charged and a neutral sphere, we employ a disc whose diameter may be conceived gradually to increase, then the induced positive electricity upon it will sooner cease to exercise a superior influence upon the neutral sphere when the disc is composed of a conducting, than when of a non-conducting substance. Accordingly, with a conducting disc, the action of the electricity, unlike that of the charged sphere, more easily manifests itself, therefore the inductive action of this sphere will be apparently diminished; with non-conducting discs, the electricity, like that of the sphere, acts more easily, consequently the inductive action of the sphere will be increased.

If the conduction on the surfaces of insulators did not make the comparison a very precarious one, it would be interesting, with a view to their classification, to compare the smallest dimensions at which discs of different non-conducting substances perceptibly diminish the influence of a certain body. Glass, mica, and shell-lac conduct on the surface better than in the interior, gutta-percha worse; and even when the greatest care is used in the treatment of the plates, this conduction cannot be retained sufficiently constant as to prevent the same plate at different times from exercising a considerably different action. The examination of a plate, by using it as an interposed plate, is much more delicate than its direct examination by means of an electroscope; and differences in superficial conduction can be detected thereby, which, by the latter method, remain completely hidden.

LVIII. *Researches into the Alum Manufacture. By* John Wilson, *Esq., Jun., Student in the Laboratory of St. Thomas's Hospital College, London*.*

THE history of alum-making is of considerable antiquity. Dioscorides and Pliny describe a substance, alumen, which however does not seem to have had much, if any, resemblance to the alum of the present day. Pliny chiefly speaks of two kinds of alumen, the liquid and the solid (liquidum spissumque); the liquid should be limpid and milk-white, and its characteristic is its striking a black colour with the juice of the pomegranate; and he informs us that it is very astringent, and is efficacious when mingled with honey as a cure for ulcers of the mouth. From this character of giving a black with pomegranate juice, it appears to have contained iron as one of its ingredients, which is not unlikely if the salt was native.

There was also a species of alumen called by the Greeks schistos, which split up into capillary filaments and liquefied when heated; this substance was prepared for use by heating it till it ceased to turn fluid, or in other words, by expelling its water of crystallization. This description agrees with the characters of some alum shales which become decomposed by exposure and split up, while hair salts shoot from the cavities. Dioscorides states that there are many kinds of alumen, but that most of its varieties may be found in Egypt. The best for medical purposes is very white, astringent, granular, and free from hard concretions, and splits up into capillary crystals.

The date of the discovery of our alum is unknown; previous to the middle of the fifteenth century, all the alum used in

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* Communicated by Dr. Robert Dundas Thomson, F.R.S.

Europe was imported from the East. In the fourteenth century an alum work was in operation at Rocca in Syria. There were also several near Constantinople. In 1459, Bartholomæus Pernix or Perdix, a merchant of Genoa who had frequently been to Rocca, discovered the mineral of alum in the island of Ischia, which is situated at the entrance of the Gulf of Naples. About this time Johannes de Castro, having visited the alum-works at Constantinople, discovered the alum stone at Tolfa. It is said he was led to this observation by the similarity of the trees at Tolfa to those which he had seen growing upon the hills of Turkey; and experiment showed the two minerals to be equally adapted for preparing alum.

At Viterbium the attempts made by the Genoese had great success, and the manufacture increased greatly, especially as Pope Pius the Second strictly forbade the use of oriental alum. But the art did not remain within the bounds of Italy, in the sixteenth century a work having been set up at Alcmaron near Carthagena. In Germany, in the year 1544, several existed; and in the time of Agricola there was an alum-work at Commoton in Bohemia. Bergman says that at this time (16th century) the art had scarcely penetrated into Sweden. The first alum-work in that country was founded in 1637 at Andrarum, where the alum shale is of great depth, being upwards of 360 feet.

In Queen Elizabeth's reign alum-making was introduced into England by Sir Thomas Chaloner of Gisborough in Yorkshire. It is said that he observed the trees in his neighbourhood tinged with a peculiar colour, and suspecting that this was owing to some aluminous mineral, caused a search to be made, the result of which was the discovery of alum shale. It was towards the end of the sixteenth century that his works were commenced; and in a short time so great was their success, that sixteen works of the same kind were soon established in the neighbourhood.

Alum was first made in Scotland in the year 1766, by Messrs. Lightbody and Co. at Hurlet, but their process was found to be unprofitable, and abandoned in 1768–69; and it was not till the year 1797, when works were erected at Hurlet by the late Charles Macintosh of Campsie, and John Wilson of Hurlet, and their partners, that the manufacture of alum was successfully established. In consequence of the success of the Hurlet works, the same Company in the year 1808 commenced a larger work at Campsie, where a shale similar to that at Hurlet is found in abundance.

In the year 1820, the alum made in England and Scotland amounted to from six to seven thousand tons annually; namely four thousand five hundred to five thousand five hundred in England, and fifteen hundred tons in Scotland. At present about seventeen thousand tons are made annually, of which twelve thousand tons are produced in England, and the rest in Scotland. The processes by which alum is produced from the shale or other material are different, as the composition of these varies.

In the alum-works which were carried on formerly at Smyrna and Solfatara, and in other places where the ore contains a sufficient amount of alkali to form alum without an additional quantity, the process is simple. The rock containing the ready-formed alum is dissolved in a lead vessel and the solution crystallized.

At Tolfa, where the alum-stone contains an excess of alumina, the ore is calcined in order to render insoluble the excess of hydrate of alumina; to effect this, the stone is broken up into small pieces, which are carefully calcined till sulphurous fumes begin to be given off. Whenever this happens, the calcined stone is removed to stone cisterns, where it is moistened with water; in a few days the calcined stones crack and swell, becoming like slacked quicklime. After being thus moistened and exposed for some months, they are thrown into leaden boilers and boiled with water for about twenty-four hours, the water as it evaporates being replaced by the mother-liquor of a former crystallization. When the ore has been thus boiled for a sufficient length of time the fire is extinguished, and the sediment having subsided, the clear liquor is run into wooden reservoirs, where it crystallizes; in these it remains about fourteen days, the mother-liquor being used as described for dissolving the calcined ore.

The alum obtained by this process is what is termed Roman alum, and is considered the purest to be found in commerce. It frequently occurs in cubical crystals, and generally is coated with a reddish-brown powder resembling ochre, but which is said to contain no iron, being put on by the manufacturers to give their alum the peculiar appearance which characterizes the Italian salt.

The alum made in this country is not obtained in this way, as neither potash nor sulphuric acid exist ready-formed in sufficient quantity in any of our shales, so that the processes adopted are necessarily more complicated.

The alum ore is an aluminous shale containing carbonaceous matter, and diffused iron pyrites. By the calcination of this shale the sulphur is oxidized, and unites with the alumina, iron, and other bases, forming sulphates. By digesting the calcined ore in water, and adding a salt of potash or ammonia to the liquor, alum is obtained.

At Hurlet the method formerly pursued was to leave the alum shale exposed to the air in the mine, which has always a temperature of from 60° to 70° (Fahrenheit) for several years. By this treatment the ore becomes gradually covered with a white efflorescence of sulphate of iron and alumina, having an appearance like asbestus. The following are analyses of this hair salt, which I made at Dr. R. D. Thomson's laboratory at St. Thomas's Hospital under his superintendence; and here I take the opportunity of thanking him for the kind assistance he afforded me in these and subsequent analyses. A trace of potash is present.

	1.	2.	3.	4.	5.	Average.	Atoms.
Alumina	9·64	9·36	9·04	7·16	6·32	8·30	1·20
Protoxide of iron..	18·00	18·36	17·92	18·68	19·27	18·44	4·09
Lime	·24						
Sulphuric acid ...	34·36	33·60	35·20	33·92	34·93	34·40	6·88
Water...............	36·80	37·16	37·04	40·32	40·77	38·41	34·10
	99·04	98·48	99·20	100·08	101·29	99·55	

These analyses show this salt to be composed of 1 atom tersulphate of alumina united to 4 atoms protosulphate of iron and 34 atoms of water, or $Al^2 O^3\, 3SO^3 + 4FeO\, SO^3 + 34HO$.

The decomposed ore was then lixiviated, and the black residue thrown upon the hill, as it was termed, or mass of previously lixiviated shale. This hill, by further oxidation under the influence of the atmosphere, afforded another product of alum. The lixivium was then evaporated, and the copperas allowed to crystallize out. To the solution from these crystals a salt of potash or ammonia was added, when alum was deposited. These crystals, after being washed, were roached in the ordinary manner.

As the demand for alum increased, this process was found too tedious; the fresh or slightly decomposed ore was therefore taken and calcined, thus accelerating the natural process of decomposition. To ascertain the composition of the fresh shale I analysed it, and the results are as follows:—

Specific gravity 2·54.

25 grs. fused with carbonate of soda gave—

		Per cent.
Silica	12·07	48·28
Alumina . . .	6·74	26·96
Peroxide of iron	2·89	11·56 = 10·41 FeO = 8·09 Fe
Carbonate of lime	1·07	2·38 CaO.

100 grs. dried till it ceased to lose weight at 300° F. lost 2·02 per cent. water.

25 grs. deflagrated with pure nitre and carbonate of soda, and the filtered solution acidulated and precipitated by Ba Cl, gave—

		Per cent.	
BaO SO^3	10·76 . . .	5·93	sulphur.
...	10·57 . . .	5·87	...
...	11·02 . . .	6·08	...
Average		5·94	...

To determine carbon and hydrogen, the dried shale was burned with chromate of lead, with the proper precautions.

		Per cent.	
16·65 grs. gave	2·42 CO^2 . . .	3·96	carbon.
	1·60 HO . . .	1·06	hydrogen.
14·51 grs. gave	2·13 CO^2 . . .	4·00	carbon.
	1·41 HO . . .	1·08	hydrogen.

	Carbon.	Hydrogen.
Average . . .	3·98	1·07

The total iron present in the shale is 8·09 per cent., and 5·94 per cent. of sulphur require 5·19 of iron to form bisulphide with it. Deducting 5·19 from 8·09, we have 2·90 per cent. iron, or 3·72 per cent. protoxide uncombined with sulphur. The nitrogen was obtained by combustion with soda lime, and precipitation by bichloride of platinum, 17·83 grs. giving—

Platinum ·76= ·62 per cent. nitrogen.

I also found potash in small quantity. 200 grs. of shale gave ·78 KCl = ·24 per cent. KO.

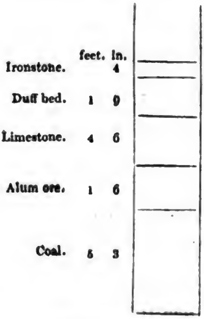
The complete analysis will stand thus:—

Silica	48·28
Alumina	26·96
Protoxide of iron	3·72
Lime	2·38
Magnesia	trace
Potash	·24
Bisulphide of iron	11·13
Water	2·02
Carbon	3·98
Hydrogen	1·07
Nitrogen	·62
	100·4

This shale lies in seams which vary in thickness from a few inches to about three feet and a half. It underlies the limestone, and is deposited above the coal. It is very hard as it comes from the mine, but soon decomposes when exposed to the air. Sometimes small particles of the pyrites are visible when it is fractured, but in general this is not the case. There is another seam of alum ore which is called by the miners the Duff bed, lying above the limestone. This mineral, although not so suitable for making alum as the other, is still of great value for that purpose. It has almost the same composition as the ordinary alum ore.

The sketch of a portion of a section of a Hurlet pit shows the relative positions of the minerals mentioned. After the coal has been dug out, leaving pillars at regular intervals to support the roof, the alum shale is taken out, and then the lime-blowers, as they are called, proceed to remove the limestone by blasting it with gunpowder.

I find this limestone to have a specific gravity of 2·71, and to be composed of—



Carbonate of lime	88·814
Carbonate of magnesia	1·182
Carbonate of iron	4·764
Bisulphide of iron	·395
Silica	3·412
Carbonaceous matter	trace
Water	1·075
	99·642

In calcining the ore for alum-making, a flue of brick is constructed upon the ground, and upon this brushwood and coals are laid which are ignited, and alum shale placed above them. When these have become ignited, another layer of ore is deposited upon it, while at the same time waste alum shale is spread over it to prevent the combustion from being too rapid. The presence of carbonaceous matter and sulphur assists the combustion, and renders a further addition of fuel unnecessary. In from twelve to eighteen months the calcination is ended, and the ridges being broken up, the ore is wheeled to the steeps to undergo the process of lixiviation. The composition of the calcined shale is as follows:—

		1.		2.		Average.	
Insoluble.	Silica	35·28		38·31		36·79	
	Alumina	14·48		14·21		14·34	
	Peroxide of iron	21·40		21·67		21·53	
	Lime	3·26		2·56		2·91	
	Magnesia	trace	74·42		76·75		75·57
Soluble.	Sulphuric acid	8·88		8·86		8·87	
	Alumina	2·92		1·24		2·08	
	Peroxide of iron	1·80		1·56		1·68	
	Lime	·87		1·52		1·19	
	Water	10·72	25·19	11·16	24·34	10·94	24·76
			99·61		101·09		100·33

The ore is thrown into square cisterns of stone having wooden bottoms; mother-liquor is run upon it, and the liquor percolates slowly through the bottom of the steep into cellars beneath. After three or four washes with mother-liquor, pure water is employed. From four to eight washes in all are drawn off; but this varies with the quality of the calcined ore. When the proper number of washes has been taken off, the insoluble residue is removed and used for covering the ridges of calcining ore. This exhausted material consists of—

Silica	40·39	Insoluble	90·52
Alumina	17·25		
Peroxide of iron	30·66		
Lime	2·22		
Sulphuric acid	1·92	Soluble	10·20
Peroxide of iron and alumina	·92		
Lime	1·00		
Water	6·36		
			100·72

The composition of the first four washes from the steeps I have found to be as follows in the gallon:—

	No. I. Sp. gr. 1188.	No. II. Sp. gr. 1129.	No. III. Sp. gr. 1100.	No. IV. Sp. gr. 1090.
Sulphuric acid	933·05	595·05	456·90	415·35
Peroxide of iron	346·75	118·68	112·42	53·31
Alumina	194·59	149·99	59·42	91·50
Protoxide of iron	73·18	126·92	85·11	138·45
Chlorine	27·44	42·85	38·54	36·60
Magnesia	14·13	14·83	16·06	21·48
Lime				
Potash	75·67	60·98	28·90	27·05
Grs. per gallon	1664·81	1109·30	797·35	783·74

The potash found in these analyses is derived from the mother-liquor which is thrown upon the raw shale previous to calcination; and as muriate of potash is the salt employed, it accounts also for the chlorine. The shale from which these liquors were obtained must have been different from what I examined, as magnesia and no lime existed in these liquors. The liquors are pumped up from the cellars into wooden reservoirs, whence they are allowed to flow into the evaporating boilers. These last are about 60 feet long, 7 feet broad, and from 4 to 5 deep. They are arched over and are made of fire-bricks. At one end is the furnace, and at the other the chimney. The flame and hot air pass over the liquor, and the draught carries off the steam as it rises. During the evaporation, a brownish or buff-coloured substance falls in considerable quantity, which is commonly called a subsulphate of alumina; but a specimen which I analysed I found to contain upwards of 50 per cent. sulphuric acid, 25 of peroxide of iron, and about 3 or 4 of alumina. When the liquor has attained the strength 65° to 70° of Twaddel's hydrometer (1325 to 1350 spec. grav.), the boiler is opened and the liquor run into the alum-coolers, which are made of stone. The proper quantity of muriate of potash or sulphate of ammonia is added either in solution or dry; if dry, the mixture is agitated by wooden poles to cause the salt to dissolve and be equally diffused. The coolers are left at rest for about a week, at the end of which time the mother-liquors are made to flow into a tank for the purpose, and the first crystals of alum collected and washed with cold water. These crystals are of a brown colour, being contaminated by iron, and contain,—

Sulphuric acid	34·33
Alumina	6·44
Peroxide of iron	4·52
Potash	2·33
Ammonia	2·81
Chlorine	·36
Water	49·31
	100

The first alum is dissolved in mother-liquor from the second crystallization by means of steam. This operation is performed in a cistern lined with brick about 9 feet deep, to the bottom of which a steam-pipe passes. When the alum has dissolved, the steam is shut off and the deposit allowed to subside. The liquor is run into stone coolers, where the second alum crystallizes. This I found was composed of—

Sulphuric acid	36·08
Alumina	9·60
Peroxide of iron	·88
Ammonia	3·83
Potash	·73
Chlorine	trace
Water	48·88
	100

The second alum is dissolved for the last time in vessels of lead into which steam is passed: these vessels are called roaching pans. The solution is closely covered over and allowed to settle, and is then run into the roaching-tubs, where it deposits itself in crystals, shooting towards the centre of the tubs. The tubs are simply large wooden casks lined with lead: when the alum has attained a sufficient degree of hardness, the staves are removed, a hole is made near the bottom of the cask to allow the mother-liquor to run off into the cellar for that purpose, and the alum broken up into lumps or ground, as the case may be. The mother-liquor is again evaporated, as in the first process of alum making, when it deposits a crop of first alum. The pure alum contained—

Sulphuric acid . .	35·57
Alumina	11·40
Peroxide of iron . .	trace
Ammonia	3·42
Potash	·83
Water	48·78
	100·

In preparing potash alum, the muriate is the salt generally used; hence small quantities of chlorine are often to be found in the alum. By the process I have described, three parts of shale produce one part of marketable alum.

In 1836, and for several subsequent years, alum was made at Hurlet in the following way. Alum shale containing the smallest amount of sulphur was calcined in a reverberatory furnace; it was then ground and sifted, the powder was thrown into lead cisterns and hot dilute sulphuric acid run upon it. A strong action occurred, and the greater part of the shale dissolved. The solution was evaporated and the alkali applied in the usual way; the sulphate of ammonia was at that time made by distilling gas liquor into the mother-liquor of the alum; oxide and sulphide of iron were thrown down, and the solution consisted of sulphate of ammonia.

In Sweden, where wood is the common fuel, and consequently very expensive, the alum shale, which is very carbonaceous, is used for performing the calcination, an operation occupying about a month or six weeks. The late Dr. Thomas Thomson, in his 'Travels in Sweden,' tells us that so great is the quantity of combustible matter in the Swedish alum shale, that he saw it employed for burning limestone. In the same way the shale is used for the evaporation of the alum liquors, the shale being calcined and the evaporation performed simultaneously. Wood is also used in Sweden for the calcination of the alum shale, as appears from a letter to the late Charles Mackintosh, Esq., published in his memoir, in which the process followed at Haensetter is thus described:—"Their process of alum making is nearly as follows: the schistus, which has something the appearance of our undecomposed ore, lies under a stratum of limestone about 2 feet from the surface of the earth, and upon being quarried is built into kilns in the following manner: first, a layer of fir or rather pine-leaves or branches intermixed with birch; second, a layer of schistus which has been used as fuel in the evaporating or roasting boilers; third, a layer of raw schistus, and so on in rotation as before. After burning, the schistus is put into steeps, which are nearly the same as ours at Hurlet; water is poured upon it, the same water being poured upon five steeps before it is fit for use. The liquor is then run into settling cisterns, after remaining some time in which it is next put into the first boilers for evaporation. These boilers, which are of lead, are constructed in the following manner:—Six of them, 4 feet diameter each, are built in one storey, in two rows, with three fires under them; they are made of lead, open above and arched in the bottom, which is of cast iron. From these boilers the liquor is run into wooden coolers which are very small, 3 feet long and 2½ feet broad, with three wooden partitions in each. The crystallized alum is afterwards washed and roached exactly in the same manner as it is with us. Their roaching-casks are about twice as large as ours, and require to stand for about three weeks. From this it appears that the small quantity of alkali formed from burning the schistus with wood is sufficient for their purpose."

The Kinekulle slate in Dr. Thomson's museum at St. Thomas's Hospital, I found to have the following composition:—

	Specific gravity, 2·42.
Silica	52·28
Alumina	16·64
Protoxide of iron	6·96
Iron }	3·61
Sulphur }	4·13
Potash	7·98
Lime	1·53
Magnesia	1·10
Water	1·40
Carbonaceous matter	4·37
	100

The calcined Swedish ore generally has a brown colour, but when it assumes a red appearance it is found to be not so productive in alum.

At Whitby the calcining heaps are from 90 to 100 feet in height. The stratum of aluminous shale is abut 29 miles wide. Its specific gravity is about 2·40, and the upper part of the rock is always the most abundant in sulphur. It contains a large amount of bituminous matter, which renders great care necessary to regulate the combustion, as it is apt to burn too rapidly and drive off portions of sulphur. About one ton of alum is produced from 130 tons of calcined shale.

In some alum ores considerable quantities of magnesia exist. When this is the case, Epsom salts are produced in the process of calcination, and being more soluble than alum, remain in solution after it has crystallized. To obtain the Epsom salts, the liquor is evaporated sometimes by waste heat from the alum boilers when the salt crystallizes, often in considerable quantities; one work in Yorkshire producing annually about 800 tons of this salt.

In certain specimens of Hurlet shale magnesia is found, sometimes as much as 1½ or 2 per cent., but there was no appreciable amount in what I examined. Formerly an efflorescence of nearly pure sulphate of magnesia was to be found in the mines in considerable abundance, but of late years its occurrence has been less frequent; it consists of long, transparent, brittle needles, often more than 2 inches long. A specimen from Hurlet in Dr. Thomson's museum at St. Thomas's Hospital, which he kindly permitted me to analyse, I found consisted of,—

	Per cent.	Atoms.	Epsom salt.	
Sulphuric acid . .	33·17	1	32·52	1
Magnesia	15·42	1·07	16·25	1
Protoxide of iron .	1·28			
Alumina	·23			
Water	49·91	7·2	51·23	7
	100·01		100·	

The formula is $(MgO\ FeO)\ SO^3, 7HO$.

A specimen from Peru resembling this in appearance, also in Dr. Thomson's museum, contained—

		Atoms.
Sulphuric acid	35·96	7·19
Magnesia	2·54	1
Alumina	15·88	2·2
Water	45·80	39·6
	100·18	

$$MgO\ SO^3 + 2Al^2\ O^3\ 5SO^3 + 40HO.$$

Alum is also made from clay. For this purpose the purest clay is calcined in a reverberatory furnace at a moderate heat.

This process has the effect of peroxidizing the iron, and rendering the clay more porous. The heat must be regulated carefully, otherwise the clay undergoes a commencement of fusion, in which condition it is nearly insoluble in acid. About 45 per cent. of its weight of sulphuric acid, of specific gravity 1·454, heated by the waste heat of the furnace to about 160° F., is run upon it and digested with it for some months, at the end of which time the mass is lixiviated, concentrated, and the proper quantity of alkali added: the alum precipitates and is purified in the ordinary way.

Concentrated alum or tersulphate of alumina is an article introduced of late years into commerce, for which there is a considerable demand. The objection to its more general use is, that its composition is not always the same. It is prepared by dissolving clay in sulphuric acid, and adding ferrocyanide of sodium to precipitate any iron which the acid has dissolved, as prussian blue. The clear liquid is evaporated till it becomes solid on cooling. The prussian blue is boiled with caustic soda to reconvert it into prussiate of soda, thus avoiding any loss in this respect.

The two analyses given were made upon a sample of concentrated alum sent by Messrs. H. Patinson and Co., of Newcastle-on-Tyne.

The sulphuric acid was precipitated by chloride of barium. 25 grains gave—

I. Sulphate of barytes 27·89 = SO^3 per cent. 38·46
II. ... 27·61 = ... 38·08

25 grains gently heated and then strongly ignited left,—

I. Alumina . . 4·84 = alumina per cent. 19·36
II. ... 4·55 = ... 18·20

The sulphuric acid per cent. and alumina being deducted, leave,—

I. Water per cent. . . . . 42·18
II. ... . 43·72

	I.	II.	Average.	
Alumina	19·36	18·20	18·78	1
Sulphuric acid .	38·46	38·08	38·27	2·6
Water	42·18	43·72	42·95	13·2
	100	100	100	

$$Al^2O^3 + 2\tfrac{1}{2}\,SO^3 + 13\,HO$$

or

$$2Al^2O^3 + 5SO^3 + 26HO\,?$$

Fig. 8.

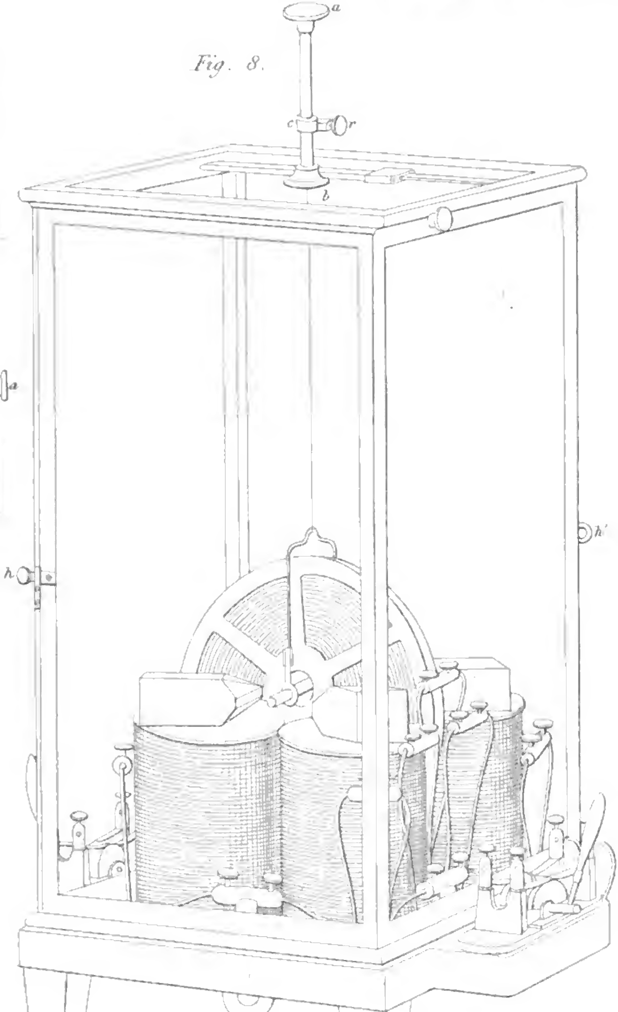
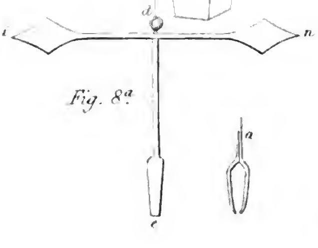


Fig. 8a.



J. Basire.

LIX. *The Polymagnet.* *By* Professor Tyndall, *F.R.S. &c.*

[With a Plate.]

THE polymagnet was devised for the purpose of exhibiting before a class of pupils as many as possible of the phænomena of electro-magnetism and diamagnetism.

The instrument consists of an arrangement of two horseshoe electro-magnets, a helix of covered copper wire disposed between them, and suitable means of suspension.

A section of one of the electro-magnets and its surrounding spirals is given, fig. 1, Plate IV. *ab, cd* are two cylindrical cores of soft iron, which are united by a cross-piece of the same material, *ef.* Through the cross-piece pass the strong screws *g* and *h* into the cores, and by them the ends *b* and *d* of the cores, which are accurately planed so as to ensure perfect contact with the cross-piece, are attached to the latter. The diameter of the cores is 1·125 inch, and their distance apart, from centre to centre, 4·85 inches; the cross-piece *ef* is drawn in proportion.

Round each core is a helix of copper wire, overspun with cotton, which was afterwards saturated with shell-lac. In winding the helix, two lengths of wire, one covered with red cotton and the other with green, were laid side by side and coiled as a single wire. The diameter of the wire is 0·1 of an inch, and the weight of it which surrounds each limb of the magnet is 12 lbs. For all four limbs, therefore, a weight of 48 lbs. is made use of.

The second electro-magnet is in every respect similar to the one just described.

Fig. 2 is a front view of a helix of covered copper wire, intended to be placed between the two electro-magnets; it has an internal diameter, *ab,* of 1 inch; an external diameter, *cd,* of 8 inches, and measures along its axis 1·15 inch. The diameter of its wire is 0·065 of an inch, and its weight is 6 lbs.; it is wound so as to form a double coil, as in the case of the electro-magnets. The radial strips, and central and surrounding ring seen in the figure, are of brass, and hold the coils of the helix compactly together.

Fig. 3 represents a stout slab of mahogany which supports the apparatus. *ab, cd* are hollows cut in the slab to receive the cross-pieces of the two electro-magnets; from *e* to *f* the slab is cut quite through, the cross-pieces merely resting on the portions between *f* and *b, f* and *d,* &c. The small apertures at *x, x'* show where the screws enter which attach the cross-piece to the slab of wood. The central aperture at *g* shows where the pin *g* of the helix, fig. 2, enters, the helix thus occupying the central portion of the board. Right and left are two projections for the reception of two current reversers, which will be described immediately. The apertures 1, 2, 3, 4 are for the reception of pins projecting from a glass case intended to cover the whole apparatus.

When the magnets and central helix are fixed in their places and looked down upon, their appearance is that represented in fig. 4; at *a* and *c* the tops of the cores are seen, the moveable masses of soft iron which belong to them being removed; the two ends of the other electro-magnet bear two such masses, each formed from a parallelopiped 4·5 inches long, 2 inches wide, and 1·25 inch high, having one end bevelled off so as to render it pointed, the other end being suffered to remain flat. The distance between those moveable masses may be varied, or the body to be examined may be suspended either between *surfaces* or *points*, according to the nature of the experiment. The projections of the current reversers upon the horizontal plane are seen right and left.

Simplicity and efficiency being the objects aimed at, a current reverser was devised which fulfils these conditions. A front view of the instrument is given in fig. 5, and its horizontal projection in fig. 6. Q is the section of a quadrant of wood, which is capable of being turned by the handle HP; *ab* is the section of a strip of brass laid on the periphery of the quadrant; *cd* is a shorter strip similarly laid on; between *b* and *c* is a gap, formed of the wood of the quadrant itself, or of a piece of ivory or glass inlaid; *s* and *s'* are two brass springs*, which are shown in the figure to rest upon the strips of brass *ab* and *cd*; M, M' are two clamps secured to the wooden pillars O and O' by screws S which pass up through the latter. The plan below corresponds to the section above. From *b*, fig. 6, the strip of brass crosses to *c'*, and from *c* to *b'*, both being insulated from each other at R. Supposing, then, the two clamps M and L to be connected with the two poles of a galvanic battery, the current entering at M would flow along the spring *s* to *b*, thence to *c'*, and finally along the spring *s'* to the clamp L': in like manner the current entering at L would attain the clamp M'. In this position of things the handle of the instrument leans to the left, as in fig. 5. If the current is to be interrupted, this is secured by setting the handle vertical; for when the handle is in this position, the spring *s'* rests upon the non-conducting surface *bc*, and the circuit is broken. If it be desired to send the current direct from L to L', and from M to M', this is accomplished by causing the handle to lean to the right; when this is done, both the springs *s*, *s'* rest upon the self-same strip of brass *ab*, and there is direct metallic communication between L and L', and between M and M'. This reverser has been tested practically, and found very convenient. It is very similar to an instrument devised by Prof. Reusch, but simpler and more easily constructed.

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* Copper, I think, would be better than brass.

Fig. 7 is a plan of the top of the glass case which surrounds the polymagnet. At C a brass tube is cemented to the glass, which is here perforated, and through the tube a rod passes furnished at its lower extremity with a hook, to which is attached a suspending fibre. *ab* is a horizontal brass cylinder capable of being turned on its axis by the milled head at *a*, and thus coiling a suspending fibre around a groove marked at *d*: the cylinder is also capable of sliding right and left, so that the body suspended from the fibre may be moved laterally, and the amount of motion measured on the graduated bar above*. *a'b'* is another horizontal suspension rod, in every respect similar to the former.

The whole instrument, surrounded by its glass case, is shown in perspective in fig. 8. The magnets are visible, with the moveable masses of soft iron resting upon them; in the centre is seen the helix sketched in fig. 2, and within the helix a bismuth bar supported by several fibres of unspun silk attached to the central rod which passes through the top of the glass case. The manner of suspension of the bismuth will be understood from the drawing, certain practical artifices which suggest themselves when the drawing is attentively inspected, being introduced to facilitate the placing of the axis of the bar along the axis of the surrounding helix. The current reversers are seen *without* the case; two opposite sides of the latter can be opened by the handles *h* and *h'*, so that free and easy access to the interior is always secured.

Experiments to be made with the Polymagnet.

1. All the experiments that are usually made with an upright electro-magnet.

2. The various portions of the instrument may with great facility be lifted separately out of the case. One of the electro-magnets being thus removed, a rope can be passed through a ring *r*, introduced for this purpose into the cross-piece, fig. 1: adjacent to the screws *g* and *h* two plates of brass are seen; these are attached to the brass reels of the helices, and by passing a pin through the holes shown in the figure, the helices are prevented from falling when the magnet is turned upside down. Attaching the rope to a hook in the ceiling, or to a strong frame made for the purpose, experiments on the lifting power of the magnet may be made.

3. While one of the magnets is suspended as last described, the other, which is of exactly the same size, can be brought up

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* This arrangement, though very convenient for private research, is not necessary for lecture experiments.

against it, the free ends of the four cores being thus in contact. The same current being sent through both magnets, we have the mutual attraction of two electro-magnets instead of the attraction of an electro-magnet for a mass of soft iron, as supposed in the last experiment. The arrangement just described is indeed precisely that devised by M. Pouillet in the construction of a powerful electro-magnet for the faculty of sciences at Paris. To the cross-piece of the second magnet a ring is also attached, from which weights can be suspended.

4. The cross-pieces may be removed by withdrawing the screws *g* and *h*, and the spirals may be made use of singly with their corresponding bar-magnets. As two wires surround each coil, one of them may be used to exhibit the induced currents developed by the other. The phænomena of the extra-current may also be studied, and the remarkable effect produced by connecting the two ends of one of the wires, or the spark of the extra-current in the other, may be exhibited.

The milled head *a*, fig. 7, can be screwed off, and the rod *ac* pushed downwards into the case; the helix in the centre can also be readily lifted out of its position and removed from the glass case—we will suppose this done. The two electro-magnets alone are now within the case, and the view is uninterrupted, which would not be the case if the helix had been permitted to remain.

The fibre hanging from the groove *d*, fig. 7, can be so arranged that any substance attached to it shall hang between the moveable masses of soft iron which surmount the electro-magnets, and the same arrangement can be made for the fibre suspended from the groove d'.

But a body suspended between the moveable masses of soft iron would be hidden by these masses from the audience, and hence to render the motions of the body visible the following expedient was adopted :—fig. 8*a* represents a thin index of ivory about 4 inches long, and shaped as in the figure; from the centre depends the stem *dc*, which is terminated by a tongs-shaped arrangement which can clasp the body to be submitted to experiment; to the right of the index a section of the little ivory pliers, by a plane passing through the stem at right angles to *i n*, is given; the stem is slit up to *a*, so as to allow of the pliers being opened to receive the body to be examined, which they then clasp in virtue of the elasticity of the ivory. The stem *de* is of such a length, that when the body is in the centre of the space between the poles, the index *i n* is seen above them; and, as the index follows all the motions of the body underneath, these motions are recognized by all who see the index.

5. If an ordinary magnetic bar, sufficiently feeble, be suspended

between one pair of poles, and an ordinary diamagnetic bar between the other pair, on sending the same current round both magnets, the index of the former sets itself parallel to the polar line, while the index of the latter sets itself perpendicular to the polar line, and thus the phænomena of magnetism and diamagnetism address the eye simultaneously.

6. In the same way, if a normal* magnetic bar be suspended between one pair of poles, and an abnormal magnetic bar between the other, the antithesis of their deportment may be made manifest. The same antithesis is exhibited when we compare a normal diamagnetic bar with an abnormal one.

7. And when between one pair of poles is suspended a normal magnetic bar, and between the other pair an abnormal diamagnetic one, the apparent identity of deportment of both bars is rendered evident at once. The same identity is shown when we compare the abnormal magnetic bar with the normal diamagnetic one.

8. Causing the points to face each other, instead of the flat ends of the poles, and observing the directions given in the paper spoken of, the curious phænomena of rotation on raising or lowering the body from between the points, first observed by M. Plücker, and explained in the paper referred to, may be exhibited.

9. To show that a bar of bismuth, suspended within a helix and acted upon by magnets, presents phænomena exactly analogous to those of soft iron, only always in opposite directions, let the flat helix be replaced between the two electro-magnets. The bar of bismuth used in experiments with the instrument now described is 6 inches long and 0·4 of an inch in diameter. Suspended so as to swing freely within the helix, its ends lie between the moveable masses of iron which rest upon the electro-magnetic cores. Four poles are thus brought simultaneously to bear upon the bar of bismuth, and its action is thereby rendered both prompt and energetic. The two poles to the right of the bar must both be of the same name, and the two to the left of the bar of the opposite quality. If those to the right be both north, those to the left must be both south, and *vice versâ*. On sending a current from 10 or 15 cells round the helix, and exciting the magnets by a battery of 4 or 5 cells, the current reversers place the deflections of the bar entirely under the experimenter's control. By changing the direction of the current in the helix

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* For the explanation of normal and abnormal bars see a memoir "On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet," to be published in the forthcoming part of the Philosophical Transactions. The paper will, in due time, be transferred to the pages of the Philosophical Magazine.

by means of its reverser, a change of deflection is produced; the same is effected if the polarity of the magnets be changed by the reverser which belongs to them. For a full description of all these phænomena I must refer the reader to the paper on the nature of the diamagnetic force, already mentioned.

10. To those acquainted with what has been done of late years in diamagnetism, numerous other experiments will suggest themselves. The antithesis of two isomorphous crystals, one magnetic and the other diamagnetic, the general phænomena of magnecrystallic action, and the analogous effects produced by pressure, may all be exhibited.

By placing one of the helices of the electro-magnet upon the other, a coil of double length is obtained, and two such coils may be formed from the four which we have described. For the additional expense of the iron merely, a single electro-magnet, far more powerful than either of the others, because excited by twice the quantity of coil, may be obtained.

I think it would be an improvement if the suspensions were independent of the glass case, so as to permit of the entire removal of the latter. The best way of showing the deflection of the bismuth bar within the central helix to a large audience, is to attach a long, light index to the bar itself, and permit this index to enter a French shade which will protect it sufficiently from currents of air. With this arrangement the motions are strikingly evident, and may be seen by hundreds at once. The instrument above described was constructed by Mr. Becker, of Newman Street, and its cost is about twenty-four pounds. It was not my intention originally to have so much wire round the electro-magnets; and the effects may also be made manifest with a smaller central coil. I have no doubt that with 8 lbs. of wire round each limb of the electro-magnets, and a central coil weighing 4 lbs., the experiments might be exhibited to a large audience with perfect distinctness. A sensible diminution of cost would of course accompany this diminution of material and labour.

LX. *Note on the Aberration of Light.*

By the Rev. Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

THE effect of the aberration of light on the apparent place of any body of the solar system, is taken account of by the astronomical calculator on the following principle:—The apparent direction of the body, as affected by aberration, is its real direction at the instant the light by which it is seen started from it. This rule is enunciated by Dr. Young in vol. ii. of his

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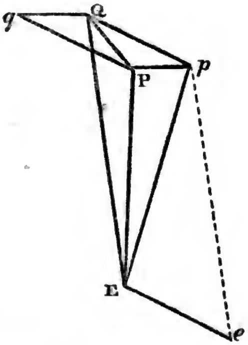
[*] Communicated by the Author.

'Natural Philosophy' (p. 294), but is not accompanied by proof, and I have not met with any proof of it elsewhere. In order to prove the rule completely, it is necessary first to explain the cause to which aberration is due. In vol. iii. (1852) of the Philosophical Magazine (p. 53), I have proposed an explanation of aberration which may be briefly stated as follows. In every determination of the apparent place of a celestial object by an optical instrument, the light from the object passes actually or virtually through *two* points rigidly connected with the instrument. One of these is necessarily the optical centre of the object-glass, and the other is a point in the field of view of the telescope, which we may suppose to be coincident with the intersection of two wires. Let, at the instant of an observation, O be the position of the first point, and W that of the other. Then the instrument gives by its graduation and by a note of time, the direction of the line joining O and W referred to certain fixed directions. If the transmission of light were instantaneous, this direction would coincide with the path of the light, supposing the path to be rectilinear in the short space OW. But if light occupies a sensible time in passing between the two points, in that time the position of the optical centre of the object-glass has changed, by reason of the earth's motion, from O' to O; so that O and W being simultaneous positions of the *direction-points*, the instrument gives OW for the path of the light, the real path being $O'W$. It is found by observation that the difference between these two directions is the whole amount of the aberration of a star. This general explanation of aberration being admitted, the following is the proof of the rule above mentioned.



Let e and p be simultaneous positions of the observer and the object, and while light is travelling from p to the observer, let the observer be carried by the earth's motion from e to E. Then, leaving out of consideration any causes, such as atmospheric refraction, which may affect the course of the ray before it enters the telescope, the object is seen by the observer when at E by a ray which has described the path pE. Draw pQ parallel and equal to eE, and join EQ. Thus by reason of aberration as above explained, the ***instrumental*** direction of the object is EQ. In the time that light takes to pass from p to E, let the object move from p to P, so that EP is its real direction at the instant of observation. Consequently the ∠QEP is the difference between the instrumental and the real directions. Draw Qq parallel and equal to pP, and complete the parallelogram QqPp. Now the earth being conceived to be at rest, the apparent angular motion of the body in the interval of the transit of light from p to E is the angle subtended at E by the resultant QP of the motion Qq of the body and the motion Qp equal and opposite to that of the earth, viz. the ∠ QEP. Hence EQ, the instrumental direction of the body at the time of observation, coincides with its actual direction at a time preceding the observation by the interval occupied by the passage of the light from the body to the observer. This result establishes the rule it was proposed to demonstrate.

It follows from this theory, that the instrumental direction of a terrestrial object, whose motion in space is parallel and equal to that of the observer, coincides with the actual direction; and that the instrumental direction of a fixed object, as the sun, and of an object having a fixed direction, as a star, differs from the actual direction by the whole amount of instrumental aberration. The theory also shows that the value of the constant of aberration may be in some degree affected by the retardation which the light undergoes in that part of its course between the direction-points O and W, which lies within the substance of the object-glass, or passes through other glasses.

Cambridge Observatory,
May 11, 1855.

LXI. *On the Periodical Variations of Terrestrial Magnetism. By* A. Secchi, *Director of the Observatory of the Collegio Romano**.

[Concluded from vol. viii. p. 399.]

Part II. § 2. *On the Variations of the other Magnetic Elements.*

THE two magnetic elements, the variations of which remain to be determined, are the intensity and the inclination. But as their direct determination is very difficult, an indirect method is used, which consists in finding the variations of the components of the total force resolved along three coordinate axes taken with reference to the horizon.

The position of the dipping-needle shows the direction of the resultant of all the magnetic forces of the globe; and the direct method of determining the intensity of this force would be to observe the oscillations of the dipping-needle itself. This method has, however, several inconveniences.

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* Communicated by Mr. Archibald Smith.

The first is, that as the time of an oscillation depends on the intensity of the magnetism of the needle as well as of that of the earth, a change in the one might be attributed to the other, and there are no means of determining in which it has taken place. Another disadvantage is, that as such experiments require a considerable amount of time and no ordinary skill, they may give a great deal of trouble to the observer, and yet leave it uncertain whether the magnetism of the needle and of the earth have remained constant in the interval occupied by the observation.

For these reasons, philosophers, following the steps of Gauss, have abandoned the direct measurement of the total force, and for the same reasons that of the variations of the inclination; and have adopted the following method of investigation.

Conceive the total force resolved along three rectangular coordinate axes; one horizontal and parallel to the magnetic meridian, another horizontal and at right angles to the magnetic meridian, the third vertical. For each of these directions there is a particular apparatus for determining the magnitude and the variations of the components; and from these, the direction of the resultant being known, the value and variation of the total force and inclination are easily deduced. The instrument which is used to measure the horizontal component is called the unifilar magnetometer. Its office is to give the absolute value of the horizontal force. But the description of the method pursued would lead us too far from our subject*. It may suffice to say, that these experiments likewise can only be made at distant epochs, and therefore what we have already remarked of vibrations of the dipping-needle is equally applicable to them, viz. that they cannot be used to determine the horary variations, or those of short duration. These last are usually deduced from the variations in the position of a bar placed at right angles to the magnetic meridian. The apparatus used for this is called the *bifilar magnetometer*, a name descriptive of its construction.

It consists of a bar-magnet suspended by two parallel threads in a state of equal tension, which, were a non-magnetic body suspended from them, would remain in the plane of the meridian; but a magnetized bar being suspended, their line of suspension undergoes a torsion until the bar takes a direction at right angles to the magnetic meridian. The force of torsion exerted by the threads counteracts the magnetism of the earth, and the needle is rendered in a manner astatic, so as to change its position with the slightest variation of the terrestrial force. Observations with this instrument are made, with the same ease as those of the declination, by means of a mirror or collimator attached to the magnet, and observed by a telescope with a graduated scale. One precaution must be taken; as the force of the magnet varies in intensity with the temperature, it is necessary to take account of this, so as to correct the observations for its influence by known methods of sufficient accuracy.

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* For the theory of this observation see Gauss's memoir *Intensitas*, &c.; for the practice see Weber's memoirs published in Taylor's Scientific Memoirs, and the instructions in the introduction to the *Annuaire Magnétique et Météor.* of Russia. The method is shortly this: the declination needle is made to oscillate, first loaded with certain weights, and then without them. By this means the magnetic moment of the needle, multiplied by the intensity of the terrestrial force, is obtained; next, a bar magnet is placed at different distances from the needle, and from the deviations which it produces, the value of the force divided by the magnetic moment is deduced, and from these the magnetic force may be easily obtained.

The vertical force is measured by means of the *balance magnetometer,* invented by Dr. Lloyd. This consists of a bar supported horizontally on knife-edges, which rest upon two agate planes, exactly like the beam of an ordinary balance. This is arranged in the plane perpendicular to the magnetic meridian, and is adjusted by means of small weights, so as to undergo a sensible inclination with the slightest change of the vertical magnetic force. Its motions are observed by means of microscopes attached to the stand of the instrument, and collimating on the intersection of spiders' webs fixed at the ends of the bar, or by observing, as before, the image of a scale reflected by a mirror attached to the bar.

When the variations of the horizontal and vertical force are obtained, the variations of the inclination are deduced from them by known formulæ. The balance magnetometer, as well as the bifilar, is subject to variations depending on the temperature, and therefore it is necessary to determine the coefficients of the correction. It does not, however, appear that its results are in all cases as certain or as exact as those of the bifilar.

A. *Horizontal Force.*

The component which we are now considering is that which is obtained from the bifilar magnetometer, arranged at right angles to the magnetic meridian. The variations may be expressed in the following manner.

General Laws.—The bifilar magnetometer is subject to a horary variation of a double period, diurnal and semi-diurnal; in the semi-diurnal period the magnitude of the variation depends on the geographical latitude, and is zero at the equator; the phase depends on the angle which the sun makes with the magnetic meridian.

We will demonstrate this by steps. Beginning with the stations of middle latitude, there is this simple law; the curve of the bifilar magnetometer is similar to that of the declinometer, but with a retardation of three hours.

Explanation.—A glance at Colonel Sabine's figures in the second volume of the Hobarton Observations, plate 1, p. 5, for the declination, and at plate 4, p. 43, for the component of the bifilar magnetometer, will be sufficient to show, that while the minimum of the declination occurs between 20^h and 21^h, and the maximum at about 2^h, the minimum of the bifilar magnetometer occurs about 23^h, and the maximum between 4^h and 5^h. See also the figures in which this celebrated author makes the comparison between Hobarton and Toronto in the first volume of the Hobarton Observations. At p. 34, plate 1, the curves of the declination are shown; and at p. 54, plate 2, figs. 1 and 2, that of the horizontal force; the perfect agreement of the curves will be seen, (though the scales of the abscissæ are different) and the same retardation between Hobarton and Toronto which has been already remarked in the declinations.

Next let us consider the peculiarities of the equatorial observatories. At St. Helena a singular law holds in the horizontal force. It has a single simple period, and the only indication of a secondary period is that the axis of the abscissæ is not divided by the curve into equal parts, but the diurnal part is less extended than the nocturnal. See the St. Helena Observations, p. 30, plate 4, fig. 3.

Here, then, the semi-diurnal period vanishes entirely or nearly so, and cannot be compared with that of the declination.

But we shall presently see, in the theoretical law, the true explanation of this singular fact, and it will be one of the principal proofs of the theory which we are about to expound. In the variation of this component two very decided periods are evident, the diurnal and the annual. The diurnal maximum occurs between 23^h and 0^h, and the minimum at 9^h or 10^h; but in May and June (the winter months) it occurs later, viz. at 11^h. The form of the curve shows a rapid increase and an equally rapid decrease. The annual variation is likewise remarkable for its simplicity, being a perfect curve of sines. See p. 28, plate 2, fig. 2. We shall see further on, that at Bombay (lat. 18° 53′ N.) the phases approximate to those of St. Helena, except that the secondary periods are more sensible, though not much more so. From the two extreme cases which we have here considered, we may infer what would happen at an intermediate station, like the Cape of Good Hope; that there would be a diurnal and semi-diurnal period, but the latter considerably less developed than at Hobarton and Toronto. Observation confirms this; and fig. 1, p. 40 of the Cape of Good Hope volume shows it at a glance.

The period, then, of the horizontal force is the resultant of two periods, the one diurnal, the other semi-diurnal; and the value of the semi-diurnal period is a minimum at the equator, and increases with the geographical latitude. The epochs of the changes depend in this case, too, on the hours at which the sun passes the magnetic meridian, and are somewhat advanced in the summer of the hemisphere and retarded in the winter.

If from the middle latitudes we ascend to the Pole, we shall find that the curve of the bifilar appears to be in advance of that of the declinometer: this peculiarity, which seems to complicate the law which we have enunciated, depends entirely on the coefficients with which the diurnal and semi-diurnal period are alternately affected according to the latitude.

The following are the laws relative to the annual variation of the diurnal means. So far as regards the monthly means, we have already remarked that at St. Helena there is evidently an annual period depending on the sun's declination; and in order to display the effects of the solar declination in the other observatories, it would be necessary to repeat the analysis made for the magnetic declination. But unfortunately, really perfect observations are as yet few, and hardly sufficient for the seasons. As far as regards Hobarton, the march appears to be not very unlike that of the declination. Thus during the summer, the diurnal variation has its greatest extension, and becomes successively less in spring and autumn, and is at its minimum in the winter.

So far as regards the absolute value of this component, it is greater than the annual mean in summer, less in spring and autumn, and a minimum in the winter. It would not be difficult to show, from the nature of this curve, that here, too, is to be found the superposition of the two periods depending on the solar declination and on the horary angle, which are added together in summer, and of which one is subtracted from the other in winter. At Toronto the effects are similar in the respective seasons. At St. Helena the absolute maximum of the horizontal force occurs in the months of February, March and April, and the minimum in August and September.

In these variations it is not easy to separate that which is caused by the temperature from that which is strictly the magnetic period,—partly because the temperature exercises an influence on the bars, and if the variations are not accurately corrected there is a danger of error,—and partly, because as the temperature affects the force of all magnets, it may affect that of the earth also.

Besides, as the variation of this component depends both on that of the inclination and on that of the total force, it is not easy by means of the horizontal observations alone to determine to which cause each phase must be attributed. A few observations, and those subject to some uncertainty, tend to show that at St. Helena the annual variation of the inclination is small, so that we must consider the variations of the horizontal force as depending entirely on those of the total force.

At Makerstoun the values of the horizontal component have their maxima at the solstices, and their minima a little after the equinoxes*.

In general in this, as in the declination, the months of April and August are marked by the greatest diurnal excursions; this is attributed by Mr. Broun to the extraordinary perturbations, but it probably depends on some other cause. But for the principal details on the epochs of the maxima and minima, we must refer to the original works, this memoir being already too long.

B. *Vertical Component.*

This is given by the balance magnetometer arranged at right angles to the magnetic meridian. It follows laws analogous to those of the horizontal force. At St. Helena the curve has a simple period, but with this difference from the curve shown by the bifilar, that the curve of the horizontal force is nearly a curve of cosines (reckoning from noon), and that of the vertical force a curve of sines. But the rudiments of a secondary period are seen in a slight undulation, which it makes at about 10^h, as also in the intervals of the intersection of the curve with the axis being greater from midnight to noon than from noon to midnight. The maximum occurs between 5^h and 6^h, and the minimum at 20^h. Its amount is smaller from October to March, during which period it is retarded, and greater from April to September, when it is in advance.

At the Cape of Good Hope the march of the vertical force is so similar to that of the declination, that it is susceptible of the same analyses and gives the same results. In these curves the diurnal period always predominates, though somewhat modified in parts. See Sabine, Obs. Cape, p. 40, pl. 5. fig. 2.

At Toronto and at Hobarton the usual antagonism displays itself in this component. The double period is developed as much as in the declination, and the hours of minima and maxima are almost identical with those of the declination. The double period displays itself at Hobarton in a somewhat singular manner, producing a maximum towards seven in the evening.

But to follow out all these irregularities would be very tedious; we will therefore abstain, referring the reader to the works already quoted. We will only add, that the explanation of many anomalies is to be found in the following remarks, viz. that as the origin of the periods which are superposed depends on the different hours at which the sun passes the magnetic meridian, the semi-diurnal period has necessarily various positions with regard to the diurnal; and in this manner various inflections occur which it would be difficult to account for in any other way. For instance, the variations of the inclination given at the Cape of Good Hope, p. 44, pl. 6, can easily be decomposed by an experienced eye into the usual principal periods, diurnal and semi-diurnal, which gain alternately on each other with varying parameters in the different months.

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* Makerstoun Observations, 1846, p. 32, where also it is mentioned in a note that this is confirmed at Munich by Dr. Lamont.

We may therefore finish this discussion with the following general conclusion: that "the horizontal component, as well as the vertical, may be decomposed into a diurnal and semi-diurnal period, which depend on the declination of the sun and on the geographical latitude."

C. *Inclination and Total Force.*

Given the laws of the variation of the two preceding components, that of the resultant or total force may easily be deduced; and the absolute magnetic inclination being known, the variation of the inclination may also be deduced from the variations of the horizontal and vertical force.

General Law.—" The phases of the inclination are analogous to those of the declination, but three hours earlier."

Explanation.—If the maximum of the declination is at 2^h, the maximum of the inclination would be at 23^h or thereabouts. This may be seen in the Makerstoun curves, &c., and also in the Hobarton curves, the explanation of which we will give presently.

As for the other peculiarities, it will be sufficient to remark, without analysing each case in detail, that in general the maxima of the horizontal force coincide with the minima of the inclination.

Colonel Sabine calls attention to the analogy that exists between the variation of the inclination at Hobarton and Toronto, places which are almost antipodal. In these localities the variation of the inclination in both its periods is the same at almost precisely the same hours, with this difference only, that at Hobarton the south pole (the lower) is to be considered, and at Toronto the north (the lower).

The total force at Toronto is subject to two periods, viz. the following:—

Principal maximum at . . .	5^h
Principal minimum from . .	15^h to 16^h
Secondary maximum from . .	18^h to 20^h
Secondary minimum from . .	22^h to 23^h

According to this distinguished writer, an analogous double period is wanting at Hobarton, and the total force has a simple progression with the minimum at 20^h or 21^h, and the maximum between 5^h and 6^h, the intermediate march being continued without interruption. But on carefully examining the curves themselves, given by him at p. 68, pl. 3, it will be seen that the simple period exists only in appearance, and that in certain months the secondary period is very obvious, and rudiments of it exist in all, though to a very small extent. This difference is certainly owing to the great difference of latitude and magnetic force between the two places.

There are not yet a sufficiency of published observations at St. Helena to determine this law; but a copious series of observations at the Cape show a period in the variations of the total force almost complementary to that of the declination. The similarity of the two kinds of curves, which generally differ about three hours in their phases, renders a more complete analysis unnecessary.

D. *Complex Period of the Needle.*

The process which we have described for determining the motions of the needle consists in a series of decompositions of the forces, rendered necessary from the mode in which the magnetic bars are supported. The laws of the variations of the components being determined, we may deduce from them what would be the motion of a needle, not on an axis, but suspended by a *single point,* which would be its centre of gravity, and free to obey every magnetic variation in whatever direction it took place. To give an idea of the combined motions which the needle makes in a complete oscillation, we may refer to two figures in particular in Sabine's Plate, Hobarton Observations, vol. i. pl. 3, the first of which belongs to December at Hobarton, the other to June at the same place. The principle upon which these figures are traced is the following:—The point where the two axes intersect represents the mean diurnal position of the needle in declination as well as in inclination. Along the horizontal axis a distance is taken representing the variation of the declination for a given hour, and from the point so obtained, an ordinate is erected representing on the same scale the variation of the inclination for that hour. Thus are obtained the figures to which we have referred.

In Colonel Sabine's Plates, he has given the curves for each month of the year, and they are all extremely instructive, but the two to which we have particularly referred, the march of the curve in the two extreme months of the year, is well shown. It may be seen from these, and still better from the whole series, that the oscillation of the needle has always a double period, diurnal and nocturnal, but their respective lengths vary with the season. The diurnal period, considerable in summer, is contracted in winter, and the nocturnal period, short and hardly discernible in summer, is greatly developed in winter; in this will be seen the fact, elsewhere noticed, that the absolute nocturnal minimum is greater than the diurnal minimum, and hence we see the cause of the error of those who consider that there is a single period in winter. But two things are particularly to be observed in these curves. 1st. The nocturnal loop is always in diametical opposition to the point of noon; from this it appears that the phases succeed each other near the lower meridian with the same march as near the upper.

2nd. That the magnitude of each loop in the opposite seasons, diurnal as well as nocturnal, is in a constant proportion to the one diametrically opposed to it, viz. between $\frac{1}{7}$th and $\frac{1}{6}$th; thus, for example, the loop of the diurnal curve for December becoming the nocturnal loop in June, is diminished to about $\frac{1}{6}$th. In like manner the diurnal loop of June, when it becomes the nocturnal loop in December, is diminished to about $\frac{1}{6}$th. This constant proportion, which is observed in all the months, must not be overlooked; and physically considered, it must depend on the manner in which the influence of the solar magnetism operates across the earth.

3rd. The appearance of these curves is that which would arise from the superposition of two circular spirals with different moduli, the one having a simple, the other a double period. The curves which are seen in Wheatstone's undulation machine, when two spirals are superposed, the one half the length of the other (in which case the projection of the resultant at right angles to the axis of the spirals forms a kind of *c*), are evidently of the same kind as the present.

Mr. Broun has given analogous curves for Makerstoun, and a glance at these, as in the case of Hobarton, will show the same law, though somewhat more complicated from having grouped too many months together, and from the higher latitude and more frequent disturbances.

Among the points to be remarked in these curves is the following:—"Tracing in them the direction of the magnetic meridian (that is to say, noting the hour at which the sun passes it), it is seen that the greatest velocity of the needle occurs when the sun passes through this plane, and that the centre of the nocturnal loop is to be found in the same line, or very near to it, and that the movements of the needle in inclination are complementary (but with a distance of 3^h) to those of the declination."

From these facts we conclude that "a perfectly free needle would describe during the day a species of double spiral produced by a compound circular motion having two periods, the one diurnal, and the other semi-diurnal; or of two periods, the one while the sun is above the horizon, and the other while it is below; the excursions of which are in the proportion that the diurnal arcs bear to the nocturnal, and have for principal axis the local magnetic meridian."

Lastly, there is a fundamental characteristic of all the principal elementary periods, which consists in the *maximum and minimum being about* 6 *hours distant from one another.*

E. *Total Force.*

Colonel Sabine has investigated whether the maxima and minima of the total force vary during the year; from the discussion of the observations at Hobarton and Toronto he has arrived at the conclusion that the total force has its maximum in the months of December and January in both hemispheres, although these correspond to opposite seasons. Such a law corresponds too nearly with the change of the distance of the earth from the sun to admit of our doubting that it depends upon it. We have then that "the disturbing force of the sun increases as its distance from us diminishes, and does not depend on the temperature of the seasons."

The exact determinations of this force hitherto obtained are too few to enable us to get out the rigorous expression of this law, that is, whether it is inversely as the square of the distance; but the fact appears to be established, and will perhaps be brought out by the discussion of other observations, particularly if care be taken to eliminate from them the periodical changes which depend on the seasons, and the secular changes.

PART III.

In this part we will briefly discuss the different hypotheses which have been proposed to account for the diurnal magnetic period, and we will take this opportunity of referring to the extraordinary variations.

Justice must be done to the enlightened spirit of modern physicists, who, intent on the study of facts and their laws, care little to construct hypotheses; from this it arises, that whatever has been proposed has been rather by way of conjecture than with any real endeavour to establish a theory. We too, in the same spirit, and merely for the purpose if possible of combining facts, have supposed the sun to act as a great magnet. The explanations hitherto proposed may be reduced either to thermo-electric currents induced by the sun in the different strata of the earth, or to the electricity developed in the meteorological changes of which the sun is the principal cause. A single reflection seems to exclude these from being *principal* causes of the magnetic diurnal period. The characteristic fact, as we have already noticed, is that the magnetic elements have a double period, diurnal and nocturnal.

Now temperature and the other causes suggested have a simple period, with greater or less modifications it is true, but not so constantly repeated when the sun is below the horizon as the magnetic period is in all climates and in all seasons. This appears to us a law peculiarly characteristic of magnetism, which shows it to be essentially distinct in cause, and to have a separate origin from meteorological phænomena; in the same way that the semi-diurnal lunar period in the tides is a proof of a special action of our satellite on the waters of the ocean, which is universal gravitation. And just as the fact that the tide is more or less retarded after the moon passes the meridian, is not a sufficient objection to destroy belief in this cause, so some irregularity of a like nature observed in the magnetic period will not be sufficient to disprove the reality of the magnetic action, if we believe the proofs adduced to be otherwise sufficient. We remarked at the beginning of this work, that Arago's observations, well discussed, ought to lead to the same conclusions as those of other observers; and now that we have, at last, before us an abstract of these observations, it appears that we were not mistaken, and that they afford a striking proof of the existence of the double period *.

What has led some to consider the magnetic period as a simple one, has been, seeing that in certain seasons the extreme minima occur at night. The error arises from not distinguishing the absolute from the relative maxima; but these are the true characteristics of the phænomenon, and ought to be looked upon as decisive in the matter.

To this proof in support of the solar magnetic theory, may be added another, already noticed by Col. Sabine, and worked out by us in § 1 of Part II. of this Memoir, viz. the opposite action of the sun according to its declination, the inversion occurring exactly at the epoch of the equinoxes; and here another difference will be seen between the effects of thermical and meteorological causes, and the magnetic effect of the sun. The former do not reach their extremes for a considerable time after the corresponding astronomical phases, while the latter have an almost exact coincidence with them.

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* We received the fourth volume of Arago's works, the first of his scientific works, after the present treatise was finished: we see with pleasure that our theories are not contradicted by the observations or the deductions of this celebrated astronomer and physicist, so that, without our knowing it, our opinions are almost coincident with his. We shall see presently how his observations furnish an additional proof of a decennial period in the magnetic changes which we expected to find there. For the double period, see pp. 499, 540.

We do not pretend, however, that there are not considerable difficulties in the way of this hypothesis; and although it explains very well certain very singular facts,—as, for example, the interval of six hours between the diurnal maxima and minima, a fact the explanation of which has never, as far as I am aware, been even attempted on any other hypothesis, and which yet is so marked in all the magnetic variations in the mean latitudes; also the singular exception which it suffers at the equator, becoming simple for the horizontal and for the vertical components, and various other points,—yet we must confess that there are some irregularities which our formulæ do not explain. Of this nature is the fact, that at St. Helena, and generally under the equator, the period for the declination of the needle appears to be rather eight hours than twelve, so that it presents sometimes three maxima. Without repeating here what we have said elsewhere in general terms, viz. that these periods may find their explanation in those terms of the formulæ which we have neglected *, we may say that this fact may simply depend on the configuration and nature of the ground near to the places of observation. Thus, for example, at St. Helena, an island situated in the midst of the Atlantic, and entirely volcanic, the distribution of magnetism must be very different from what it is at a place in the interior of a continent; and we know, in fact, that the isogonal lines rapidly change their direction in passing from seas to continents, and this explanation may also apply to equatorial stations near the coast. We know too how much the vicinity of magnetic bodies may influence the diurnal variation of the needle †. This explanation seems to be confirmed by the fact that the diurnal curves derived from the declination at St. Helena resemble much more closely those of other countries than the annual diurnal mean. In fact, the latter is exclusively due to the horary angle, and therefore more strictly dependent on the distribution of the earth's magnetism round the place of

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* We have in every case worked out these formulæ completely, and there have resulted terms which, developed in series, give sines and cosines of triple arcs; from this we may deduce the explanation of a period of the third of a day. But as this development may be made in every periodical function, it does not give a legitimate proof of the truth of the explanation, until we have demonstrated theoretically that these terms may have coefficients of considerable magnitude, a point which I have not yet investigated.

† See the volume of Arago already cited, where he relates the experiments of Barlow on this subject, p. 492.

observation. It may also be said that the small maxima in the morning and evening, which are in truth for the most part only indicated, are only a portion of the regular period cut short midway by the discontinuity introduced by the passage of the sun from above to below the horizon, as we have elsewhere remarked in regard to the periods observed towards evening in high latitudes *. For these reasons we have urged that a complete explanation of the phænomenon depends on the law of the distribution of magnetism on the globe.

It may not be useless to state here what is habitually observed at Bombay, that being a place situated to the north of the equator, and in a latitude not very dissimilar in amount to St. Helena, the lat. being 18° 53′ 30″ N. From the observations made at this place and reduced by Mr. Montriou, an oscillation results analogous to that of other countries; having an eastern maximum a little before 8^h, and a minimum between noon and 1 P.M. Besides which, there are two other small oscillations, one near sunrise, the other near sunset; it is evident that this is the nocturnal period interrupted by the interposition of the earth. During the night the needle has a very small oscillation. The horizontal force has a simple period, but with disturbances which indicate the commencement of a secondary period; the vertical force shows a tendency to a like period. When the observations have been continued for a greater number of years, more certain results will be obtained. See Obs. Magn. and Meteor. at the Obs. of Bombay for the year 1847, part 1. p. 493, and Plate 1. The Editor of the Bombay Observations then concludes:—"The presence of the sun seems to produce great magnetic variations in the day-time, and it is otherwise manifest that it is not on account of the heat only of that body; for if this was the case, the curves of the temperature would be similar to the magnetic curves. Besides, the presence of the sun begins to be felt two hours before sunrise, and lasts almost as long after sunset, so that the solar magnetic influence appears to be quite independent of the temperature of the place."

But we are very far from denying that meteorological causes may often affect the needle; we know that every meteorological change is accompanied by a change more or less marked in the vapour of the atmosphere, and therefore by a development of electricity. But when the needle is usually seen to complete its regular oscillation tranquilly in the midst of the most violent storms, and during tempests loaded with electricity, with tremendous thunder and lightning, it may well be asked, *What are the conditions under which electricity must develope itself in*

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* This appears to us to be the true explanation of the phænomenon.

order to affect the needle? That such an action does exist, however, appears to be proved by the fact, that in our climates the needle performs its oscillation with the greatest regularity during calm and serene days, and that on change of weather this regularity is invariably disturbed. We have, in proof of this, a year's observations at Rome: and it would be well to discuss magnetic observations more from the meteorological point of view than has hitherto been done. From the few observations which we have made, it appears that light and passing overcloudings have more effect on the needle than tempests themselves. We have already remarked elsewhere, that at Rome the perturbations exhibit themselves in that particular state of the atmosphere in which there are slightly phosphorescent clouds having at night the appearance of the rudiments of the aurora borealis. This fact was observed by us a second time on the evening of the 27th of July. We were making some observations on stars in the meridian, when towards half-past nine we were interrupted by a slight overclouding coming from the north; a very rare occurrence, since generally with us the sky begins to overcast in the south-west. While we were waiting for it to clear, the cloud appeared slightly luminous at the edges, so that there seemed to be a diffusion of the milky way in unwonted parts of the heavens. Soon after this it cleared, and the observations were continued; but almost immediately the same overclouding recommenced with the same luminous appearance. I then remembered the fact observed before, that a similar state of the atmosphere had been accompanied by magnetic perturbations, and on going to look at the magnetometer I found it more than 20 divisions (about $7\frac{1}{2}'$) out of its usual position, and the regular observation made at 9·35 had been marked by the observer as being an extraordinary one for that hour. This was the more striking from the needle having performed its diurnal oscillation with the greatest regularity during the whole of the preceding season. This was, without doubt, a phænomenon of the kind which accompanies the aurora borealis. But it may be asked, was the condensation of vapours the cause, or the effect, of the perturbation? It is generally considered to be most probable that the perturbation is the effect; but is this certain? M. De la Rive has proposed, in his Memoir on the Aurora Borealis [*], a theory which accounts with some felicity for the effects of atmospheric electricity on the needle; but it may be doubted whether this cause is sufficient to explain all the facts to which the author would apply it. An accurate study of the laws to which the extraordinary perturbations of the needle are subject, combined with the study of the aurora borealis, can alone throw light on this question.

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[*] *Bibl. Univ. Archiv. des Sciences Naturelles*, vol. xxiv. p. 337.

All that we know with certainty on this subject is, like so much besides, due to Colonel Sabine. He has collected the principal results at which he has arrived from the discussion of the Hobarton and Toronto Observations in a memoir inserted in the Philosophical Transactions (March 1852), from which we will give a short extract, as well to complete the exposition of the laws of the magnetic changes, as to obtain some light for guidance in future researches.

A comparison of the Toronto and Hobarton observations establishes, that even the extraordinary perturbations, though occurring at all hours of the day, yet when taken in a mass, have a regular period, which depends on the local time, and have opposite directions in the opposite hemispheres; so that the perturbations which cause an easterly deviation at Toronto, cause a westerly deviation at Hobarton, in conformity with the complete magnetic antagonism at the two stations. This fact is elicited without difficulty from the coincidence of the perturbations observed at the two places in the same day, with a difference in local time corresponding to their difference in longitude. The general result is, that easterly perturbations at Toronto and westerly perturbations at Hobarton, have their minimum in number and magnitude during the day and their maximum during the night. This maximum occurs at Hobarton between 10^h and 11^h, and at Toronto at 9^h. This difference in time, as we have already remarked, occurs in all the other magnetic changes. The minimum occurs at Hobarton between 5 and 6 A.M., and at Toronto between 2 and 3 P.M. The easterly perturbations at Hobarton and the westerly at Toronto, have a distinct period. Their maximum at Toronto is at 5 A.M., and at Hobarton at 6 A.M.; the minimum at Toronto is between 9 and 10 P.M., and at Hobarton at 10 P.M. Taking the perturbations in mass, and laying down the curve representing their mean effect on the curve of the diurnal oscillation of the needle, the following law is elicited:—"The morning perturbations tend to diminish the ordinary excursion of the local period, and the evening ones to augment it."

This law may be enunciated in another way. "The pole which is turned to the sun is by the mean effect of the perturbations moved towards the east from 5 A.M. to 5 P.M. About 6 A.M. and 6 P.M. it passes zero, and the rest of the day is moved to the west. The maximum movement in the morning is at about 7, and in the evening at about 9. In both places a secondary minimum towards the west is observed at noon." In other respects the curves are tolerably regular and of the usual form; but at Hobarton the principal maximum and minimum are less marked than at Toronto; the march of the two curves is in direct opposition at the two stations. These conclusions agree with those of Mr. Broun at Makerstoun, as may be seen in the results for 1846, p. 87, plate 1. The following are the results with respect to the frequency and magnitude of the perturbations in the different months of the year:—"The mean value of a perturbation is a maximum in the equinoctial months, less in the winter months, and a minimum in the summer months." At Hobarton the difference between the summer and the equinoctial months is scarcely perceptible. The proportions of the frequency and of the magnitude of the perturbations in each month, relatively to the sum of those observed in a year, come out a minimum in the winter months, a maximum in the equinoctial months, and intermediate in the summer months. These conclusions, however, may be somewhat varied by the use of different systems of reduction, depending chiefly on the definition of an extraordinary perturbation. This is not the case in the laws of the perturbations taken with reference to the day, because in that case all the methods of reduction bring out the same result. Thus the same result is seen in the curves given by Mr. Broun, whose method of reduction is different from that used by Colonel Sabine.

But a most singular fact, which has been discovered in these researches, is the start which the mean annual values of the perturbations take between the years 1845 and 1846, when they are almost doubled. This being a fact of the greatest importance, Colonel Sabine has endeavoured to place it beyond doubt by the best possible proofs. The following is the table given by him (p. 115):—

Year.	Ratios of the number of perturbations.	Ratios of the aggregate values.
1843	0·60	0·52
1844	0·78	0·78
1845	0·72	0·65
1846	1·20	1·15
1847	1·28	1·42
1848	1·43	1·52

The circumstance of the last three years having ratios almost double of the first three, appears not to be accidental, particularly when we observe that in the two observatories, which are almost antipodal, the same fact appears, and that during all the six years the same instruments were employed. Besides, in the same years the observed diurnal excursions of the declination, of the inclination, and of the total force, have sensibly increased beyond the limit of any probable error. Lastly, the same fact, in regard to the diurnal excursion of the declination, results from the observations of Dr. Lamont at Munich in Bavaria. Further observations may throw much light on this subject. For the present we can only say with Colonel Sabine, that so general a change in the march of all the magnetic elements demands a proportionate cause; and that as these do not arise from the ordinary effects of the climate, which in these years have exhibited no extraordinary change, it is necessary to seek some other cause.

Colonel Sabine also points out the singular coincidence between the years of the maximum and minimum of these magnetic changes and those of the maximum and minimum number of the solar spots, observed by Schwabe in these years. This number was a minimum in 1833 and 1843, and a maximum in 1828, 1837 and 1848. From these and from other observations, Wolf has deduced a decennial period in the changes of these spots; and it remains to investigate whether a like march can be traced in the older magnetic observations. The publication of Arago's observations has come opportunely for this comparison. From the table at p. 500–501, vol. i. of his Scientific Works, the declination-needle appears to have had a minimum excursion in 1823 and 1824. Before that it was greater, and having reached this minimum, it increased continuously until it arrived at a maximum in 1829. These epochs correspond with those derived from the period observed in the solar spots, which was a maximum in 1828 and a minimum in 1823. From the Göttingen observations, we find a maximum in the excursions of the declination-needle in 1836–37. This maximum also coincides with a maximum of the solar spots in Schwabe's table*. Hence Colonel Sabine thinks it not impossible that changes in the solar atmosphere may extend their influence to the earth in the form of magnetic action.

The truth is, that to consider the whole complexity of magnetic perturbations as a mere meteorological effect, appears to be assigning to them a cause not adequate to the effect. The fact mentioned above, that the maxima of the perturbations at Hobarton succeed each other with the same retardation as the other magnetic phases, is one which cannot be explained either by the retardation of the effect of temperatures, or by the condensation of vapour. We cannot conceive how these should account for the general retardation of one hour. It is then a purely magnetic fact, the explanation of which depends on that of the physical cause of solar and terrestrial magnetism. The same may be said of the greater perturbations at the epochs of the equinoxes, which certainly bear no relation to the state of the atmosphere or to the solar heat. Colonel Sabine makes the acute observation, that the coincidence of the solar spots with the maximum of the perturbations demands a cosmical cause, depending on that body*.

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* Results of the Magnetic Observations made by Gauss at Göttingen, *vide* Taylor's Scientific Memoirs, vol. ii. part 1. art. 2. p. 57; see Humboldt's Cosmos, vol. iii. p. 292, Sabine's translation.

We may be permitted to refer here to the hypothesis of Mairan on the solar atmosphere, and on its relation to the zodiacal light and the aurora borealis, and therefore to the magnetic perturbations. We are far from admitting the theory as proved, since it appears impossible to admit that the solar atmosphere extends so far as half the radius of the orbit of Mercury†, whence it is rather to be inferred that the zodiacal light depends on a nebulous ring circulating round the sun between Venus and the Earth.

But whatever hypothesis be adopted, there are various coincidences which may be deserving of regard. Mairan had, even in his day, remarked the greater frequency of the aurora borealis at the equinoxes‡, the epochs at which the zodiacal light is most visible. Neither had the relation between the greater frequency of the aurora borealis and the epochs of the greater solar spots escaped him, a relation already remarked by Cassini§.

The paucity of observations at that time permitted suggestions to be made which have been since proved to be groundless; but in general such coincidences are worthy of consideration. Modern observations of the eclipses of the sun, of the protuberances and of the corona, as also of the spots, of the temperature of various parts of the disc, as well as photographic impressions, have placed beyond doubt the existence of the solar atmosphere even beyond the zodiacal light‖. In reading Mairan, one cannot help seeing the serious difficulty which he finds in explaining why the auroras have their maximum at the equinoxes, and not at the epochs at which the earth passes through the nodes of the solar atmosphere; but do we truly know the place of the nodes of the zodiacal light? He assumes that they are the same as those of the solar equator, but this is not proved; and if the zodiacal light constitutes a ring, it might well be otherwise. On the magnetic hypothesis, the greater frequency of the aurora at the epochs of the equinoxes would have relation to the position of the poles of the sun with reference to the earth, these poles being in fact more directed towards the earth at the equinoxes, and being more or less oblique to it at other times.

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* It may be said, that as the spots are apertures in the atmosphere of the sun, they give freer passage to the magnetic action of the solar nucleus.

† See Mairan's Works, p. 199, in which for the months of the year the following numbers are given relative to the frequency of the aurora, beginning with January,—21, 27, 22, 12, 1, 5, 7, 9, 34, 70, 26, 15. *Mairan sur l'Aurore Boreale, suite des Mémoires de l'Ac. des Sciences,* 1731.

‡ *Vide* Humboldt, vol. iii. p. 413, Sabine's edition.

§ *Vide* Humboldt's Cosmos, vol. iii. *et seq.*, Sabine's edition.

‖ In the past winter I have been engaged in taking the solar disc on plates of glass, in order thus to confirm the discovery made in 1852 of the difference of temperature in different parts of the disc. The impressions obtained were stronger in the centre than at the edges; but I have abstained from publication, because I do not consider this proof as very decisive. In fact, the image cannot have an equal intensity in every part of the field of the telescope, on account of the obliquity of the different pencils of rays which compose it. It is otherwise in the thermo-electric experiments, where the pile remains invariable in regard to the axis of the lens, and therefore this obliquity hardly exists, or is equal for all points of the disc from which rays fall upon it. I think it right to say this, because I find photographic experiments recently brought forward as proofs of the smaller intensity of the light at the edges than at the centre, and I think it probable that they may be subject to the exceptions which I found in my own.

Those who hold the theory of the production of electricity by vapours, may say that these become rarefied in the morning and condensed in the evening; and hence may arise opposite electric states, the fluid passing in the morning from the earth to the atmosphere, and in the evening from the atmosphere to the earth. This may be true; but why should this condensation always take place at nine in the evening? The hygrometric curves of the different months show at all events a variation in the hour of maximum according to the seasons.

A hypothesis, however, can be found which would conciliate the various facts, viz. that atmospheric changes may generate electricity, but that the direction of the current, which of itself would be indeterminate, may be determined by the magnetic action of the sun. But to expand this further into a hypothesis would be at present premature. We will only say that it is not improbable that the earth is subject to the magnetic action of the sun in a manner unknown to us; but now that magnetic phænomena are developing themselves under so many aspects, we may hope that the explanation of these mysterious actions will soon be found. Not only magnetism, but diamagnetism also may cooperate, and still more the induced currents which exist in bodies of every kind. Two things only I wish to notice. First, the value assigned by Gauss to the magnetism of a cubic metre of the earth*, is such as to make one believe that the whole mass of the earth is really magnetic, and that this force results not only from ferruginous substances, but from the whole globe itself. He proves, in fact, that the eighth part of a cubic metre of the earth has a magnetic moment equal to that which is possessed by a bar of steel 1 lb. in weight and 30 centims. in length, magnetized to saturation. He justly observes, that such a result must surprise physicists, and that it would require 8464 trillions of such bars to represent in space the magnetic force of the earth! The other is, that magnetism may act upon bodies in a manner quite surprising, and of which we are very far from forming an idea before seeing its effects*. The marvellous experiment performed with Ruhmkorff's apparatus, in which a cube of brass, 2 centims. in the side, rotating with the greatest rapidity is struck motionless, if I may use the expression, by an invisible force at the moment of the completion of the circuit of the great electrical magnet between the poles of which it is situated, and without being drawn to one side or the other, remains there fixed, in spite of the powerful torsion of the wire which tends to cause it to rotate,—and resumes its rapid motion when the current ceases,—proves that non-magnetic bodies in motion may, under the influence of a magnet, give rise to phænomena of the most mysterious nature. An action of this kind must take place between the earth and the sun, and thus perhaps may be explained some of those anomalies which still present no small difficulties to every theory which is proposed.

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* General Theory of Terrestrial Magnetism, Taylor's Scientific Memoirs, vol. ii. part 6. art. 5. p. 225. No. 31.

The object of this memoir having been to coordinate the laws of the diurnal motion of the needle, we leave to another opportunity the discussion of its secular variations, as well as of the lunar action, which appears to Colonel Sabine to have been placed beyond doubt; and perhaps the time is not distant when the diurnal magnetic variations will have been subjected to laws depending only on the most simple relation of distance and angle between two magnets.

Conclusion.

The matters discussed at length in this memoir may be summed up in the following propositions:—

1. The action of the sun upon the needle is opposite, according as the sun is north or south of the equator.

2. The action of the sun on the declination-needle has a period, in part but not entirely, analogous to that of the temperature and of the annual and diurnal meteorological changes.

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* I owe it to the kindness of R. P. Palladini, Professor in the College at Naples, that I was a witness of this magnificent experiment, performed with the apparatus with which he has provided the physical cabinet of that college. In truth, one cannot but be surprised at seeing a body rotating with such velocity, stop at the instant at which the circuit is completed, as if it had encountered an invisible obstacle. The torsion given to the thread was sufficient to make the cube rotate with such velocity as to appear a cylinder, and yet on completing the circuit it stopped instantly, and when the current was sufficiently strong, it stopped without the angles being directed to the poles of the magnet (as is usual in the case of weak currents). The explanation of this fact is involved in the phænomena of the magnetism of rotation, but I wished to adduce it as an instance of how a force already known may act in certain circumstances in a new and astonishing manner.

3. The periods of the horizontal and vertical components, following the law of the geographical latitude, and occurring at hours wholly different from the variations of temperature, show a different origin from these. Therefore, if the coincidence in time, in the variations of the temperature and declination, have contributed to the belief of the existence between these two of a mutual relation of cause and effect, the study of the other components makes this coincidence disappear, and therefore destroys every foundation of the hypothesis.

4. All the phænomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance.

LXII. *On Table Spar from the Morne Mountains.*

By Dr. M. Forster Heddle*.

HAVING by my analysis of the Edinburgh Castle Rock *pectolite* taken *table spar* out of the list of British minerals, I have more than usual pleasure in reintroducing it from the Morne Mountains.

Two analyses were made; the first on a specimen sent me by Mr. Greg, the second on one obtained from an Irish dealer, with the following results:—

	On 25 grs.	On 30 grs.
Silica	51·360	50·434
Oxide of iron .	·980	·840
Lime	42·500	43·920
Magnesia . .	·484	·396
Water . . .	1·480	1·360
Carbonic acid .	not det.	2·371
		99·321

The powdered mineral absorbs ·142 per cent. of moisture, and before the blowpipe gives slight and but momentary evidences of the presence of soda.

Both of the above specimens are contaminated by an admixture of carbonate of lime, doubtless derived from the matrix, which is limestone altered by heat.

The *table spar* from this locality bears much resemblance to the *pectolite* found on the Ayrshire coast, consisting of a vein of diverging and interlacing white fibres.

Seeing that *table spar* has been formed by the fusion of lime and silica in a crucible, it is probable that the mineral may have been formed in a similar manner, the granitic heat inducing the combination of a siliceous vein with its calcareous matrix.

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* Communicated by the Author.

LXIII. *On the Benzole Series.*—Part II. *Note on some Derivatives of Xylole.* *By* Arthur H. Church, *Esq.*[*]

WITH xylole, the third member of the benzole series, we have at present but slight acquaintance. Cahours, to whom we owe the discovery of this hydrocarbon, has, I believe, not yet published the details of his investigation. Having had occasion to prepare pure xylole in order to make a determination of its boiling-point[†], I imagined that a study of some of its derivatives might prove of interest. The small quantity of pure xylole having a constant boiling-point of 126°·2 at my disposal, induced me to employ for those experiments, an account of which I propose to give in the commencement of the present paper, the whole of the crude xylole boiling between 124° and 130° obtained from two gallons of light coal-naphtha. In following the directions of Cahours for the preparation of this hydrocarbon, I did not obtain from 16 oz. of the oil from wood naphtha more than 1 oz., boiling at from 126° to 128°.

1. *Preparation of Nitroxylole and Xylidine.*

Crude xylole was dissolved in nitric acid of spec. grav. 1·5, the acid being kept cool; the nitro-compound formed was then separated by the addition of water, and washed until free from nitric acid. Nitroxylole thus obtained is a yellow oily liquid, heavier than water; its odour is less pleasant than that of nitrobenzole; only when impure does it decompose on keeping. By the well-known process of reduction the nitroxylole was converted into xylidine; this base was then purified by repeated crystallization of its combination with oxalic acid; afterwards the xylidine was separated from this salt by distilling it with lime. The xylidine was dissolved in hydrochloric acid, and a considerable quantity of bichloride of platinum solution added. At first a slight precipitate took place; this was redissolved by the addition of a small quantity of water; the whole was then set aside under a bell-jar with oil of vitriol. By fractional crystallizations of the platinum salt I hoped to obtain a xylidine compound, from which, by distillation with caustic soda or potassa, pure xylidine might be procured; for we know in many cases how excellent a method of separation that of fractional crystallization is, and special inquiries by Anderson, C. G. Williams, and others have proved the advantage of this method for the purification of the platinum salts of a series of bases isomeric with that to which xylidine belongs. After twelve hours, the solution to which bichloride of platinum had been added, had deposited a crop of crystals (I.).

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[*] Communicated by the Author.

[†] Philosophical Magazine, April 1855.

This first crop of crystals consisted of slender needles.

I. ·7225 grm. gave ·2088 grm. platinum :—

Per cent. platinum.	Theory (cumidine).
28·89	28·92

The second crop, of needles similar to the first crop.

II. ·5502 grm. gave ·16 grm. platinum :—

Per cent. platinum.	Theory (cumidine).
29·08	28·92

The third crop, stellar groups of short yellow needles.

III. ·325 grm. gave ·098 grm. platinum :—

Per cent. platinum.	Theory (xylidine).
30·15	30·16

The fourth crop, similar to the third.

IV. ·3254 grm. gave ·099 grm. platinum :—

Per cent. platinum.	Theory (xylidine).
30·39	30·16

The fifth and sixth crops, of short, flattened, orange prisms, gave respectively 30·2 and 30·33 per cent. of platinum. The seventh crop, fine golden-yellow scales.

VII. ·2015 grm. gave ·0632 grm. platinum :—

Per cent. platinum.	Theory (toluidine).
31·36	31·51

Preliminary examinations with small quantities of a solution similar to that from which were obtained the crops of crystals described above, gave me some idea as to the proper time for the separation of each successive crop. Crops I. and II. (cumidine platinum salt) weighed together less than 2 grms., while the united weight of crops III., IV., V., and VI. (xylidine platinum salt) was not less than 25 grms. Of crop VII. not more than ·3 of a gramme was obtained.

22 grms. of xylidine platinum salt were introduced into a Florence flask with an excess of caustic soda, the neck of the flask being bent and connected with a condenser. By distillation a large quantity of a nearly colourless oil was obtained in the receiver. This oil was xylidine, $C^{16} H^{11} N$.

Xylidine rapidly attracts oxygen from the air, acquiring a reddish-violet colour and gradually resinifying. It turns dahlia paper green, and slightly restores to reddened litmus paper its original blue tint; the oxalate and sulphate, which were the only salts I prepared, have an acid reaction. Xylidine boils, according to my determination, at 213° to 214°*. The sulphate of xylidine is difficultly soluble in cold water, but crystallizes from hot water in long colourless needles.

2. *Preparation of Nitro-sulphoxylolic and Sulphoxylolic Acids.*

One portion of the pure xylole boiling at 126·2 I treated with fuming nitric acid, &c., and dissolved the nitroxylole thus obtained in fuming sulphuric acid: after three days (the liquid having been heated for one hour at 100° C.) the solution was diluted with water, saturated with carbonate of barium and filtered. The filtrate, evaporated nearly to dryness on the water-bath, yielded a lemon-yellow crystalline powder, nitro-sulphoxylolate of barium; in quantity, however, too small to admit of an attempt to isolate the acid, or to form from this barium compound other salts by double decomposition. On ignition and treatment of the residue with nitric and sulphuric acids†,—

·42 grm. of the barium salt gave ·1607 grm. of sulphate of barium, corresponding to 22·5 per cent. of barium; calculation as $C^{16} H^{8} NO^{4} Ba\, 2SO^{3}$ requires 22·94 per cent.

The remainder of the pure hydrocarbon at my disposal (2·5 grms.) was placed in contact with four volumes of Nordhausen sulphuric acid, and set aside for a week: at the expiration of this time, radiating tufts of needles, long and perfectly colourless, filled the xylole, still partly undissolved by the sulphuric acid; these crystals were collected, placed over sulphuric acid, and enclosed in a glass tube which was afterwards sealed; they were pure sulphoxylolic acid. This substance has a strongly acid reaction; at first its taste is sour, then bitter; it crystallizes well from xylole, is exceedingly soluble in water and in sulphuric acid, deliquesces rapidly in the air, may be fused *in vacuo* or in xylole vapour without decomposition, but if heated above its fusing-point, acquires a dark colour: the fused acid crystallizes on cooling in fine prisms. A barium salt prepared from this acid gave me 26·93 per cent. of barium; theory as $C^{16} H^{9} Ba\, 2SO^{3}$

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* I had intended to make a combustion of this base, but was prevented by an accident which occurred to the apparatus in which I was heating the xylidine in order to determine a second time its boiling-point. I retained, however, a small portion which had distilled twice at a little below 213°, and combined it with sulphuric acid. ·3 grm. of this sulphate gave by precipitation with chloride of barium ·2047 grm. of sulphate of barium, corresponding to 28·11 per cent. of SO^{4}; the formula $C^{16} H^{12} N\, SO^{4}$ requires 28·23 per cent.

† The same process was adopted in all the other determinations of barium given in the present paper.

requires 27·02. The sulphuric acid used in the preparation of this sulpho-acid had acquired a reddish-yellow tint, and gave after neutralization with carbonate of barium, filtration and evaporation, a second portion of sulphoxylolate of barium. Of this salt ·5001 grm. gave ·2287 grm. of sulphate of barium, corresponding to 27·01 per cent. of barium; calculation as $C^{16} H^9 Ba\, 2SO^3$ requires 27·02 per cent.

Sulphoxylolate of barium crystallizes in pearly scales similar to those of sulphotoluolate of barium; if to its hot concentrated aqueous solution cold water be added, I have occasionally found the salt separate in beautiful iridescent plates. The aqueous solution is not altered by ebullition.

I append the analytical results of those experiments relating to pure toluole (boiling at 103·7), spoken of in my former note on the benzole series. The nitro-sulphotoluolic and sulphotoluolic acids were obtained in the same manner as the corresponding terms of the xylole series. The determinations of sulphate of barium were made by ignition of the barium compounds with the precautions previously mentioned.

I. Analysis of toluole.

·4024 grm. of oil gave ·3151 grm. of water and 1·3450 grm. of carbonic acid.

These numbers correspond to the following per-centage composition:—

	Experiment.	Theory ($C^{14} H^8$).
Carbon . . .	91·15	91·3
Hydrogen . .	8·7	8·7

II. Determination of barium in nitro-sulphotoluolate of barium.

·3022 grm. of salt gave ·1289 grm. of sulphate of barium, corresponding to 25 per cent. of barium; theory as $C^{14} H^6 NO^4 Ba\, 2SO^3$ requires 24·41 per cent.*

III. Determination of barium in sulphotoluolate of barium.

·705 grm. of salt gave ·349 grm. of sulphate of barium, corresponding to 29·14 per cent. of barium; theory as $C^{14} H^7 Ba\, 2SO^3$ requires 29·08 per cent.

May 1855.

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* The excess of barium given by experiment over that required by theory arose from a slight admixture of nitrate of barium with the original salt.

LXIV. *On the Existence of an Electrical Æther through Space.*

By George James Knox, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the year 1840 you were kind enough to insert in your Magazine a paper, entitled "On the Direction and Mode of Propagation of the Electric Force traversing Interposed Media," in which memoir I endeavoured to prove, from the experiments of Sir H. Davy, that an electric current consists in alternate states of induction and equilibrium of the particles of the medium conveying the current, the *intensity* of the current being proportional to the *rapidity of change* of induction and equilibrium, and consequently that the *mass* of oscillating æther surrounding the particles represents the *quantity*, while the *rapidity* of the oscillations represents the intensity of an electric current.

Your March Number contains some very interesting experiments which were made by Mr. Latimer Clark, on the transmission of currents of electricity of varying intensity through 768 miles of gutta-percha wire, indicating a velocity of propagation of about 1000 miles in a second, which velocity is sensibly *uniform* for *all intensities from* 31 *cells to* 500; which results, Dr. Faraday remarks, "afford a fine argument in favour of the opinion of those who suppose the electric current to be analogous to the vibrations of air under the action of sonorous bodies."

The experiments of Professor Grove on the electro-chemical polarity of gases (vol. iv. p. 513), where he obtains rings alternately bright and oxidated, showing effects of oxidation and reduction by the same current on the same plate, he considers as "analogous to the phænomena of interference in light; though doubtless, if this be a right view, the very different modes of action of light and electricity would present very numerous phænomenal distinctions."

The idea has lately been presented to my mind, that the oscillations of the electrical æther in combination with the particles of the medium conveying a current, produce undulations, not only in the æthers of light and heat, but also in another æther, which Dr. Draper calls the tithonic æther, which, if experiment proves to be the case, should be more correctly termed the electrical æther.

Dr. Draper, in the year 1847, undertook a series of experiments upon the rays of light emitted by incandescent bodies, from which he concluded that when a platinum wire is heated by the voltaic pile or otherwise, it emits rays of light, which increase in refrangibility proportionally to the increase of heat, which he explains thus:—"As the luminous effects are undoubtedly owing to a vibratory movement executed by the molecules of the platinum, it seems from the foregoing considerations to follow, that the frequency of those vibrations increases with the temperature."

Sir David Brewster has observed, that in the spectra produced by the electric light the chemical rays are more numerous than in those produced by the lime light.

The problem then to be solved is,—whether the chemical rays be produced *directly* by the oscillations of the electrical æther in the platinum wire, or *indirectly* by the heat produced.

This question might be resolved by observing the effect produced by voltaic piles of *different* intensities, the *heat* remaining *constant*; and if so, it would afford a strong argument in favour, not only of an oscillatory movement in the electrical æther in combination with the particles of bodies, but also of the existence of such an æther through space.

I have the honour to be, Gentlemen,
Your obedient Servant,
GEORGE JAMES KNOX.

2 Finchley New Road, St. John's Wood,
May 3, 1855.

LXV. *On a New Compound of Gold and Mercury.*

By T. H. HENRY, *Esq., F.R.S.*[*]

WHEN gold is treated with mercury in large excess, a definite compound is formed which remains dissolved in the mercury, from which, however, it often separates in a crystalline form, and from which it may be almost entirely separated by mechanical means, such as pressure through chamois-leather. This solid amalgam crystallizes in four-sided prisms, and contains six parts of gold to one of mercury, and fuses on elevating the temperature (Gmelin, vol. iii.). The mercury, however, which has passed through the chamois-leather always contains gold, in proportion varying from a minute trace to 10 grs. in the pound. In the metallurgical processes for extracting gold, it becomes important to estimate the amount of gold remaining in the fluid part of the mercury, and it was during some experiments made with the view of ascertaining the best method of doing so that this new amalgam was discovered.

This substance is best obtained by dissolving gold in mercury in the proportion of 1 part of gold to 1000 of mercury, about 7 grs. to the lb. avoirdupois, squeezing the solution through chamois-leather, and dissolving the mercury in dilute nitric acid with gentle heat. The compound is left in the form of four-sided prisms of the most brilliant metallic lustre, which may be boiled in nitric acid without decomposition, and exposed to the atmosphere for months without becoming tarnished. On exposure to heat they do not fuse, but afford a sublimate of metallic mercury, amounting in my experiments to rather less than 12 per cent.; the form of the crystals remained unaltered, their lustre was little affected, and the residue consisted of pure gold. This would correspond to a compound of four atoms of gold to one of mercury:—

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[*] Communicated by the Author.

Au . . .	197×4=	788	or	88·74
Hg . . .	100	100	...	11·26
		888		100·00

LXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 398.]

Feb. 1, 1855.—Colonel Sabine, Treas. and V.P., in the Chair.

THE following communication was read:—

"Micro-chemical Researches on the Digestion of Starch and Amylaceous Foods." By Philip Burnard Ayres, M.D. Lond.

After some general historical remarks on the methods hitherto employed in the investigation of the complicated phænomena of the process of digestion, the comparatively small results obtained by chemical analysis of the contents of the stomach, intestinal canal, and of the evacuations, by Tiedemann and Gmelin, Berzelius, and others, the author proceeded to demonstrate the necessity of a minute examination of the contents of the alimentary canal by the microscope, and such chemical tests as we possess for the determination of the changes of such articles of food as exhibit definite structure.

In order that we may ultimately arrive at a complete exposition of the phænomena of digestion, he is of opinion that it will be necessary to examine,—first, the structure of particular kinds of food, then the changes produced in them by cooking, and lastly to trace the changes they undergo at short intervals, through the alimentary canal from the stomach to the rectum. The results of a series of researches of this character on the changes in starch, and starch-containing foods, are presented in this memoir.

The method adopted for the examination of the changes in starch and starch-foods was as follows:—An animal was kept fasting twenty-four hours, and afterwards confined to a diet consisting of the starch or amylaceous food, with water, for five or six days, until the debris of all other kinds of food previously taken were cleared from the alimentary canal. At a determinate time, after a meal, the animal was killed, the abdomen laid open as quickly as possible, and ligatures placed at short intervals on the intestinal canal, from the pylorus to the rectum. The contents of the stomach and each portion of the intestinal canal included between the ligatures were then carefully examined. This mode of examination sufficed to determine the changes which occur in the food during normal digestion; but other questions as to the particular secretion or secretions by which the changes observed were effected.

The fluids poured into the alimentary canal are five in number,—the saliva, gastric juice, bile, pancreatic juice, and finally, the intestinal mucus.

The influence of the saliva is easily determined, by chewing the particular food subjected to experiment, and keeping the mixture at about 98° Fahr. The combined action of the saliva and gastric juice is seen in the contents of the stomach. To determine the action of the bile, the common bile-duct was tied, and to ascertain the action of the intestinal mucus, it was necessary to ligature the bile and pancreatic ducts. If the digestion of the substance is not effected in the stomach, it is evident that it cannot be attributed to the saliva or gastric juice; if the digestion is still effected in the intestinal canal after ligature of the bile-duct, it cannot be attributed to the action of the saliva, gastric juice or bile; if it still go on after ligature of the bile and pancreatic ducts, the digestive power must of necessity be referred to the action of the intestinal mucus, provided no change has previously taken place in the stomach; but if the food passes unchanged after cutting off the supply of bile and pancreatic juice, but proceeds after ligature of the bile-duct alone, the act of digestion must be referred to the pancreatic juice.

The author first briefly describes the structure of the starches and starch-containing vegetables employed in his experiments; then the changes produced by cooking, and finally enters on a minute description of the changes observed in the experiments he performed on normal digestion, and after cutting off the supply of bile and pancreatic juice.

The correct appreciation of the structure of the starch-granule is of considerable importance in relation to these investigations, and the author believes that he has been able to afford a satisfactory solution of this vexed question. The changes observed during the digestion of starch favour the original opinion of Leuwenhoeck, that the starch-granule consists essentially of an investing membrane or cell-wall, enclosing an amorphous matter, the true starch, which strikes an intense blue colour with iodine; and these changes also support the opinion of Professor Quekett, that the concentric circles seen on the starch-granules of many plants are simple foldings of the investing membrane, leaving it still doubtful, however, whether these concentric circles are not in the starches of some plants composed of linear series of dotted elevations or depressions of the investing membrane.

By these experiments it was determined that the concentric circles remain after the whole of the starch matter, colourable by iodine, was removed, and that even then the characteristic cross and colours were still seen when the granules were viewed by polarized light, although more feebly than before; this result being probably due to the lessened power of refracting light, after the removal of the starch matter.

After describing the structure of the wheat-grain and flour, the changes occurring in the wheat-starch during the manufacture of bread are given in detail; but the most interesting of the changes produced by cooking are those seen in the boiled or roasted potato and in the boiled pea.

In each of these the act of cooking effects two purposes:—it causes great enlargement and physical change of the starch-granules, and dissolves the intimate adhesion of the starch-cells, which afterwards appear as ovoid or globular, slightly adherent bodies distended by the swollen starch-granules, the outlines of which are indicated by more or less irregular gyrate lines, produced by the mutual compression of the starch-granules within an inelastic cell-membrane.

The starch-granules of the pea possess a much thicker investing membrane than those of the potato, which causes their outlines to remain much more distinct after the removal of the true starch substance during the process of digestion. The other structures seen in the pea are carefully described; the most curious among them being the cells composing the external layer of the testa, which bear so strong a resemblance to columnar epithelium of the intestine, that they might be mistaken for the latter by an inattentive observer.

The substances submitted to experiment were,—1, boiled wheat-starch; 2, wheaten bread; 3, uncooked tous les mois; 4, boiled tous les mois; 5, boiled potato; 6, uncooked peas; 7, boiled peas; 8, boiled peas after ligature of the bile-duct; 9, boiled potatoes after ligature of the bile and pancreatic ducts. Several subsidiary experiments were made to determine the action of the intestinal mucus, the saliva, and the substance of the pancreas, on starch.

The conclusions at which the author arrives from the experiments are,—

1. That the starch-granule is composed of two parts, chemically and histologically distinct,—a cell-membrane and homogeneous contents. The markings seen on many varieties of starch are referred to folds or markings of the investing membrane.

2. No perceptible change occurs in the starch, whether raw or cooked, during its sojourn in the stomach of quadrupeds or the ventriculus succenturiatus and gizzard of birds; all the granules preserve their perfect reaction with iodine and their pristine appearance.

3. The conversion of boiled starch into dextrine and glucose is chiefly effected in the first few inches of the small intestine, but it continues to take place in a less degree throughout the entire intestinal canal.

4. In the digestion of boiled wheat or other starch, or of wheaten bread, the bulk of the mass rapidly diminishes in its passage through

the small and large intestines, so that it ultimately yields only a small quantity of fæcal matter. After being deprived of their contents, the membranes of the granules shrink and shrivel up into a minute granular matter, which constitutes the chief bulk of the fæcal evacuations after an exclusive diet of starch food.

5. The digestion of raw starch food (peas) in the pigeon or other granivorous birds goes on much more slowly, and progresses pretty equally throughout the entire intestinal canal. The starch-granules, whether free or included in cells, become intersected by radiating or irregular lines or fissures, more or less opaque or granular; they also gradually lose their characteristic reaction with iodine; and this important change, commencing at the surface, progresses towards the centre, until the whole of the starch matter is removed, leaving the starch-membranes often apparently whole, retaining their characteristic markings. The fissured and granular condition of the starch-granules is not due to their trituration in the gizzard, but to the action of the intestinal fluids, since it was often seen in granules enclosed in and protected by perfect starch-cells. In the digestion of raw starch food, a considerable quantity always escapes change, for many starch-cells and granules in the fæces perfectly retain the characteristic reaction with iodine.

6. As the starch remains unchanged in the stomach, its conversion into glucose cannot be attributed to the saliva or gastric juice, unless we suppose these fluids to remain inactive in the stomach, and suddenly to regain their activity in the first part of the small intestine. The author found that the saliva was capable of effecting the conversion of starch into glucose, but that the mixture of saliva and gastric juice in the stomach did not possess that property even after being rendered alkaline by carbonate of soda. It is probable that the converting power of the saliva, as it flows from the mouth, depends not on the true saliva, but on the buccal mucus; for Magendie found that saliva taken from the parotid duct was wholly inactive, while the mixed saliva from the mouth effected the conversion with great facility. Unless, then, the sublingual and submaxillary glands secrete a different fluid from the parotids, it is evident that the activity of the saliva must be attributed to the buccal mucus.

7. The difference between the digestion of boiled and raw starch in dogs is seen in the experiments on the digestion of boiled wheat-starch, boiled tous les mois, and bread. In all these, some starch-granules escape the action of heat and water, and remain in nearly their pristine condition. These uncooked starch-granules undergo slow and imperfect changes, being fissured, broken, and more or less altered, but in general retaining their characteristic reaction with iodine.

8. The conversion of starch into glucose is not effected by the bile, since after ligature of the common bile-duct, the changes occur to as great an extent as when the bile passes freely into the intestinal canal.

9. It is not due to the pancreatic juice, inasmuch as after ligature

of the bile and pancreatic ducts in the same animal, the digestion of starch is still effected.

10. The only remaining secretion is the intestinal mucus, which is especially abundant at the upper part of the intestinal canal; and a further proof is afforded of the activity of the intestinal mucus taken from the upper part of the duodenum above the entrance of the pancreatic duct after ligature of this duct and the common bile-duct, by its capability of converting a large quantity of fresh boiled starch into glucose out of the body.

11. In the cooking of starch-containing vegetables, such as potatoes and peas, the adhesion of the starch-cells is dissolved or weakened, so as to render them easily separable and amenable to the action of the intestinal fluids. At the same time the starch-granules undergo a large increase in bulk, distend the cells, and by their mutual compression, their outlines present the appearance of gyrate lines beneath the cell-wall. The cells seldom burst so as to emit their contents, or present any appreciable opening through which the intestinal fluids can directly penetrate. The author cannot positively affirm so much of the starch-membranes, because these are so extremely delicate that fissures might be invisible, but he believes that in a great number the membranes remain entire.

12. If this be the case, the conversion of starch matter into glucose must be effected by the permeation or endosmose of the intestinal fluids through the invisible pores of two membranes, in the digestion of the pea, the potato, and other similar foods, and the glucose must escape through the same membranes by exosmose.

13. Before the conversion of starch into glucose, the amylaceous matter contained in the starch is more dense than the intestinal mucus in immediate contact with the cells, and an inward current or endosmose is established; but after that conversion, the syrupy fluid is less dense than the mucus, and then an outward current or exosmose occurs, by which the glucose escapes from the cells into the intestine and is absorbed. If this be the case, as the details of the experiments tend strongly to prove, a new and important function is assigned to the intestinal mucus.

14. In normal digestion, chyme escapes very slowly from the stomach into the duodenum, in small quantities, as it is detached from the alimentary mass by the muscular movements of the stomach, and this gradual propulsion often occupies several hours after a meal. This slow propulsion is evidently intended to expose the comminuted food fully to the action of the intestinal juices, and produce an intimate mixture with them. The comparatively empty condition of the upper part of the small intestine, even during active digestion, is thus fully explained.

15. If the food be too finely divided or incapable of a second solidification in the stomach, it passes too rapidly into the first part of the small intestine, is insufficiently mixed with the intestinal fluids, and a considerable part escapes digestion. On the other hand, if it enters the small intestine in masses incapable of reduction

by the muscular action of the parts or solution in the fluid, it traverses the intestinal canal unchanged, except at the surface, which is then alone exposed to the action of the intestinal fluids.

16. It is not necessary for the conversion of starch into glucose that the fluids in the duodenum or other parts of the intestinal canal should be alkaline, or even neutral, for in several of the experiments the contents of every part of the alimentary canal had an acid reaction.

17. The greater part of the intestinal mucus is not excrementitious, for little, if any, mucus is perceptible in the fæces in normal digestion, except at their surface, whereas the greater proportion of the contents of the small intestine consists of mucus. A considerable quantity of mucus is seen in the cæcum, but it rapidly diminishes in the colon, and is scarcely detectible in the fæces, except that on the surface, which is probably derived from the mucous membrane of the rectum. The author raises the question, whether one of the chief functions of the cæcum is not to effect the conversion of the intestinal mucus into some other substance capable of re-entering the blood, and performing some ulterior purpose in the animal economy.

18. In normal digestion, the separation of the epithelium of the mucous membrane of the intestine is the exception instead of the rule, as stated by some physiologists. The author questions the theory of the detachment of the epithelium of the villi in each act of absorption, on the grounds that the presence of detached epithelium was unfrequent in the whole course of his experiments; that epithelium is readily detached by manipulation; that the continual reproduction of such a vast amount of cell-tissue must necessarily be accompanied by a vast expenditure of vital force; and finally, that it is not necessary, because fluids readily penetrate epithelial membranes.

19. The passage of a given food through the whole length of the intestinal canal may occupy a comparatively short time, especially when the animal is fasting. In one experiment, where a pigeon refused food until the fæces contained no visible debris of previous food, starch-granules were detected in the fæces within two hours after a meal, and this although the intestine of this animal is extremely narrow and about a yard in length.

20. A remarkable circumstance in the digestion of starch or starch foods is the constant presence of myriads of Vibriones in the lower part of the intestinal canal. They are generally first observed in the lower part of the small intestine, as minute brilliant points, just visible with a power of 600 diameters, in active movement. They increase in numbers towards the cæcum, in which a large number of fully-developed vibriones are constantly seen. These minute organisms increase in size and length in the colon and rectum, and their fissiparous mode of propagation, first described by the author in the 'Quarterly Journal of Microscopical Science,' may be distinctly traced by examining the contents of these portions of the intestine.

February 15, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communication was read:—

"An Account of some recent Researches near Cairo, undertaken with the view of throwing light upon the Geological History of the Alluvial Land of Egypt."—Part First. By Leonard Horner, Esq., F.R.SS.L. & E., F.G.S.

The author commences by observing, that although it be highly improbable that we can ever form an appropriate estimate in years of the age even of the most modern strata, we are not cut off from all hope of being able to assign an amount in years to the duration of some of the great geological changes which, in past ages, the present surface of the earth has undergone, by causes that are still in operation; especially by a careful study of the formation of the deltas of great rivers, and of the action of the latter on the rocks and soils they traverse in their course. If in a country in which a certain alteration in the land has occurred, we know that such alteration has taken place in part within historical time, and if the entire change under consideration presents throughout a tolerable uniformity of character, we may be justified in holding the portion that has taken place within the historical period to afford a measure of the time occupied in the production of the antecedent part of the same change.

Egypt supplies us with the earliest evidence of the existence of the human race recorded in works of art; in its monuments we find the dawn of the historical period and of civilization; and that land alone, of all parts of the world as yet known to us, offers an instance of a great geological change that has been in progress throughout the whole of the historical period, in its annual inundations and the sediment these deposit to form the alluvial land in the valley of the Nile; and there is good reason for believing that the change had been going on with the same uniformity for ages prior to that period when our reckoning of historical time begins. To investigate the formation of the alluvial land in the valley of the Nile in Upper and Lower Egypt is therefore an object of the highest interest to the geologist and the historian.

The author being impressed with the conviction that this geological problem could only be solved by having shafts and borings made in the alluvial soil to the greatest practicable depths, determined to have some such experiments made; as the results might lead the way to other researches on a greater scale. The ground on which he hoped to be able to form a chronometric scale by which the total depth of sediment reached might be measured, was the same as that on which the French engineers in 1800 had proceeded, viz. the accumulation of Nile sediment around monuments of a known age. If that depth of sediment be divided by the number of centuries that have elapsed since the date of the erection of the monument, we obtain a scale of the secular increase of which the base of the monument is zero, assuming that the average increase from century to century has been uniform within an area of some extent.

Then if the excavation be continued below the base stone, and the sediment passed through exhibits similar characters as to composition with that above the base line of the monument, it would be fair to apply the same graduation below the zero-point of the scale as above it, and, if we reached so far, we should be able to estimate the time that has elapsed since the first layer of sediment was deposited on the rock forming the channel over which the water spread when it first flowed northward from its sources in the interior of Africa, subject however to correction for causes that might make a difference in the rate of increase between the earlier and later periods.

The author submitted his scheme to the President and Council of the Royal Society, who encouraged his proceeding by acceding to his request of a grant from the Donation Fund at their disposal, towards the expenses of the researches.

The author introduces his subject by a sketch of the physical geography and geology of Egypt, a description of the annual inundations, and of the sediment deposited from the water of the Nile.

Egypt is separated from Nubia by a low hilly region, about fifty miles broad from north to south, chiefly composed of granitic rocks, but associated with two kinds of sandstone, the one belonging to the cretaceous series, the other of the newer tertiary age. The valley of Upper Egypt is bounded by two ranges of hills running northward, the Arabian range on the right, the Libyan on the left of the river, both alike composed of sandstones and limestone. The cretaceous sandstone extends from the granitic rocks forming the first cataract at Assouan for about eighty-five miles, where it is covered by a limestone which has the characters of the upper chalk of Europe. This chalk continues on both sides of the valley for about 130 miles, when it is covered in its turn by a tertiary nummulite limestone, and of which the further prolongation northward of both ranges is composed; this nummulite limestone can be well studied in the extensive quarries of Gebel Mokattam above Cairo.

The author briefly describes the operations of the private association of English, French, and Austrian Engineers in 1846–47, commonly called the French Brigade, for the purpose of determining the disputed question of the relative levels of the Red Sea and Mediterranean. The French engineers, at the beginning of the present century, had come to the conclusion that the Red Sea was about 30 feet above the Mediterranean, but the observations of Mr. Robert Stephenson, the English engineer, at Suez, of M. Negretti, the Austrian, at Tineh near the ancient Pelusium, and the levellings of Messrs. Talabot, Bourdaloue and their assistants, between the two seas, have proved that the low-water mark of ordinary tides at Suez and Tineh is very nearly on the same level, the difference being, that at Suez it is rather more than one inch lower.

At the island of Philæ, about five miles above Assouan, may properly be placed the first entrance of the Nile into Egypt. It is here about two miles broad, but is soon after divided into several branches by the rocks that rise up in its bed to form the rapids, commonly called the First Cataract, which have a descent of about 85 feet in a distance of five miles. Here the river is contracted to about a third of a mile. Assouan is about 300 feet above Cairo, and the distance between the two places being 556 miles, the average fall of the river is little more than half a foot in a mile, 0·54, and Assouan being 365 feet above the Mediterranean, and 696 miles distant from it, the average fall of the Nile from the foot of the First Cataract to the sea is 0·525 in a mile. Low Nile at Cairo is 43 feet 6½ inches (as measured by the French Brigade in 1847) above low-water mark in the Mediterranean, and the distance being 149 miles, the average fall is little more than 3½ inches in a mile. The author cites a report of Mr. Rennie to the British Association in 1834, showing that the fall of the Thames, between Chertsey and Teddington Lock, is nearly 17½ inches in a mile.

The commencement of the annual inundation is about the summer solstice. The rise is scarcely perceptible for six or eight days, it then becomes more rapid, and about the middle of August has usually reached one-half of the greatest amount; it attains its maximum towards the end of September, remains pretty stationary for about fourteen days, and then begins to fall, at first at a more rapid rate than that with which it rose, but after it has fallen one-half the decrease is very gradual, and it goes on sinking until the end of May. The rise continues about 90 days, the falling lasts 250. In 1846, Mougel Bey, the French engineer of the Barrage near Cairo, found the maximum rise 7·20 metres, or 30 feet 10 inches.

When the inundations commence, the Nile is of a reddish colour, and is loaded with sand and mud. From the fall between the Second Cataract at Wadi Halfa and the First, a distance of 214 miles, being not more on an average than 9 inches in a mile, very little coarse gravel can be transported by the river into Egypt. The greater portion of the heavier detritus falls down in the higher parts of Upper Egypt, and from the very gentle slope of the Delta, only a small amount of the solid matter suspended in the water can reach the sea; still, however, the sea has been observed to be turbid at a distance of forty miles from the mouths of the Nile.

The author then proceeds to describe the recent researches. His first and indispensable step was to procure the aid of a competent person to conduct his projected operations, and he was fortunate in obtaining the active and intelligent aid of an Armenian gentleman, educated and long resident in England, Hekekyan Bey, a civil engineer, who had occupied some important positions in the service of the Viceroy Mehemet Ali, especially as chief of the Polytechnic School in Cairo. But nothing could be done without the consent of the then Viceroy Abbas Pacha, more especially as the spot the author had selected for his first operations was in a garden of the Pacha. By the active intervention of Her Majesty's Consul-General in Egypt, the Honourable Charles Augustus Murray, not only was the Viceroy's consent obtained, but his Highness was pleased to direct his ministers to place at the disposal of Hekekyan Bey whatever was necessary to conduct the operations in the most complete manner; and, with truly royal munificence, ordered that the whole expense of them should be defrayed by his Treasury. The author never contemplated having the means of making these researches on more than a very limited scale, but he had now the prospect, and it has since been realized, of their being conducted on a very enlarged plan.

It has been often a subject of regret that experiments of this nature, of which the French had set an example half a century ago, had not been followed up. On this subject the author remarks, that the operations are of a nature that scarcely any individual traveller could undertake; for they require a large body of men, some practised in the art of surveying, and as they can only be carried on continuously after the inundation waters have subsided for some time, and therefore at a season of the year when the heat is excessive, those only inured to the climate could stand the work.

The place selected for commencing the operations was at the Obelisk of Heliopolis, about six miles below Cairo, the oldest known; erected, according to Lepsius, 2300 years before Christ. The author having given Hekekyan Bey full and minute directions as to the manner in which the researches were to be carried on, the observations to be made, the plans and reports to be drawn up, and the specimens of soils sunk through to be selected, the operations commenced in June 1851. Sixty men were employed under the direction of Hekekyan Bey, assisted by an officer of Artillery, and some young engineers from the Polytechnic School in Cairo.

Nine pits or excavations were sunk at different distances around the obelisk, each down to the level of the filtration water from the Nile at that season, and as much under the surface of that water as was practicable. The most important of these was one close to the obelisk. The upper surface of the pedestal on which the obelisk rests, was reached at the depth of 5 feet 6 inches below the surface of the ground, Nile mud being accumulated to that height; the pedestal was 6 feet 10 inches in height, and it was found resting on two limestone flags, the upper 16 inches, the lower 15 inches in thickness, and this foundation was laid upon pure quartzose sand. This last was penetrated to the depth of 3 feet 2½ inches below the lower layer of limestone.

The author gives a section and description of each of the nine excavations. But before doing so, he states that he obtained twenty-eight specimens of soils sunk through in different parts of the Nile Valley, eleven of which were carefully analysed at the Royal College of Chemistry, under the superintendence of Dr. Hofmann. A collection of specimens, duplicates of which are in the possession of Hekekyan Bey, serve as a standard for the description in his reports of the soils passed through, to avoid the necessity of sending specimens of identical alluvia. These samples were carefully compared with the specimens analysed, and were found to resemble them closely in external characters.

The results of the analyses are given by the author, and the average of eight specimens of Nile mud gives the following composition in 100 parts:—

Silica	54·585
Sesquioxide of iron	20·215
Sesquioxide of alumina	6·418
Alumina	5·237
Carbonate of lime	3·717
Sulphate of lime	0·245
Lime	1·912
Magnesia	0·762
Potassa	0·473
Soda	0·553
Organic matter	5·701
	99·818

In order to ascertain the amount of solid matter held in suspension in the water of the Nile near Cairo, the author described to Dr. Abbott, a resident in that city, the method he had followed in 1832 to determine the amount of solid matter suspended in the water of the Rhine at Bonn, and requested him to undertake the experiment on the same plan, which he did, and the result gave 110·6 grains in an imperial gallon. The residuum sent was analysed at the Royal College of Chemistry, and yielded very nearly the same result as to composition as the above average analysis of the Nile sediment.

On examining the descriptions of the soils sunk through in the nine excavations at Heliopolis, it appears that they consist of two principal kinds, viz. earths and sands. The earths vary in colour, but are so nearly allied, passing by such insensible shades into each other, and having so great a resemblance to the modern Nile sediment, that they may all be classed as Nile mud. The sands are almost entirely pure quartz, similar to those of the adjoining deserts.

In the same horizontal plane, even in this limited space of half a square mile, there is a very considerable difference in the nature of the soil, and in none of the excavations was there an instance of lamination in the deposit.

When it is considered how small is the amount of sediment left annually by the inundations in any one place, it is very difficult to conceive, in the author's opinion, how there should be in any one spot so great a thickness as 12½ feet of one kind of sediment, as is the case in one of the excavations, without any lamination or other sign of successive deposition, and still more inconceivable that in pits within a very short distance of each other different kinds of soil should be found at the same levels. Other causes than the tranquil deposit from inundation water must have been at work in the formation of this portion of the alluvial land. The layers of sand were most likely blown across the valley from the desert.

The author deems it advisable to abstain from general remarks, and from all inferences as to the secular increase of the alluvial deposits, until he has had an opportunity of laying before the Society an account of the far more extensive researches made in the district of Memphis in 1852, and during the last year in a series of pits sunk in a line across the valley of the Nile, extending from the Libyan to the Arabian Chain, in the parallel of Heliopolis.

In the various excavations that have been made in the prosecution of this inquiry, many objects of art of historical interest have been discovered; but as these do not come within the province of the Royal Society, the author proposes to give an account of them in a memoir to be laid before another learned body.

March 1, 1855.—Charles Wheatstone, Esq., V.P., in the Chair.

The following communication was read:—

"On the Structure, Functions, and Homology of the Manducatory Organs in the Class Rotifera." By Philip Henry Gosse, A.L.S.

In this paper the author institutes an examination of the manducatory organs in the class Rotifera, in order to show that the various forms which they assume can all be reduced to a common type. He further proposes to inquire what are the real homologues of these organs in the other classes of animals, and what light we can gather, from their structure, on the question of the zoological rank of the Rotifera.

After an investigation of the bibliography of the class from Ehrenberg to the present time, in which the vagueness and inexactitude of our knowledge of these organs is shown, the author takes up, one by one, the various phases which they assume throughout the whole class; commencing with *Brachionus*, in which they appear in the highest state of development. Their form in this genus is therefore taken as the standard of comparison.

The hemispherical bulb, which is so conspicuous in *B. amphiceros*, lying across the breast, and containing organs which work vigorously against each other, has long been recognized as an organ of manducation: it has been called the gizzard; but the author proposes to distinguish it by the term *mastax*. It is a trilobate muscular sac, with walls varying much in thickness, receiving at the anterior extremity the *buccal funnel*, and on the dorsal side giving exit to the *œsophagus*.

Within this sac are placed two geniculate organs (the *mallei*), and a third on which they work (the *incus*). Each *malleus* consists of two parts (the *manubrium* and the *uncus*), united by a hinge-joint. The *manubrium* is a piece of irregular form, consisting of *carinæ* of solid matter, enclosing three areas, which are filled with a more membranous substance. The *uncus* consists of several slender pieces, more or less parallel, arranged like the teeth of a comb, or like the fingers of a hand.

The *incus* consists of two *rami*, which are articulated by a common base to the extremity of a thin rod (the *fulcrum*), in such a way that they can open and close by proper muscles. The fingers of eac rest upon the corresponding *ramus*, to which they are attached by an elastic ligament. The *mallei* are moved to and fro by distinct muscles, which the author describes in detail; and by the action of these they approach and recede alternately; the rami opening and shutting simultaneously, with a movement derived partly from the action of the *mallei*, and partly from their own proper muscles.

All these organs have great solidity and density; and, from the action of certain menstrua upon them, appear to be of calcareous origin.

The writer proceeds to describe the accessory organs. The ciliated disc has an infundibuliform centre, which commonly merges into a tube before it enters the *mastax*. The particles of food that float in the water, or swimming animalcules, are whirled by the ciliary vortex into this tube; and, being carried into the *mastax*, are lodged upon the *rami*, between the two *unci*. These conjointly work upon the food, which passes on towards the tips of the *rami*, and enter the *œsophagus*, which opens immediately beneath them.

From this normal condition, the author traces the manducatory organs through various modifications, in the genera *Euchlanis*, *Notommata aurita*, *N. clavulata*, *Anuræa*, *N. petromyzon*, *N. lacinulata*, *Furcularia*, *N. gibba*, *Synchæta*, *Polyarthra*, *Diglena*, *Eosphora*, *Albertia*, *F. marina*, *Asplanchna*, *Mastigocerca*, *Monocerca*, and *Scaridium*. Some of these display peculiarities and aberrations highly curious. Notwithstanding the anomalies and variations which occur, however, the same type of structure is seen in all; and the modifications in general may be considered as successive degenerations of the *mallei*, and augmentations of the *incus*.

The form of the manducatory organs, which occurs in *Triarthra*, *Pompholyx*, *Pterodina*, *Œcistes*, *Limnias*, *Melicerta*, *Conochilus*, *Megalotrocha*, *Lacinularia*, and *Tubicolaria*, is next examined. The organs are shown to be essentially the same as in the former type, but somewhat disguised by the excessive dilatation of the *mallei*, and by the soldering of the *unci* and the *rami* together, into two masses, each of which approaches in figure to the quadrant of a sphere.

Attention is then directed to what has been called (but by a misapprehension) the "stirrup-shaped" armature of the genera *Rotifer*, *Philodina*, *Actinurus*, &c. Here, however, the organs are proved to have no essential diversity from the common type; their analogy with those last described being abundantly manifest, though they are still further disguised by the obsolescence of the *manubria*.

Floscularia and *Stephanoceros*, the most elegant, but the most aberrant forms of Rotifera, close the series. The *mastax*, in these genera, is wanting; and in the former genus the *incus* and the *manubria* are reduced to extreme evanescence, though the two-fingered *unci* show, in their structure, relative position and action, the true analogy of these organs.

Having thus shown that there is but one model of structure, however modified or disguised, in the manducatory organs of the Rotifera, the author proceeds to the question of their homology. He argues on several grounds that they have no true affinity with the gastric teeth of the Crustacea, though he states his conviction that the Rotifera belong to the great Arthropodous division of animals.

It is with the Insecta that the author seeks to ally these minute creatures; and, by a course of argument founded on the peculiarities of structure already detailed, he maintains the following identifications:—that the *mastax* is a true *mouth*; that the *mallei* are *mandibles*; the *manubria* possibly representing the *cheeks*, into which they are articulated; that the *rami* of the *incus* are *maxillæ*; and that the *fulcrum* represents the *cardines* soldered together.

While the author maintains the connexion of Rotifera with Insecta, through these organs in their highest development, he suggests their affinity with Polyzoa, by the same organs at the opposite extremity of the scale, since the oval muscular bulbs in *Bowerbankia*, which approach and recede in their action on food, seem to represent the quadriglobular masses of *Limnias* and *Rotifer*, further degenerated.

If this affinity be correctly indicated, the interesting fact is apparent, that the Polyzoa present the point where the two great parallel divisions, Mollusca and Articulata, unite in their course towards the true Polypi.

March 8, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

The following paper was read:—

"On the Perihelia and Nodes of the Planets." By Edward J. Cooper, F.R.S.

Prefatory to my volume on Cometic Orbits, published in 1852, I invited the attention of astronomers to the several points of resemblance between the planetary orbits and those of periodic comets. Among these it was shown, that of the heliocentric longitudes of perihelia and ascending nodes of the then known planets and periodic comets, two-thirds were situated in the heliocentric semicircle between 315° and 135°. The planets stood thus in quadrants—

$$
\text{L. P.'s between}\;
\left.\begin{array}{l}
45^{\circ}\text{ and }135^{\circ}= \qquad\qquad 9\\
\left.\begin{array}{l}135\text{ and }225=4\\225\text{ and }315=3\end{array}\right\}7\\
315\text{ and }\;45= \qquad\qquad 7
\end{array}\right\}16.
$$

$$
\text{☊}\quad\text{between}\;
\left.\begin{array}{l}
45\text{ and }135= \qquad\qquad 13\\
\left.\begin{array}{l}135\text{ and }225=4\\225\text{ and }315=4\end{array}\right\}8\\
315\text{ and }\;45= \qquad\qquad 1
\end{array}\right\}14.
$$

Here the L. P.'s appeared as 16 to 7, and the ascending nodes as 14 to 8. Two additional asteroids were subsequently discovered leaving the L. P.'s as 16 to 9, and the ascending nodes as 15 to 9.

Again, in 1853, I sent a note upon the same subject to the Royal Astronomical Society of London. At that time a considerable addition had been made to the asteroids, and the total number of planets had risen from 25 to 35. Following the same distribution of the perihelia and ascending nodes as in my previously published work, the result was—

$$\text{L. P.'s between}\ \left.\begin{array}{l} 45^\circ \text{ and } 135^\circ = \quad 13 \\ \left.\begin{array}{l} 135 \text{ and } 225=5 \\ 225 \text{ and } 315=6 \end{array}\right\} 11 \\ 315 \text{ and } \ 45= \quad 11 \end{array}\right\} 24.$$

$$\text{☊}\quad \text{between}\ \left.\begin{array}{l} 45 \text{ and } 135= \quad 19 \\ \left.\begin{array}{l} 135 \text{ and } 225=9 \\ 225 \text{ and } 315=4 \end{array}\right\} 13 \\ 315 \text{ and } \ 45= \quad 2 \end{array}\right\} 21.$$

The suspicion of some yet undiscovered law became strengthened by this further investigation; and it occurred to me to ascertain if any other heliocentric semicircles would mark the effect of such law more clearly. Let me be permitted to extract the concluding passage from the note as it is printed in the Royal Astronomical Society's Notices:—

"But if, instead of the semicircles 315° to 135° and 135° to 315°, we adopt those from 45° to 225° and 225° to 45°, we see that of the ascending nodes of thirty-four planets, twenty-eight are found in the first semicircle and only six in the second. Again, the semicircles that contain the greatest number of L. P.'s of planets are between 0° and 180°, or 10° and 190°. That which contains the greatest number of nodes is between 35° and 215°. In the first case there are twenty-six, and in the latter twenty-nine. The quadrant containing the largest number of L. P.'s of planets is that between 11° and 101°, of which there are sixteen. That containing the largest of nodes is from 35½° to 125½°, of which there are twenty."

At the present moment (January 1855) we have orbits, more or less accurate, of forty-one planets. It cannot be altogether uninteresting to pursue once more the traces of a law still unknown, if it have existence. Our position now stands thus—

$$\text{L. P.'s between}\ \left.\begin{array}{l} 45^\circ \text{ and } 135= \quad 16 \\ \left.\begin{array}{l} 135 \text{ and } 225= \ 6 \\ 225 \text{ and } 315= \ 6 \end{array}\right\} 12 \\ 315 \text{ and } \ 45= \quad 13 \end{array}\right\} 29.$$

$$\text{☊}\quad \text{between}\ \left.\begin{array}{l} 45 \text{ and } 135= \quad 19 \\ \left.\begin{array}{l} 135 \text{ and } 225=11 \\ 225 \text{ and } 315= \ 5 \end{array}\right\} 16 \\ 315 \text{ and } \ 45= \quad 5 \end{array}\right\} 24.$$

But, be it remembered, that in 1853 of the then known planets the greatest number of L. P.'s were found to be situated in the heliocentric semicircles 0° to 180° or 10° to 190°. At present we shall find the perihelia of thirty out of the forty-one planets in either of these semicircles. The greatest number of nodes were then (1853) between 35° and 315°=29; and 45° and 225°=28. At present, of forty planets there are thirty nodes in either of these heliocentric semicircles. These facts are at least very singular. I may tabulate them—

Of forty-one planets, L. P.'s between 0° and 180°=30
10 and 190=30
Of forty planets ☊ between 35 and 215=30
45 and 225=30
and between 354° to 355° and 174 to 175=31

We here perceive that there are thirty L. P.'s situated in the heliocentric semicircle between 0° to 10° and 180° to 190°. It is also the fact, that there are thirty ascending nodes between 357° to 7° and 177° to 187°, which may be called the same semicircle as that in which the thirty L. P.'s are found.

The quadrant containing the greatest number of L. P.'s of the forty-one planets, is that between 10° and 100°=20.

Those containing the greatest number of ascending nodes, are

between 36° to 43° and 126° to 133°=20
and between 62 to 66 and 152 to 156=20.

Surely there must be an undiscovered cause determining the orbits in this way. Having laid these facts before my first assistant Mr. Graham, he computed the degree of probability of such a law, arguing thus:—"Were the nodes and perihelia indifferent to all heliocentric longitudes, it would of course be an equal chance in the case of a planet whose orbit had not been determined, in which semicircle either would be found; and the *à priori* probability that, of the forty-one known L. P.'s, thirty would be in one semicircle, is about $\frac{1}{696}$; and that of the forty ascending nodes, thirty-one would be in one semicircle, is about $\frac{1}{4021}$. Thus the probability that there is some influence causing a tendency to one semicircle, ascertained from the facts before us, is very strong: for, for the L. P.'s, the odds are about 660 to 1, and for the ascending nodes about 4430 to 1 in favour of such a supposition." But after all it may be an accidental coincidence; as, consistently with the laws of planetary motion, such a congregation of perihelia or nodes may occur at periods exceedingly remote. The further consideration of this subject must be left to analysts, of leisure and inclination to pursue it.

LXVII. *Intelligence and Miscellaneous Articles.*

RESEARCHES UPON THE MAGNETIC POWER OF OXYGEN.

BY M. E. BECQUEREL.

IN a memoir read on the 21st of May, 1849, at the Academy of Sciences, relating to the action of magnets upon all bodies, I announced that oxygen is a magnetic body capable of being attracted by magnets, and that atmospheric air also possesses this property in consequence of the amount of oxygen contained in it.

The process employed to measure the action exerted by a magnet upon gases, as compared with that produced upon a body which is taken for unity, consisted in placing little sticks of wax, sulphur, glass, charcoal, &c. *in vacuo*, and in different gases, so as to ascertain the magnetic power of these gases from the difference in the effects observed under these two circumstances; it is susceptible of great precision. Amongst other results obtained, I may refer to the fact that the relation of the attraction exerted by a magnet upon oxygen, to the repulsion which takes place with the same volume of water, is in proportion to the density of the gas, and that it may be represented by 0·18 at a temperature of 53°·6 F.

Since the publication of these researches, Faraday[*] has given for the comparative action exerted upon oxygen and water, ascertained by another method, a number similar to mine. M. Matteucci[†] has also indicated a method by which he has succeeded in arriving at a number which differs but little; but M. Plücker[‡] has arrived at different results, by means of a balloon successively filled with oxygen and empty, in contact with the armatures of an electro-magnet, determining the weights necessary for the interruption of this contact. If we reflect that the earth is surrounded by a mass of air equivalent in weight to a stratum of mercury of 76 centimetres in thickness, it is easy to understand that such a mass, subject to incessant variations of temperature and pressure, must influence some of the phænomena dependent upon terrestrial magnetism. Thus, if we calculate the magnetic power of this gaseous mass, we shall find that it is equivalent to an immense iron plate of a little more than one-tenth of a millim. in thickness, which would cover the whole surface of the globe. I have thought, therefore, that it would not be uninteresting to examine afresh the action exerted upon oxygen, air, and gases at different temperatures and at different pressures, so as to determine their specific magnetism for different magnetic intensities.

As in my first researches I have referred all the determinations to distilled water, and have determined by means of my delicate balances the attractions and repulsions effected by a powerful electro-magnet, of rather less power than that formerly employed; I decided upon this method, because M. Plücker, having adopted an analogous process, arrived at numbers differing from those given by me; but I avoided all contact between the bodies submitted to the action of the magnets and the armatures, and I have reduced all the determinations to the same temperature and pressure.

In my previous experiments, I supposed in each case that the intensity of the action exerted upon the substances examined varied as the square of the intensity of the electrical current circulating round the electro-magnet; but in these new researches, the action exerted upon the bodies submitted to experiment being complicated by that exercised upon the envelope, its adjuncts, &c., I preferred the direct determination by experiment of the action exerted upon each body at different magnetic intensities, varying the number of couples employed from ten to sixty, and determining the intensity of the current in each case by means of a tangent galvanometer; it was then possible by a very simple interpolation formula to find the magnetic action exerted upon each substance, and at different determinate and constant intensities.

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[*] *Bibliothèque Universelle de Genève*, 1853, p. 112.

[†] *Comptes Rendus*, vol. xxxvi. p. 317.

[‡] *Annales de Physique et de Chimie*, vol. xxxiv. p. 342.

The results obtained lead to consequences of which the following are the principal:—

1. With an electro-magnet, of which the iron was 7 centimetres in diameter (instead of 11 centimetres, as in the previous experiments), the repulsive action exerted upon bismuth, water, &c. does not vary in proportion to the square of the intensity of the current which circulates round the electro-magnet, except just at the intensity corresponding with 15 to 20 Bunsen's couples; with a larger number of couples rising to 60, the relation between the action measured by the balance and the square of the intensity diminishes in proportion as this intensity becomes greater. The actions exerted upon different substances at different magnetic intensities lead, with some, to a specific magnetism, which varies with the magnetic intensity.

2. Between the limits of intensity of currents of from 10 to 60 elements, the specific magnetism of oxygen compared with that of water does not vary sensibly $\frac{1}{20}$th of its value. It is in the direct ratio of the density of the gas.

3. At 32° F., and a pressure of $0^{m}\cdot76$, the specific magnetism of oxygen, compared with that of water, is on the average +0·1823.

4. The action exercised by atmospheric air is measured by $\frac{21}{100}$dths of the action of oxygen under the same circumstances of temperature and pressure.

5. The following numbers have been obtained to represent the specific magnetism by volume of some solid and gaseous substances at 32° F. and a pressure of $0^{m}\cdot76$, the intensity of the current being comprised between 30 and 60 Bunsen's couples.

Substances.		Specific magnetisms.	Substances.	Specific magnetisms.
Water		−1	Water	−1
Oxygen		+0 1823	Copper (galvanic deposit)	−1·41
Deutoxide of nitrogen		+0·0498	Copper (pure)	−1·68
Air		+0·0383	Silver (pure)	−2·32
Chlorine	according to aqueous solutions.	−0·0046	Nativegold(massweighing 4813·5)	−2·41
Ammoniacal gas		−0·0020		
Sulphurous acid		−0·0005	Gold (pure)	−3·47
			Bismuth	−22·67

6. This method of experiment, which has enabled me to check that which I employed in my first investigations, but which nevertheless is less delicate than the latter, did not allow me to determine the diminution of the magnetic attraction which oxygen seems to undergo when its temperature is raised (the density remaining equal)*.—*Comptes Rendus*, April 16, 1854, p. 910.

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* From the abstract of M. E. Becquerel's researches here presented to us, the reader would be led to infer two things:—first, that the differential method of experiment is M. E. Becquerel's method; and secondly, that the discovery of the magnetic properties of oxygen is M. E. Becquerel's discovery. Both of these inferences would be contrary to fact. Nearly *four years* before M. E. Becquerel made use of the differential method, it was applied with success by Mr. Faraday (see Experimental Researches,

ON THE ALTERATION OF THE VOLUME OF SOME SUBSTANCES BY HEATING AND FUSION. BY HERMANN KOPP.

Previous investigations have unmistakeably shown that there is a certain connexion between the specific gravity and the chemical constitution of bodies. This applies particularly to fluid bodies, as with these the influence of temperature upon the volume of the substance could be easily ascertained. Certain rules also exist with respect to solid bodies, when their specific volumes are compared.

The author now publishes a series of experiments, the object of which was to ascertain the volume of one substance in a solid and fluid state; they also include the determination of the change of volume undergone by some substances when heated or fused. He gives these results as facts which may serve hereafter for the explanation of the question, when similar observations have been made upon a great number of bodies.

The author describes his method of investigation as follows:—For the determination of the expansion of solid bodies which are to be fused in the course of the experiment, a fluid is required the expansion of which is known. The methods of employing such a fluid are two,—namely, 1, *thermometric,* in which the operation is performed in a thermometer-like apparatus, enclosing a solid body together with a fluid of known expansion, in which case the expansion of the solid body is found by deducting that of the fluid from the whole; and 2, *gravimetric,* in which the specific gravity of the body is compared in the fluid and solid states. The author prefers the former method. The fluids employed were *water* for phosphorus, wax, stearine and stearic acid; *sulphuric acid* for sulphur; *olive oil* for chloride of calcium, phosphate of soda, hyposulphite of soda, and the readily fusible metallic alloys; and *oil of turpentine* for ice.

The results of the author's experiments are as follows:—It is rarely the case that a solid body exhibits the same coefficient of expansion when approaching its melting-point that it does when at some distance from it; the coefficient of expansion usually increases rapidly towards the melting-point. Amongst the substances here investigated, it is only with phosphorus, and according to other observers, ice, that the degree of expansion does not increase distinctly towards the melting-point.

The increase of volume exhibited by a melted body when compared with the same body at a lower temperature, arises mostly from the large expansion which the substance undergoes on approaching the melting-point whilst still in the solid state, and from its sudden expansion at the moment of fusion. In some bodies only one of these expansions is of particular importance: thus, for in-

vol. iii. p. 59, &c.); and nearly *two years* before the paper referred to by M. Becquerel was communicated to the Academy of Sciences, Mr. Faraday discovered the magnetic properties of oxygen. (See a letter addressed on this and kindred subjects to Mr. Richard Taylor, Phil. Mag. vol. xxxi. p. 401.) In a note bearing date the 28th of November, 1850, Mr. Faraday states his own claims with reference to this subject. (See Exp. Res. vol. iii. p. 219.)—ED. PHIL. MAG.

stance, with phosphorus the volume only increases at the moment of fusion, whilst with wax the increase at this moment is but small, but there is great expansion at the temperatures very closely approaching the melting-point.

Phosphorus expands uniformly up to its melting-point (111° F.), when its volume compared with that at 32° F. =1·017; it then, at the moment of fusion, exhibits a sudden augmentation of volume amounting to 3·4 per cent., so that at 111° F. its volume is =1·052.

Rhombic sulphur expands unequally, increasing rapidly in the vicinity of its melting-point (239° F.); if its volume be taken equal 1 at 32° F., it is =1·010 at 122° F., at 212° F. =1·037, and at 239° F. = 1·096; at the moment of fusion it increases 5 per cent., and then amounts to 1·150.

White wax increases in its expansion very rapidly as it approaches the melting-point (147° F.), but very little (about 0·4 per cent.) at the very moment of fusion; its volume compared with that at 32° F. is 1·068 at 122° F., 1·128 at 140° F., 1·161 at 147° F., and becomes 1·166 when fused.

Pure stearic acid expands less than wax before melting, but its volume increases about 11·0 per cent. at the moment of fusion; its volume at 32° F. being regarded as unity, it is 1·038 at 122° F., 1·054 at 140° F., 1·079 at 158° F., and becomes by fusion at the latter point 1·198. Impure stearic acid, melting at 136° F., expands rather more strongly on approaching its melting-point than pure acids at the same temperature.

Stearine, employed in the first modification, exhibits an increasing expansion up to the temperature at which it passes to the second modification; at this temperature it shows a considerable change of volume (amounting to $2\frac{1}{4}$ per cent.), but above this the volume increases rapidly in the vicinity of the melting-point, and at the moment of fusion there is an expansion of about 5·0 per cent. Stearine, with a melting-point of 140° F., shows a volume of 1·031 at 122° F., whilst still in the first modification; on passing to the second it becomes 1·008, and the volume then increases to the melting-point, when it is 1·076, becoming 1·129 when fused.

Water at the moment of freezing expands nearly 10 per cent. in volume. 1·1 volume of ice gave 1 volume of water at 32° F., which contracted when heated up to 39° F., at which temperature it occupied 0·99988; when heated further, it expands so that its volume at 212° F. amounts to 1·043.

Hydrated salts, on the contrary, expand at the moment of fusion. Chloride of calcium, $CaCl+6HO$, which expands from 32° F. to 68° F. in the proportion of 1 to 1·007, reaches 1·020 at 84° F.; at this temperature it melts, and its volume increases 9·6 per cent., so that in the fluid state it is 1·118. Phosphate of soda, $2NaO, HO, PO^5+24HO$, expands from 32° to 68° F. in the proportion 1 to 1·001; at 95° F. it reaches 1·005; at the moment of fusion at this temperature its volume increases 5·1 per cent., so that in the fluid state it is 1·056. Hyposulphite of soda, NaO, S^2O^2+5HO, expands from 32° to 68° F. from 1 vol. to 1·002; at 104° F. its volume is 1·005; and at 113° F., its melting-point, 1·007; at the moment of melting it increases 5·1 per cent., so that its volume at 113° F. in the fluid state is 1·058.

Rose's fusible alloy (composed of 2 parts of bismuth, 1 of lead, and 1 of tin) expands between 32° and 138° F. in the proportion 1 to 1·0027; when further heated, it contracts, so that its volume at 179°·6 F. is again equal to its volume at 32° F., and at 203° F. amounts only to 0·9947; it then expands again on fusion between 203° and 208° F. by 1·55 per cent., its volume at the latter temperature being 1·0101.—*Annalen der Chemie*, vol. xciii. p. 129.

RECENT APPOINTMENTS.

Prof. Graham's elevation to the post of Master of the Mint left vacant the office of Assayer to that establishment; we understand that the staff has been completed by the nomination of Dr. Hofmann, Chemist to the Museum of Practical Geology, to the vacant office. We have not heard whether Dr. Hofmann proposes to continue his duties in Jermyn Street. While there are so very few state occupations for men of science, it is scarcely desirable that the best of these should be gathered into single hands.—*Athenæum*, May 26.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1855.

Chiswick.—April 1. Clear and frosty: overcast. 2. Frosty and foggy: fine. 3. Cloudy: rain. 4. Cloudy and cold: clear and frosty. 5. Slight haze: very fine. 6. Fine. 7. Overcast: very fine. 8. Cloudy and cold: slight rain. 9. Cloudy. 10. Boisterous. 11. Cloudy and boisterous: rain at night. 12. Clear: very fine. 13. Overcast: slight rain: cloudy: fine. 14. Foggy: fine. 15. Overcast. 16. Foggy: cloudless and exceedingly fine. 17. Fine: clear. 18. Fine. 19. Slight haze: fine. 20, 21. Very fine: nights frosty. 22. Cold dry easterly wind. 23. Fine: sharp frost at night. 24. Clear and dry: overcast. 25. Densely overcast: cloudy and cold. 26. Overcast and cold: fine: overcast. 27. Dry haze: fine. 28. Hazy: fine: overcast. 29. Cloudy and cold. 30. Uniformly overcast: cloudy at night.

Mean temperature of the month	46°·08
Mean temperature of April 1854	47 ·53
Mean temperature of April for the last twenty-nine years	47 ·17
Average amount of rain in April	1·59 inch.

Boston.—April 1. Fine. 2. Cloudy. 3. Cloudy: rain P.M. 4. Cloudy. 5—7. Fine. 8. Cloudy: rain A.M. and P.M. 9. Rain: rain A.M.: stormy. 10. Fine: stormy. 11. Cloudy: rain P.M. 12. Fine. 13. Cloudy. 14. Fine. 15. Cloudy. 16—20. Fine. 21, 22. Cloudy. 23, 24. Fine. 25—30. Cloudy.

Sandwick Manse, Orkney.—April 1. Clear A.M. and P.M. 2. Bright A.M.: hazy P.M. 3. Bright A.M.: clear P.M. 4. Clear A.M.: cloudy P.M. 5. Bright A.M.: drops P.M. 6. Rain A.M.: clear, aurora P.M. 7. Showers A.M.: sleet-showers P.M. 8. Snow-showers A.M. and P.M. 9. Rain A.M.: showers P.M. 10. Showers A.M.: rain, clear P.M. 11. Cloudy A.M. and P.M. 12. Rain A.M.: clear, aurora P.M. 13. Bright, solar halo, A.M.: rain, aurora, P.M. 14, 15. Bright A.M.: rain P.M. 16. Sleet-showers A.M.: showers P.M. 17. Cloudy A.M.: cloudy, aurora P.M. 18. Rain A.M.: cloudy P.M. 19. Rain, bright A.M.: showers P.M. 20. Hail-showers A.M.: cloudy P.M. 21. Bright A.M.: clear, fine P.M. 22. Clear A.M.: clear, fine P.M. 23. Bright A.M.: cloudy P.M. 24. Hazy A.M.: cloudy P.M. 25. Damp A.M.: cloudy P.M. 26. Cloudy A.M.: clear P.M. 27. Cloudy A.M.: clear, fine P.M. 28. Bright A.M.: clear, fine, aurora P.M. 29. Clear A.M.: clear, fine P.M. 30. Clear A.M.: damp P.M.

Mean temperature of April for twenty-eight previous years	43°·48
Mean temperature of this month	43 ·20
Mean temperature of April 1854	44 ·68
Average quantity of rain in April for fourteen previous years	1·83 inch.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. April.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·382	30·221	30·00	30·23	30·15	48	22	36	40½	36½	ne.	n.	sse.	……	……	……
○ 2.	30·173	29·967	29·86	30·02	29·85	46	27	36	42½	37½	se.	s.	sse.	·02	……	……
3.	29·723	29·566	29·48	29·70	29·64	51	39	38	42	39	s.	ese.	ssw.	·08	……	……
4.	29·868	29·631	29·40	29·68	29·79	51	23	41·5	42½	41½	ne.	nne.	w.	·06	·10	……
5.	30·051	30·013	29·65	29·73	29·84	57	33	46	47½	45	w.	w.	w.	……	……	·03
6.	30·135	30·119	29·67	29·72	29·66	64	40	52	48	44	nw.	wsw.	w.	……	……	·17
7.	30·120	29·994	29·73	29·80	29·53	60	32	49	44	38	nw.	nw.	w.	……	……	·09
8.	30·050	29·999	29·63	29·61	29·65	53	38	45	39½	37½	nw.	nw.	wnw.	·01	……	·27
☾ 9.	29·811	29·404	29·35	29·11	28·76	58	39	47	46	39½	w.	sw.	w.	·01	·10	·45
10.	29·301	29·253	28·80	28·52	28·84	54	40	43·5	41	40	w.	w.	nnw.	·01	……	·30
11.	29·556	29·423	29·08	29·32	29·15	57	42	47	43	40	w.	nw.	e.	·05	……	·15
12.	29·572	29·512	29·09	29·03	29·38	60	46	48	42	38½	sw.	wnw.	nw.	·01	·03	·21
13.	29·607	29·540	29·20	29·49	29·53	57	39	52	44	37½	s.	ssw.	e.	·01	……	……
14.	30·002	29·844	29·47	29·57	29·36	62	40	47·5	43	41	w.	wnw.	s.	……	……	·25
15.	30·201	30·091	29·61	29·61	29·55	65	41	54·5	45½	48	w.	w.	sw.	……	……	·15
● 16.	30·267	30·219	29·79	29·82	30·09	71	41	53	45½	41	w.	wnw.	wnw.	……	……	·36
17.	30·343	30·322	29·97	30·32	30·26	65	32	48	46	41½	e.	ne.	s.	……	……	……
18.	30·342	30·224	29·98	30·00	29·98	66	23	43	46	45	e.	se.	w.	……	……	·29
19.	30·129	30·005	29·75	29·86	29·75	67	36	43·5	46½	43	ne.	se.	w.	……	……	·07
20.	30·339	30·162	29·77	30·09	30·36	67	27	47·5	40½	36½	n.	nw.	nw.	……	……	·10
21.	30·485	30·474	30·11	30·45	30·43	54	28	44	45	40	e.	n.	w.	……	……	……
22.	30·473	30·327	30·17	30·40	30·42	52	28	47	47	43	e.	ene.	s.	……	……	……
23.	30·503	30·422	30·14	30·43	30·37	60	22	43·5	49½	45½	ne.	nw.	w.	……	……	……
☽ 24.	30·321	30·174	29·92	30·30	30·30	69	40	47	48	46	n.	nnw.	nw.	……	……	……
25.	30·161	30·134	29·80	30·29	30·30	51	27	44	47	43	ne.	nne.	ne.	……	……	……
26.	30·230	30·212	29·87	30·24	30·18	57	31	43	45½	40	n.	ne.	se.	……	……	……
27.	30·222	30·091	29·83	30·12	30·12	60	28	46	49	44	n.	nnw.	wnw.	……	……	……
28.	30·063	30·046	29·74	30·18	30·27	57	36	48	49	43½	ne.	ene.	nw.	……	……	……
29.	30·200	30·182	29·86	30·30	30·30	54	41	46	49	44	n.	ne.	sw.	……	……	……
30.	30·222	30·162	29·92	30·32	30·35	53	38	42	47	41½	n.	ne.	n.	……	……	……
Mean.	30·095	29·991	29·6 8	29·875	29·872	58·20	33·96	45·6	45·05	41·36				0·26	0·23	2·89

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

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18.	30·342	30·224	29·98	30·00	29·98	66	23	43	46	45	e.	se.	w.	......	......	·29
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☽ 24.	30·321	30·174	29·92	30·30	30·30	69	40	47	48	46	n.	nnw.	nw.	......	......	......
25.	30·161	30·134	29·80	30·29	30·30	51	27	44	47	43	ne.	nne.	ne.	......	......	......
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SUPPLEMENT TO VOL. IX. FOURTH SERIES.

LXVIII. *On the Optical Properties developed in Transparent Bodies by the action of Magnetism.* *By* M. VERDET.

[With a Plate.]

PART I.*

I PROPOSE in this first investigation to measure anew the rotations of the plane of polarization which are observed when the direction of the luminous ray is parallel to that of the magnetic forces, and to examine the laws according to which these rotations depend on the distance and on the force of the magnetic centres which act on the transparent bodies.

Various difficulties present themselves on commencing this investigation. First, in the usual conditions of the experiments, *e. g.* when the transparent body to be examined is placed between the branches of the electro-magnet of M. Ruhmkorff, the various points of the body cannot be regarded as subject to equal magnetic influences. Consequently the optical properties developed by magnetism ought to vary from one point to another, and observation can only exhibit the result of a number of unequal actions. A body of finite dimensions cannot be compared to a body of infinitely small dimensions, which is the reverse of what takes place in ordinary optical researches; hence the elementary law of the phænomena, that is to say, the only law which it is of consequence to determine, cannot be immediately deduced from these observations.

Secondly, the optical properties developed in an elementary slice of the transparent substance depend themselves on a very complex combination of circumstances. The electro-magnet is, in fact, a system of magnetic centres whose distribution is not exactly known; the properties acquired by an elementary plate of the substance depend evidently on the distance of these centres, and on the quantities of free magnetism which are there accumulated; and the law to be determined is that which would be observed if the magnetic action emanated from a single centre whose distance and force could be made to vary. As, in order to exhibit the phænomena on a scale sufficiently large for observation, we are forced to place the transparent substance very near the electro-magnet, we cannot assume that the action is analogous to that of one or of two poles, according as the electro-magnet has one or two branches.

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* Abridged from the *Annales de Chimie et de Physique*, 3rd series, vol. xli.

We are ignorant also, in consequence of the complicated form of the apparatus, of the position of the poles in the electro-magnet. To remove the difficulties which arise from the inequality of the optical action of different plates of the same transparent body, I thought it would be sufficient to use an artifice employed by several authors, especially Mr. Faraday and M. Plücker, in the study of diamagnetic attractions and repulsions.

If we adapt to the two extremities of an electro-magnet two large armatures of soft iron, which present, opposite each other, two wide vertical faces, the space comprised between these vertical faces becomes, in Faraday's words, *a magnetic field of equal intensity*; *i. e.* a molecule of magnetic fluid* placed anywhere in this space, excepting in the vicinity of its extremities, is submitted to a system of actions whose resultant varies very little in magnitude and direction. In fact, the two terminal surfaces of the armatures are charged with the greatest part of the free magnetic fluids (as experiment shows), and these fluids are distributed almost uniformly, although with a tendency to accumulate towards the edges.

If we now conceive a magnetic molecule placed in the centre of this space, it would be submitted to a system of forces whose resultant would have a certain magnitude, and whose direction would be, on account of the symmetry, the right line which unites the centres of the two terminal surfaces, *i. e.* the right line generally called the line of the poles. If we conceive the magnetic molecule to deviate from this position by receding from one of the armatures, it would approach the other; by approaching the edge of one armature it would recede from the opposite edge. Consequently, according as certain actions became more intense or more inclined towards the line of the poles, others would become less intense or less inclined. We can conceive, then, that in a certain region there could be established a compensation almost complete, in such a manner that, as before mentioned, the resultant would remain almost constant in magnitude and direction. It is for experiment to show what case satisfies this condition.

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* It is scarcely necessary to say, that when I employ the expressions magnetic fluid and other similar ones, it is only for the sake of representing the phænomena more conveniently, and without committing myself to any particular theory of magnetism.

The accuracy of these observations is evidently in no respect subordinate to the particular law of magnetic action, which is the law of the square of the distance. Consequently, without knowing the law according to which the optical properties of transparent substances develope themselves under the influence of magnetism, since we know that this law involves a variation with the distance, we may presume that, in placing a transparent substance between two armatures similar to those already mentioned, compensations of the same kind will be established for the different points of the substance, and the several infinitely small plates of which we can conceive it formed, will all acquire optical properties sensibly identical. Experiment completely confirms this supposition.

I have succeeded, in fact, without difficulty in fulfilling the conditions in question by furnishing one of Ruhmkorff's electro-magnets with suitable armatures. This electro-magnet was formed of two cylinders of soft iron, AB, A′B′, $0^{m}\cdot20$ long and $0^{m}\cdot075$ in diameter, with a narrow canal bored in the direction of their axis to allow passage to the light; each cylinder was surrounded with about 250 metres of copper wire of 2·5 millims. diameter, and joined by the pieces of iron P and P′ (Plate V. fig. 1)*; these pieces are made to slide at pleasure along the piece of iron RS, and can be fixed in any position by means of the screws V, V′. On the two extremities B and A′ of the horizontal branches I screwed two cylinders of soft iron, F, F′, $0^{m}\cdot05$ in height, and $0^{m}\cdot14$ in diameter, bored along their axes by a narrow canal; and I found that when the distance between the terminal faces of these armatures was neither too great nor too small, when it amounted, for instance, to 50 or 90 millims., a transparent substance placed in the intermediate space acquired the same optical properties, whatever was its situation, provided it was not extremely near one or the other of the armatures. In fact, allowing a pencil of solar light to pass through the apparatus, I placed across the course of the pencil a parallelopiped of heavy glass, and having excited the magnetism of the electro-magnet, turned the birefracting prism which served as analyser until I perceived the violet tint known to physicists under the name of the *sensible tint* or *tint of passage*; I then displaced the parallelopiped of heavy glass parallel to itself, in order that the light might always pass through the same thickness; as long as I did not bring it in contact with one or the other armature, the *tint of passage* did not suffer any modification. The experiment succeeded as well with the other substances which I have used in my researches.

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* Fig. 1 being an elevation of the apparatus, the two cylinders of soft iron are of course concealed by the copper wire which covers them; the internal canal is merely indicated by the lines, and the axis of the canal by an interrupted dotted line MN.

Thus, in my apparatus, the space comprised between the two terminal armatures was so constituted, that a piece of transparent substance placed in any point of this space, excepting in the vicinity of the edges, was everywhere affected in the same manner. Hence the optical properties of the fragment were the same in all points of its mass, and consequently they were the same (excepting the magnitude of the rotation) as those of an infinitely small element. At the same time this space was what Faraday terms a magnetic field of equal intensity. In fact, by employing a method which will be indicated further on, I found that the resultant of the action which the electro-magnet would exert on a molecule of magnetic fluid situated in any point of this space, was sensibly constant in magnitude and direction; as long as the molecule was not very near the limits, *e. g.* in a series of experiments where the distances of the terminal faces of the armatures were successively 50, 60, and 90 millims., I measured the resultant before defined at the centre C of the intermediate space, fig. 1, and found the numbers 134·12, 116·33, and 86·17. I measured it at the point D situated on the axis, and 15 millims. distant from one of the armatures, and found the numbers 133·87, 116·75, and 86·00: I measured it at a point E 25 millims. from the axis and in the plane, and found the numbers 133·5, 116·00, and 85·5.

The differences of the values corresponding to the same position of the armatures are sufficiently small to allow the conclusion, that, in the interior of a region whose dimensions do not exceed those of the transparent bodies submitted to experiment, the resultant of the actions which would be exercised on a molecule of free magnetic fluid does not vary one-hundredth part of its value, which fraction about represents the degree of accuracy attainable in these experiments.

For the sake of brevity, the resultant before defined will be hereafter called *the magnetic action in a given point*.

This simultaneous constancy of the magnetic action and of the optical properties would naturally lead to a simple conjecture, which experiment has entirely confirmed, and whose confirmation removes the second difficulty which I before noticed. We might, in fact, ask if the magnitude of the rotatory power developed in a transparent substance did not depend solely upon the magnitude of the magnetic action considered in the space occupied by the substance. In fact, these two quantities are simultaneously constant and variable; besides, all the external actions of a magnet, its magnetizing action, its inductive action, its action on the element of a current, depend solely upon the action which it would exercise upon a molecule of magnetic fluid. It is then probable, that the action, in virtue of which it developes the rotatory power in transparent substances, depends solely upon the same quantity. If it be so, we need no longer trouble ourselves about the distribution of the free magnetism of the electro-magnet; no longer inquire separately into the influence of the variations of the intensity of magnetization, and the influence of the variations of distance. We shall measure, on the one hand, the optical action of the transparent substance, and on the other, a magnitude which at the same time represents in every point the effect of the variations of distance and the effect of the variations of intensity; we shall then have taken account at once of the influence of the two causes, and we can determine the elementary law of the phænomena without any of those arbitrary and inexact hypotheses to which we should have been necessarily conducted, if, for instance, we had wished to deduce the law relative to the influence of the distance in experiments where it would have been necessary to vary the distance of the transparent body from the extremity of one of the branches of the electro-magnet.

Experiment has, as we shall see, confirmed this supposition; and to obtain the elementary law which was sought, it has been sufficient to measure simultaneously the optical phænomena and the magnetic action.

The measurement of the optical phænomena has only been an application of methods known to all physicists, employed in the form which appeared to me most convenient for the peculiar conditions of my experiments.

A pencil of solar light, reflected by a heliostat into the dark chamber where I experimented, was polarized by passing through a Nichol's prism fixed at the entrance of the canal which passed through the electro-magnet in the direction of its axis. At the other extremity of the same canal the pencil of light encountered a very narrow diaphragm, and the portion which emerged fell upon the analysing apparatus placed at some distance. This apparatus, constructed by M. Brunner, was composed of a small telescope having an analysing prism placed before the object-glass, and capable of turning about its axis. The rotation of the telescope and of the analyser could be measured almost to a minute by the aid of a system of two concentric circles, of which the first, fixed to the support of the apparatus, was divided into degrees and thirds of a degree; and the other, moveable with the telescope, carried two opposed verniers giving the minute;

a screw allowed the telescope to be turned very slowly. The precision which it has been possible to give to the experiments has shown that none of those precautions were superfluous. The telescope was also capable of two rotatory movements, a vertical and a horizontal one, so that it was always possible to bring the axis into the direction of the pencil of light. The analyser was sometimes a birefracting prism of Iceland spar achromatized for the ordinary ray, sometimes one of Rochon's prisms. The telescope being adjusted so as to perceive clearly the image on the diaphragm, two images could be seen through the spar prism, and four through the Rochon prism, that is to say, two principal images, and two secondary images due to the imperfection of the construction. Having conveniently chosen the distance of the telescope and the diameter of the diaphragm, it could be so arranged that only the image whose variations were to be examined remained in the field of vision, an indispensable condition for the exactitude of the experiment. I took a diaphragm 3 millims. in diameter, and placed the lunette at a distance of $0^{m}\cdot80$.

I used, besides, two methods which have given me results perfectly accordant; sometimes I employed homogeneous light, and determined the position of the plane of polarization by observing the extinction of the extraordinary image; sometimes I have employed white light, and have had recourse to the observation of the tint of passage.

In experimenting on homogeneous light, I neither made use of red glass, which would have given me too small deviations*, nor of the monochromatic lamp, which would have furnished too feeble a light. I employed a solution of sulphate of copper in carbonate of ammonia, which, when its thickness amounts to some centimetres, only allows passage to the violet rays near to the ray G. If it be requisite that the light employed should have a sufficient intensity, the use of solar light is indispensable. Rotations of the plane of polarization almost twice as great as when operating with the tint of passage, are obtained by this mode of proceeding; but the estimation of the extinction is not always made with much exactitude, and depends in a singular manner upon the sensitiveness of the eye. I determined the two positions of the analyser which caused the image of the diaphragm to disappear by two movements in opposite directions; but as between the two disappearances it was necessary to illuminate the graduation in order to read off the position of the analyser, and since this circumstance might modify the sensibility of the eye, I did not take the mean of these two observations as corresponding to the position of the plane of polarization. I made at least four observations, oftener six or eight; these observations differed usually by ten minutes, but often the differences were as much as thirty minutes.

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* We know, in fact, that the rotation of the plane of polarization due to the magnetic action varies with the length of an undulation, almost in the same manner as the rotation produced by quartz and by organic liquids. It is hence the smallest possible for red rays.

The observation of the tint of passage has given me generally more accuracy than the observation of the extinction of homogeneous light, and I have employed it more frequently. In fact, although the tint of passage results from the extinction of the most intense rays of the spectrum, *i. e.* of the mean yellow rays whose plane of polarization is much less deflected than the plane of polarization of the indigo rays, yet this smallness of deflection is more than compensated by the exactness with which the eye appreciates the variations of colour in the neighbourhood of the tint of passage.

Again, I had to employ solar light in this case, in consequence of the conditions peculiar to the phænomena which I studied. The rotation of the planes of polarization of the different colours being always very small, their dispersion was very small also, and consequently when the analyser had a position convenient for completely extinguishing the mean yellow rays, it extinguished also in a great measure the other rays of the spectrum, so that the tint of passage was only produced by a very small part of the incident light. If, then, this light had not been extremely intense, it would have been impossible to perceive the tint of passage, and the eye would have only perceived so feeble a minimum of light that all coloration would have escaped detection. It is superfluous to add, that it would have been necessary to operate quite otherwise in order to measure large rotations, such as those produced by great thicknesses of quartz or organic liquids. In this case the use of solar light would have only dazzled the eye and rendered all observation of the tint of passage quite inexact. I always determined the azimuth of the tint of passage four times; twice setting out from the red, and twice from the violet. There was generally never more than five minutes' difference between these four observations. The mean could therefore be regarded as correct to two or three minutes.

As to the estimation of the magnetic action, it was only after numerous unfruitful attempts that I found a satisfactory process. The first idea was to cause a steel needle, strongly tempered, and magnetized to saturation, to oscillate in the intermediate space between the two armatures. The square of the number of oscillations in a given time would have served as a measure of the magnetic action.

This process would have been sufficiently exact if the actions to be measured had been feeble and incapable of altering the magnetic state of the needle; but the powerful electro-magnets necessary in my experiments would have considerably affected the magnetism of the needle, and the observations would not have been in any way comparable with one another. Neither the tempering of the needle, nor the magnetization to saturation, would have been a sufficient guarantee, since the magnetic state of any needle whatever undergoes, as is well known, a temporary change when it is brought near a strong magnet.

I tried, without much success, to use the action exerted by the electro-magnet on an unmagnetized body, magnetic or non-magnetic. Under the influence of an electro-magnet, a bar of a magnetic substance acquires a temporary magnetization; and if the body has no coercive force, it is admitted that this magnetization is proportional to the magnetic action. It follows hence, that the action exerted by the electro-magnet on the bar is proportional to the square of the magnetic action, and it only remains to measure it by known methods, *i. e.* by torsion or by oscillation. If the body under treatment be diamagnetic, although we do not know exactly in what manner the phænomena occur, it appears beyond doubt that a sort of polarity or temporary magnetization is developed, and consequently we admit that the action exerted by the electro-magnet is again proportional to the square of the magnetic action. Unfortunately in both cases the law in question is only an approximative one, sufficiently exact when it is required, for instance, to correct the effect due to the small variations on the power of an electro-magnet, but which cannot be the basis of a satisfactory process, destined to measure magnetic actions which vary between rather extended limits. In magnetic substances there exists always a sensible coercive force incompatible with so simple a law; in diamagnetic bodies no indications of the existence of a coercive force have yet been observed; but the only experiments we possess, in particular those of M. Edmond Becquerel, merely give an approximative law. I could therefore do nothing with this process, although I must admit that it might be useful in many cases: it might, for instance, be employed in confirming the constancy of the magnetic action in a given space, for this research is independent of the exact form of the law which represents the action of an electro-magnet on a magnetic or diamagnetic bar. In my experiments, on the contrary, the knowledge of this law would have been indispensable.

I thought of measuring the action of an electro-magnet on a small solenoid suspended between its branches. By using the bifilar mode of suspension employed by M. Wilhelm Weber in his electro-dynamic researches, this method would have offered no great difficulties. The arrangement of my apparatus, however, did not allow me to apply it; besides that, it would have been less simple and convenient than the following, which I ultimately employed.

This method is founded on a remarkable consequence that can be deduced from the laws of induction established by MM. Neumann and W. Weber. In his first memoir on the mathematical theory of induced currents*, M. Neumann has given a formula which represents the electromotive power developed by a magnetic pole in a closed conductor when displaced in any manner whatever. If the magnetic pole be regarded as the summit of a cone having for its base the closed conductor, the electromotive force developed by an infinitely small displacement of the current is proportional to the infinitely small variation in the angular opening of the cone, and consequently the sum of the electromotive forces developed by a finite displacement is proportional to the difference between the initial and final values of this angular opening†. I shall call this sum the *total electromotive force*. From this theorem the following conclusion may be deduced:—"*If in a space where the magnetic action is constant in magnitude and direction, a circular conductor is placed so that its plane be parallel with the direction of the magnetic action, and if it be made to turn* 90 *degrees round an axis perpendicular to this direction, the total electromotive force developed is exactly proportional to the magnitude of the magnetic action.*"

This conclusion would be evident if the magnetic action were simply due to one or two very distant poles. In order to demonstrate it on the general case, let us consider a plane conductor C, fig. 2, and a magnetic pole M, and let us suppose that the conductor suffers any displacement whatever which causes it to pass from the position C to the position C'. Let us call μ the quantity of magnetism accumulated at the point M, $d^2\omega$ the area of an infinitely small element o of the plane space surrounded by the conductor, r and r' the two successive distances oM, and o'M from this element to the point M, ϕ and ϕ' the angles made by the right lines oM and o'M with the normal to the conductor; the total electromotive force will be expressed by

$$\mu\left(\int\frac{d^2\omega}{r^2}\cos\phi-\int\frac{d^2\omega}{r'^2}\cos\phi'\right).$$

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* Memoirs of the Academy of Berlin for the year 1845.

† M. Neumann has not directly demonstrated by experiment the principles of his theory; he has deduced them from the law of Lenz. But we may regard the experiments of M. Weber, of M. Kirchhoff, and of M. Riccardo Felici, as having established the exactitude of the formulæ relative to the case of closed conductors, the only case with which we have here to occupy ourselves.

If the closed conductor be submitted to the action of any number of magnetic poles, the electromotive forces respectively developed by these different poles will be united, and their sum represented by

$$F=\Sigma\mu\left(\int\frac{d^2\omega}{r^2}\cos\phi-\int\frac{d^2\omega}{r'^2}\cos\phi'\right).$$

This equation can evidently be put in this form,

$$F=\int d^2\omega\left(\Sigma\frac{\mu\cos\phi}{r^2}-\Sigma\frac{\mu\cos\phi'}{r'^2}\right).$$

Or if we call R and R′ the resultants of the actions which the magnetic poles would exercise on the unit of magnetic fluid placed in o and in o', x and x' the angles made by these resultants with the normals to the conductor, we shall have

$$R\cos x=\Sigma\frac{\mu\cos\phi}{r^2},\quad R'\cos x'=\Sigma\frac{\mu^2\cos\phi'}{r'^2}.$$

If, as is supposed, the magnetic action is the same at all points of the conductor and of the space which it traverses when displaced, the two resultants R and R′ would be constant throughout the extent of the conductor, and equal to one another. Calling ω the total area of the conductor, we should have

$$F=\omega R(\cos a-\cos a').$$

If $a=0$, $a'=90°$, we have

$$F=\omega R;$$

i. e. if the plane of the closed conductor is at first perpendicular and then parallel to the magnetic action, the total electromotive force is proportional to the area of the conductor and this magnetic action itself.

During the displacement of the conductor, the induced current is at each instant proportional to the electromotive power developed in the conductor, and consequently a quantity of electricity proportional to this force passes through any section whatever of the wire. Hence it follows, that the total quantity of electricity which passes through any section of the wire during the whole duration of the movement is proportional to the total electromotor force. It is then proportional to the magnetic action in the case just considered. Now this total quantity of electricity is precisely the only thing relative to the induced current which we can measure by the aid of the galvanometer: it is often described, but falsely, under the name of intensity of the induced current; we shall designate it simply by the expression *induced current*; thus the proposition demonstrated above could be thus enunciated, under the conditions already defined, the induced current is proportional to the magnetic action.

Consequently, if we conceive a system of circular conductors constituting a helix whose dimensions do not exceed those of the space where the magnetic action is constant, a rotation of 90 degrees round an axis perpendicular to the direction of the magnetic action will develope an induced current proportional to the magnetic action. If the movement is made very rapidly, the induced current can be measured without difficulty by the galvanometer, and the magnetic action will thus be determined*.

I constructed, therefore, a small bobbin capable of turning about one of its diameters; and in order to give to the phænomena all possible intensity, the dimensions of wire around the bobbin were such that its resistance was almost equal to that of the wire of the galvanometer, of which we shall afterwards speak†. To this end, 23 metres of copper wire covered with silk, 0·5 millim. in diameter, were rolled so as to form a bobbin 28 millims. in external diameter, 12 millims. in internal diameter, and 15 millims. high. This bobbin was mounted upon a copper support, represented in fig. 3, which was fixed upon the electro-magnet in the middle between its two branches. By means of the button B, the bobbin C could be turned 90 degrees round the dotted line FG; and in consequence of the arrangement of the apparatus, the axis of rotation was perpendicular to the line of the poles, *i. e.* to the direction of the magnetic action. The transparent bodies submitted to experiment were placed on the piece of copper L, fixed on the support above the bobbin. The stem A could be raised or lowered by means of a rackwork regulated by the screw D, the transparent body or the bobbin could be brought at pleasure to the same point. The stem A could be retained in a fixed position by the screw H.

If the dimensions of the bobbin are given, by varying the diameter of the wire, we vary in inverse ratio the number of layers which can be arranged in the height and length of wire of which each layer is formed. Calling then d the diameter, we should have

$$n=\frac{k}{d};$$

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* M. Weber has founded upon the same principles a very remarkable method for comparing the two components of terrestrial magnetic action, in order to determine the value of the inclination. Mr. Faraday made use of analogous processes in studying the distribution of the magnetic forces round a magnet.

† This condition is easily deduced from the law of Ohm. In fact, let L be the resistance of the galvanometer, λ that of a layer of bobbin wires contained in the same plane, n the number of these layers, of the electromotor force, developed in one layer; the induced current would be expressed by $\frac{nf}{n\lambda+L}$.

and since the resistance of a layer of wires is proportional to the length, and in the inverse ratio of the square of the diameter, we should have

$$\lambda=\frac{h}{d^3}.$$

On the other hand, since the sum of the electromotor forces developed in a layer is proportional to the length of the wire, we may set $f=\frac{g}{d}$. By substituting these values, the preceding expression becomes

$$\frac{kgd^2}{kh+\mathrm{L}d^4},$$

and its maximum is given by the relation

$$kh-\mathrm{L}d^4=0.$$

As, however, $\frac{kh}{d^4}$ expresses the resistance of the bobbin, we see that this resistance ought to be equal to that of the galvanometer.

The extremities of the wire of the helix were connected with those of a galvanometer constructed according to the system of M. Wilhelm Weber. It is known that the object of the arrangements adopted by this physicist, is to allow the deviations of the needle to be reduced to a very small amplitude, the smallness of these deviations being compensated by exactitude of measurement. To this end, the needle of the galvanometer is suspended by means of a frame of copper to a vertical mirror placed above the frame of the galvanometer, and itself suspended at the lower extremity of a silk thread without torsion, which supports the whole apparatus. At some distance from the mirror, and almost in the same horizontal plane, is placed a scale divided into millimetres, above which is a small telescope; by means of the telescope the image of the scale reflected in the mirror is read off, and by conveniently removing the scale and the telescope, it may be arranged that the image of the division on the scale immediately under the axis of the lunette, coincides with the vertical cross-wire when the magnetized needle is in the position of equilibrium. The axis of the telescope is thus normal to the mirror; as it is also perpendicular to the divided rule, it is easy to see, that if the needle and the mirror, which are invariably connected, are deflected through any angle, the image of the scale seen in the telescope will suffer a displacement equal to the tangent of double the angle through which the mirror is to be turned, this tangent being drawn to a circle whose radius is equal to the distance from the mirror to the scale. Hence it is the same if the axis of the telescope be not perfectly normal to the mirror, provided the angle through which the whole system is turned be sufficiently small. By increasing the distance from the scale to the mirror, the precision of measurement is in some degree indefinitely increased, provided the telescope have a magnifying power sufficient to show the divisions clearly. In my experiments the most convenient distance appeared to me to be that of $1^{m}\cdot25$; an angular displacement of 80 seconds corresponded to each division of the graduated scale; and since a quarter of a division could be easily estimated, the accuracy of the measurement reached twenty seconds.

In my instrument, the magnetized needle was a large bar of steel, $0^{m}\cdot3$ long by $0^{m}\cdot015$ in diameter; the wire of the galvanometer was 100 metres long and 1 millim. in diameter, and was rolled on an oval frame of yellow copper, $0^{m}\cdot15$ in length and $0^{m}\cdot10$ in width. The metallic mirror was a square of $0^{m}\cdot04$ each side, and the cocoon thread without torsion was $0^{m}\cdot35$ long. In the interior of the frame of brass was another frame of red copper, of the same form but 1 centimetre thick, intended to damp the oscillations of the needle by the counter-action of the induced currents which the movement of the needle developes in its mass. In the construction of this apparatus I employed M. Ruhmkorff, who acquitted himself with his usual ability.

If along the wire of such a galvanometer a current of very short duration is passed, this current communicates to the needle an impulse proportional to the integral of the actions which it exercises during the successive instants of its duration, and consequently proportional to the total quantity of electricity which it causes to pass through any section whatever of the wire; this quantity is, as was seen above, the measure of the magnetic action exerted at the point where the helix of induction is situated. If no resistance was offered to the movement of the needle, it would perform oscillations entirely comparable to those of a pendulum *in vacuo*, and the sine of the semi-deviation would exactly measure the initial impulse. In reality the needle suffers various resistances, among which the most important is that which proceeds from the reaction of the induced currents developed in the copper frame and in the galvanometric wire itself; and the amplitude of its oscillations decreases so rapidly, that the previous relation does not take place. But if the decrease in the oscillations occurs in geometrical progression, and if the initial deviation do not exceed a certain limit, it may be demonstrated, according to M. Weber, that the initial displacement of the image of the scale is proportional to the initial impulse.

I assured myself by repeated trials that these conditions were satisfied in my galvanometer. I took, in consequence, as a measure of the induced current and of the magnetic action, the displacement of the image of the scale observed in the small telescope. By suppressing the elliptic frame of red copper, we should increase without doubt the amplitude of the elongations, but we should fall into the inconvenience which makes the use of galvanometers with a single needle, such as the sine and tangent boussoles, so restricted. The needle deviated by the impulse of a current, only rests again at the expiration of some minutes; the least accidental cause would communicate to it a motion which would be equally long in disappearing, so that the successive observations would necessarily be separated by a considerable interval. On the contrary, in M. Weber's apparatus, the influence of the induced currents in the copper frame weakens the oscillations of the needle, and fixes it in its position of equilibrium with a promptitude surprising to those who observe it for the first time. The effect of small accidental oscillations is destroyed almost immediately, and nothing hinders the observations from succeeding each other at very close intervals*.

It is now easy to perceive how each experiment was made. I commenced always by making two or three observations on the

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* This advantage is rendered much more sensible by means of an ingenious modification suggested by M. Ruhmkorff. This consists in cutting hollow the magnetized bar so as to diminish considerably its moment of inertia without sensibly altering its magnetic moment. It follows evidently, that the damping of the oscillations ought to be much more rapid; the formulæ developed in the preceding note give the mathematical expression of this difference, and experiment shows very evidently that it is correct. M. Ruhmkorff had joined two different magnetized bars to the galvanometer which he had constructed for my experiments; the one was solid, and the other hollow; both were magnetized to saturation, and had the same external dimensions. With the solid bar, the amplitudes of the oscillations decreased according to a geometric progression whose ratio was equal to 0·646; with the hollow bar the geometric progression had for ratio 0·477. The following are the two series of observations relative to the two bars: *n* is the number of the order of each elongation observed, first on one and then on the other side of the position of equilibrium; *a* the amplitude of the elongation; *r* the ratio of one elongation to the preceding.

Solid Bar.

n.	*a.*	*r.*
1	+ 179·0	
2	− 116·0	0·648
3	+ 75·0	0·646
4	− 48·5	0·646
5	+ 31·75	0·654
6	− 20·25	0·638
	Mean . .	0·646

Hollow Bar.

n.	*a.*	*r.*
1	+ 180·0	
2	− 83·5	0·475
3	+ 41·25	0·482
4	− 19·5	0·473
	Mean . .	0·477

Thus the movement of the hollow bar is damped in three oscillations as much as the movement of the solid bar in five oscillations. As, besides, the oscillations of the hollow bar are more rapid than those of the solid bar, the advantage of the hollow bar is made again much more sensible.

induced current by the rotation of the small bobbin; then raising, by means of a movement of the rack A, fig. 3, the transparent body into the place previously occupied by the bobbin, I turned the analyser so as to determine the azimuth of the tint of passage if I worked with white light, or the azimuth of extinction if I worked with homogeneous light, reversing the direction of the current, but taking care not to break the circuit*. I determined anew the same azimuth. The difference of the two observations evidently gave the double of the rotation of the plane of polarization, if the magnetic action had not perceptibly varied during the experiment. In order to assure myself, I measured the magnetic action immediately after the determination of the second azimuth, and only regarded those experiments as good where the two measurements of magnetic action did not differ in their mean value by more than a hundredth of that value. It is this mean value which I have set in the tables to be given further on. I perceived, like observers who have preceded me, that the magnetism of the electro-magnet takes a certain time to develope itself and attain its maximum. It is not then necessary to commence the experiments immediately after closing the voltaic pile; it is only after some time that the magnetism has taken a value which does not vary perceptibly during a complete experiment.

The galvanometer ought to be placed at a great distance from the electro-magnet, in order that it may not be affected by it; I was obliged to have an assistant to move the little bobbin while I observed the motions of the magnetized bar. I have not had recourse to any particular mechanical arrangement, experience having shown me, that, notwithstanding the little irregularities which movements effected by means of the hand always involve, the results of many consecutive observations agreed perfectly, provided the movement was sufficiently rapid. If by chance the movement was too slow, or if it was performed at different times, I perceived it by the movement of the needle, and I recommenced the experiment.

I have experimented solely on three substances,—the heavy glass of Faraday, common flint, and bisulphide of carbon; but these three substances differ so much from one another, that a law which suits them equally may be regarded as general. I had at my disposal two specimens of heavy glass; the first, which M. De la Rive was good enough to lend me, was a parallelopiped with square base, 40 millims. long by 18 millims. on the side; polished on its two bases and on two of the lateral faces; the second, which belonged to the collection of the Ecole Normale supérieure, was a rectangular parallelopiped polished on its six faces, whose edges were respectively equal to 37·2 millims., 26·0 millims., and 12·5 millims. I describe the two samples as No. 1 and No. 2. The specimen of flint was a parallelopiped of square base 43·3 millims. long by 14·5 millims. on the side, two of its bases, and a couple of its lateral faces, being polished. These three pieces were not entirely free from defect; but by elevating or lowering the support on which they were placed, and by making them slide on this support, the ray of light could always be made to traverse a space where the defect had no perceptible influence, so that by a convenient position of the analyser the incident light could be completely extinguished when the electro-magnet was not magnetized. The bisulphide of carbon was contained in small glass cylinders, closed by plates of ordinary glass; one of these cylinders was 44 millims., and another 31 millims. long. I satisfied myself beforehand that the rotation due to the terminal plates of glass was entirely imperceptible.

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* This precaution is useful, so as not to cause variations of intensity in the current.

The whole of the experiments establish a very simple law,—"*The rotation of the plane of polarization is proportional to the magnetic action.*" The following Tables, which only contain a part of my experiments, give the proof of this law. In these tables F is the magnetic action measured by the deviation directly observed in the galvanometer, R the double of the rotation, and Q the value of the quotient $\frac{R}{F}$ when we suppose R expressed in minutes. In each table the words *white light* or *homogeneous light* indicate the manner in which the optical observation is made; the thickness of the substance traversed by the luminous rays is also indicated.

Experiments on heavy Glass, No. 1.

I.[*]

White light. Thickness, 40 millims.

F.	R.	Q.
143·37	9° 13′ 45″	3·86
115·00	7 28 30	3·90
112·37	7 17 45	3·89
87·75	5 46 45	3·95
63·62	3 55 45	3·71
	Mean . .	3·86

II.[†]

Homogeneous indigo light. Thickness, 40 millims.

F.	R.	Q.
157·5	16° 36′	6·32
119·0	13 13	6·66
109·62	11 44	6·42
	Mean . .	6·46

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[*] The hundredths of division and the seconds that we shall see in these tables result from the calculation of the mean.

[†] In these two experiments, the distance from the divided rule to the mirror was not the same, so that the magnetic actions given in the Tables I. and II. are not measured with the same unity.

Experiments on heavy Glass, No. 2.

III.

White light. Thickness, 37·2 millims.

F.	R.	Q.
148·25	6° 05′ 15″	2·80
116·37	5 28	2·82
107·00	5 9 30	2·89
92·87	4 26	2·84
89·37	4 20	2·91
83·50	4 4 20	2·93
59·37	2 57 15	2·98
	Mean . .	2·88

IV.

White light. Thickness, 26 millims.

F.	R.	Q.
143·81	4° 31′	1·88
109·62	3 30 45	1·92
85·37	2 48	1·97
	Mean . .	1·93

Experiments with common Flint.

V.

White light. Thickness, 43·3 millims.

F.	R.	Q.
148·00	4° 40′ 45″	1·90
123·81	4 2 30	1·96
92·75	2 57 15	1·91
	Mean	1·92

Experiments with Bisulphide of Carbon.

VI.

White light. Thickness, 44 millims.

F.	R.	Q.
150·37	6° 16′ 15″	2·50
112·87	4 37 30	2·46
94·19	3 55	2·49
69·00	2 54	2·52
	Mean . .	2·49

VII.

White light. Thickness, 31 millims.

F.	R.	Q.
149·62	4° 19′ 30″	1·73
113·5	3 23	1·78
93·5	2 34 45	1·65
	Mean . .	1·72

VIII.

Homogeneous indigo light. Thickness, 44 millims.

F.	R.	Q.
148·5	10° 47′ ″	4·37
124·5	9 29 30	4·57
94·4	7 7 30	4·53
	Mean	4·49

We see from these tables that the magnetic action and the rotation may vary in the relation of 1 to 3, and still remain proportional. It is further to be remarked, that in each series of experiments the magnetic action is made to vary in two ways; by varying sometimes the intensity of the current, sometimes the distance of the branches of the electro-magnet. From 4 to 20 Bunsen's elements were employed, and the distance between the armatures was varied from 50 to 90 millims., so that the distance from each of the armatures to the middle of the transparent substance might vary in the relation of 1 to 2. In order to show that in both cases the law was the same, and also to give an idea of the agreement in the individual observations, of which the preceding tables only contain the mean, I shall give here the experiments I. and VI. in detail.

Experiment No. I.

Heavy glass, No. 1; thickness, 40 millims. White light was employed. Distance between the armatures, 60 millims. 20 Bunsen's elements.

The magnetic action was measured; the mean of two consecutive observations was	143·25
Azimuth of the tint of passage	182° 28′ 45″
Azimuth of the tint of passage after reversing the current	191° 42′ 30″
Second determination of the magnetic action	143·5

Double rotation, 9° 13′ 45″; magnetic action, 143·37; ratio, 3·86.

Experiment No. II.

Distance between the armatures, 80 millims. 20 Bunsen's elements.

Magnetic action	115
Azimuth of the tint of passage	183° 21
Azimuth after inverting the current	190° 49′ 30″
Magnetic action	115

Double rotation, 7° 28′ 30″; mean magnetic action, 115; ratio, 3·90.

Experiment No. III.

Distance between the armatures, 60 millims. 10 Bunsen's elements.

Magnetic action	112·75
Azimuth of the tint of passage	183° 24′ 45″
Azimuth after inverting the current	190° 42′ 30″
Magnetic action	112

Double rotation, 7° 17′ 45″; mean magnetic action, 112·37; ratio, 3·89.

Experiment No. IV.

Distance between the armatures, 80 millims. 10 Bunsen's elements.

Magnetic action	87·5
Azimuth of the tint of passage	190° 0′
Azimuth after inverting the current	184° 13′ 15″
Magnetic action	88·0

Double rotation, 5° 46′ 45″; mean magnetic action, 87·75; ratio, 3·95.

Experiment No. V.

Distance between the armatures, 90 millims. 10 Bunsen's elements.

Magnetic action	64·0
Azimuth of the tint of passage	189° 0′
Azimuth after inverting the current	185° 4′ 15″
Magnetic action	63·25

Double rotation, 3° 55′ 45″; mean magnetic action, 63·62; ratio, 3·71.

Experiment No. VI.

Bisulphide of carbon; thickness, 44 millims. White light was used. Distance between the armatures, 62 millims. 20 Bunsen's elements.

Magnetic action	151·0
Azimuth of the tint of passage	183° 53′ 45″
Azimuth after inverting the current	190° 10′
Magnetic action	149·75

Double rotation, 6° 16′ 15″; mean magnetic action, 150·37; ratio, 2·50.

Experiment No. VII.

Distance between the armatures, 90 millims. 20 Bunsen's elements.

Magnetic action	113
Azimuth of the tint of passage	184° 39′ 15″
Azimuth after inverting the current	189° 16′ 45″
Magnetic action	112·75

Double rotation, 4° 37′ 30″; mean magnetic action, 112·87; ratio, 2·46.

Experiment No. VIII.

Distance between the armatures, 62 millims. 6 Bunsen's elements.

Magnetic action	94·12
Azimuth of the tint of passage	185° 0′ 45″
Azimuth after inverting the current . . .	188° 55′ 45″
Magnetic action	94·25

Double rotation, 3° 55′; mean magnetic action, 94·19; ratio, 2·49.

Experiment No. IX.

Distance between the armatures, 90 millims. 6 Bunsen's elements.

Magnetic action	68·5
Azimuth of the tint of passage	185° 29′ 30″
Azimuth after inverting the current . . .	188° 23′ 30″
Magnetic action	69·5

Double rotation, 2° 54′; mean magnetic action, 69·0; ratio, 2·52.

Each of these observations ought to be exact, as I have before said, to two or three minutes, and consequently the measure of the rotations does not allow a greater error than five or six minutes; the probable error of the measurement of magnetic action is half a division of a scale. Although these errors are so small, we may rest satisfied that they are sufficient to account for the differences which exist between the several values of the ratio of the rotation to the magnetic action determined in the same series of observations.

It is important to notice, that the proportionality of the rotation and of the magnetic action is confirmed with the same exactitude whether the distance from the magnetic centres to the transparent substance is changed, or whether the quantity of free magnetism accumulated in these various centres suffers a variation. This law of proportionality is demonstrated by our experiments for transparent bodies of finite dimensions, all of whose parts are equally affected by the electro-magnet; it is consequently true for all the infinitely small plates of which we can conceive the transparent body to be composed. It follows, hence, that the formula can be stated in the following manner: —*The rotatory power developed by the action of a magnetic centre in an infinitely thin plate of a monorefracting substance varies proportionally to the magnetic action,* i. e. *directly as the quantity of magnetism accumulated in this centre, and inversely as the square of the distance.*

M. Wiedemann has demonstrated that the rotatory power developed by the direct action of electric currents is proportional to the intensity of these currents. He has made no experiments on the influence exercised by the relative situation of the electric currents and of the transparent bodies; but if my results are compared with those of M. Wiedemann, if further, the general identity of the properties of magnets and of systems of closed currents be taken into account, it will appear evident that the rotatory power developed by a system of closed currents in an infinitely thin plate of a transparent substance ought to be proportional to the action which the system would exercise on a molecule of magnetic fluid.

I find a complete contradiction to the law announced by M. Bertin, according to which the rotation of the plane of polarization due to the influence of a single magnetic pole will decrease in geometric progression when the distance from the transparent substance to the pole increases in arithmetical progression. The explanation of this disagreement is not difficult to give. M. Bertin considers as pole, the terminal surface of soft iron of one of the branches of Ruhmkorff's electro-magnet. Now this surface could not be regarded as pole, at least if to this expression be attributed its precise signification; it is a system of magnetic centres distributed over a great surface, and whose action cannot be assimilated to that of a single centre. We cannot then look for the elementary law which makes the rotation of the plane of polarization depend on the distance from the transparent substance to this polar surface; we can only find an empirical formula, which ought to change when the electro-magnet is changed, or even when we simply change the terminal armatures of the same electro-magnet. But if the law which I have established in this memoir be true, the empirical formula which represents the decrease of the rotations at different distances from the polar surface ought also to represent the decrease of the magnetic action, since these two quantities are always proportional; consequently, in the apparatus of M. Bertin, the magnetic actions ought to decrease in geometric progression when the distances from the polar surface increase in arithmetical progression.

Direct experiments have completely confirmed this conclusion. The apparatus which I used, and which belongs to the Ecole Normale supérieure, was that which M. Bertin also used in his researches. I took away one of the branches of the electro-magnet, and at the extremity of the branch that I retained, I screwed instead of the large armature of my experiments, the small hexagonal armature which M. Ruhmkorff generally uses in his apparatus, and which M. Bertin himself used. I determined by the method before indicated, the magnitude of the magnetic action at different distances from the terminal surface of this armature*, and observed a very slow decrease, which may fitly be represented by a decreasing geometrical progression, although it may be in reality a little less rapid.

This may be seen in the following Table:—

Distance from the polar surface.	Magnetic action†.	Ratio of each magnetic action to the preceding.
mm 20	192·25	
30	146·00	0·76
40	113·75	0·78
50	91·00	0·80
60	73·75	0·81
70	61·25	0·83
	Mean . . .	0·796

I repeated the experiment after an interval of two months, during which time the electro-magnet had been very frequently put in use, and I obtained again the same law, as the table shows.

Distance to the polar surface.	Magnetic action‡.	Relation of each magnetic action to the preceding.
mm 25	129·0	
35	97·0	0·75
45	76·0	0·78
55	60·9	0·80
65	49·7	0·81
	Mean . . .	0·785

Thus, by the effect of time and of use, the electro-magnet did not appear to be modified, so that the law of its action on an external point was sensibly changed. We may then be permitted to believe, that when M. Bertin made his experiments in 1847 and 1848, if he had measured the magnetic actions exercised at different distances, he would have obtained results perfectly similar to the preceding; he would therefore have been able to represent these magnetic actions by a decreasing geometric progression, whose ratio would have been very little different from the mean of the two preceding determinations, *i. e.* of 0·790. Now we find in the memoir of M. Bertin* five series of experiments relative to the decrease in the rotations observed at different distances from a single branch of the electro-magnet. The two first are relative to Faraday's heavy glass, and may be represented by two decreasing geometric progressions whose ratios are respectively 0·78329 and 0·78330 for 10 millims. of increase of distance; the third is relative to a flint prepared by M. Matthiessen, and is represented by a decreasing geometrical progression whose ratio is 0·78233; the fourth is relative to the bisulphide of carbon, and is represented by a decreasing geometric progression whose ratio is 0·78329; lastly, the fifth relates to the heavy glass of Faraday, and is represented by a decreasing geometrical progression whose ratio is 0·78329. The mean of these five geometrical progressions is 0·78318, and differs very little from 0·790. Thus the experiments of M. Bertin agree entirely with the law which they seem to contradict.

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* This method only gives the value of the magnetic action exactly when this action is constant in the space where the inductive bobbin moves; but if the values of the magnetic action at different points of this space are slightly different, it is clear that the methods determine practically the mean value.

† The magnetic actions inserted in this column have been determined by the method of alternatives, in order to eliminate the influence of small variations of the intensity of the current. The pile was composed of 10 Bunsen's elements.

‡ See preceding note.

In order to show well the influence which the form of the terminal surfaces of the electro-magnet exercises upon the law of the decrease of magnetic action, I repeated the same series of experiments by screwing one of my large armatures to the extremity of the branch of the electro-magnet which I employed. I obtained a law of decrease much more slow than in the preceding case, which may again be represented by a decreasing geometrical progression. On the contrary, when I replaced the large armature by a cone of soft iron 24 millims. high, with a diameter at the base of 45 millims., the decrease was much more rapid, and entirely different from that which would have indicated a decreasing geometrical progression. The following Tables contain the results of these experiments:—

Large armature.

Distance to the polar surface.	Magnetic action.	Ratio of each magnetic action to the preceding.
mm 22	77·00	
32	73·75	0·96
42	67·87	0·92
52	61·75	0·91
62	55·50	0·90
	Mean . . .	0·925

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* *Annales de Chimie et de Physique,* 3 sér. vol. xxiii. pp. 22, 23 and 27.

Conical armature.

Distance from the summit of the cone.	Magnetic action.	Ratio of each magnetic action to the preceding.
mm 25	137·0	
35	98·0	0·71
45	74·5	0·76
55	61·75	0·83

Lastly, the experiments No. III. and No. IV., and the experiments No. VI. and No. VII., furnish a confirmation which ought not to be neglected. In the experiments No. III. and No. IV., I measured the rotations produced by two different thicknesses of the same piece of heavy glass; if these measurements corresponded to equal magnetic actions, the rotations ought to be proportional to the thicknesses, in virtue of the identity of the action of all the plates of the substance. In reality the magnetic actions were not the same in the two experiments; but it is clear, that if the experiments had been well made, the ratios of the rotations to the magnetic actions ought to be proportional to the thicknesses. Now these relations are respectively equal to 2·88 and 1·92; by dividing them by the corresponding thicknesses 37·2 and 26, we obtain the quotient 0·077 and 0·074, that is to say, numbers whose differences do not exceed the errors of experiment. The experiments No. VI. and No. VII. relative to the bisulphide of carbon, lead to the same conclusion. The ratios of the rotations to the magnetic actions in the two experiments are 2·49 and 1·72; by dividing them by the corresponding thicknesses 44 and 31, the quotients 0·056 and 0·055* are obtained.

PART II†.

In publishing his discovery of the rotation of the plane of polarization produced by the influence of magnetism, Mr. Faraday stated that the phænomenon manifested itself with the greatest intensity when the direction of the ray of light was parallel to the direction of the magnetic forces, and that it disappeared when these two directions were perpendicular to one another; but he said nothing of the manner in which the passage from one of these extremes to the other is effected. Subsequent observers have confirmed these two results, but have added nothing to them. M. Bertin made some experiments by placing a piece of flint or heavy glass upon the base of a cylindrical electro-magnet at different distances from the axis; in this manner he observed that the rotations of the plane of polarization varied with the position of the transparent substance, but he discovered no law, and it is, in fact, evident that with the complex circumstances under which he experimented, the elementary law of these phænomena could not be discovered. M. Pouillet, M. Edmond Becquerel, and M. Wiedemann in all their experiments have directed the rays of light parallel to the action of the magnetic force.

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* It would not have been possible directly to compare the rotations produced by two different thicknesses under the influence of the same magnetic action, at least in the case of heavy glass. The solar light in traversing the glass heats it perceptibly, and the glass acquires a perceptible birefracting power in every direction perpendicular to that of the ray of light. Any observation with a thickness perpendicular to the first thickness studied is therefore impossible, as long as this birefracting power has not disappeared, and this disappearance often requires more than an hour.

† From the *Annales de Chimie et de Physique*, vol. xliii. for January 1855.

In this second part of my investigation I propose to complete our knowledge of the phænomenon discovered by Mr. Faraday, by determining, in a general manner, what takes place when the angle formed by the direction of the rays of light with the direction of magnetic action varies from 0° to 90°. It will not be necessary to insist upon the inherent interest of this research; it is evident that it must precede all discussion of the theories which have been, or may be proposed to explain these phænomena.

The apparatus usually employed is not well suited to experiments of this kind; in them the rays of light must necessarily have the same direction as that of magnetic action. Ruhmkorff's apparatus is an example of this; it was, in fact, constructed with a view to give the greatest possible intensity to the phænomena when the directions above mentioned were coincident. Nevertheless, at first I endeavoured to make use of Ruhmkorff's apparatus by giving a variable direction to the luminous rays by means of two successive reflexions from plane mirrors parallel to one another. A few experiments were sufficient to show that this modification of the apparatus was not adapted to give results worthy of confidence. On this account I experimented in a manner similar to Mr. Faraday, by placing the transparent substance so as to cause the rays of light to pass a little on one side, or a little above the polar extremities of the electro-magnet: as in my former experiments, however, the apparatus was arranged so that the space occupied by the transparent body was a *field of uniform magnetic intensity*.

For this purpose I procured a strong horse-shoe electro-magnet composed of two cylinders of soft iron, each 0·20 metre high and 0·075 metre in diameter, and each surrounded by 250 metres of copper wire 2·5 millimetres in diameter; the two were fixed to the extremities of an iron bar AB 0^{m}·35 long, 0^{m}·07 broad, and

$0^{m}\cdot015$ thick, fig. 4, Plate V*. The apparatus was mounted upon a copper pedestal which rested upon four screws V, V′, V″, V‴, so that it could turn around a vertical axis passing through the middle of the transverse bar AB; the vernier C, moving upon the surface of the graduated horizontal circle DE, indicated the horizontal displacement of the system. On the upper horizontal faces of the vertical branches two armatures of soft iron were fixed, whose disposition will be easily understood from the figure. Two prismatic pieces F and F′ glided along two grooves G and G′, and could be fixed in any desired position by means of the screws U and U′. At their extremities these two pieces carried two plates of soft iron HK and H′K′, each $0^{m}\cdot016$ long, $0^{m}\cdot04$ broad, and $0^{m}\cdot005$ thick. The two edges of these plates were exactly parallel, and, when they were withdrawn from each other to a convenient distance, it was easy to show by the method indicated in the preceding memoir, that the magnetic action was sensibly the same at all points of the rectangular space HK H′K′, as well as at a small distance above and below the same; between the same limits the optic action was also invariable. In the middle of the space between the two branches of the electro-magnet was a vertical copper bar which reached almost to the level of the upper surfaces of the armatures, and carried at its extremity a horizontal plate L, graduated at the circumference. Above this plate was a second plate O, which turned on a vertical axis coincident with the axis of rotation of the electro-magnet, and was provided with a vernier which indicated tenths of a degree. Upon this second plate, and resting on a small ledge R, the transparent substance was placed. A vertical rod fixed to the side of the transverse bar AB carried a pointer S at the same height as the graduation upon the plate L. Two other vertical rods, fixed to the foot of the apparatus and independent of the electro-magnet, carried two blackened screens 15 centimetres in diameter. At the centre of one of these screens was a birefracting prism T, which polarized the incident light; in the centre of the other screen an aperture 3 millimetres in diameter was pierced, merely to allow a narrow cylindrical pencil of rays to pass through†. The analysing apparatus, which consisted of the small *lunette* used in my former investigations, was completely independent, and placed at some distance.

The experiments were conducted as follows:—A ray of solar light, which was reflected by a heliostat and polarized in passing through the birefracting prism T, was made to traverse the transparent substance, and afterwards analysed beyond the screen. The direction of magnetic action was first brought into parallelism with the direction of the rays of light, by making the zero of the vernier C coincide with the zero of the graduated scale beneath it; the transparent substance was next placed so that the faces of entrance and exit were perpendicular to the same direction: this was done by making the zero of the vernier on the plate O coincide with the zero of the corresponding graduation; and lastly, the optic phænomenon was observed. Afterwards, the electro-magnet was turned through any angle whatever; this of course caused the transparent substance to turn through an exactly equal angle, but it was brought back to its primitive position by an opposite and exactly equal rotation of the plate O; the luminous rays, however, preserved an invariable direction, and consequently they always traversed the same thickness of transparent substance. The apparatus being thus disposed, the optical phænomenon was observed, care being taken to repeat each experiment twice by turning the electro-magnet successively towards the right and towards the left; afterwards, the mean of these two results was taken in order to eliminate the possible influence of imperfections in the construction and arrangement of the several parts of the apparatus. With small angular displacements of the electro-magnet, these two results were not sensibly different from one another; when the angular displacement attained or exceeded 45 degrees, the difference between the two results was greater than the usual errors of observation.

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* The figure represents a view looking down upon the apparatus. The ray of light MN and the direction of magnetic action PQ make an angle of 30°.

† The apparatus was constructed by M. Ruhmkorff.

In the first place, it was easy to corroborate the fact that the optical phænomenon is, in all cases, simply a rotation of the plane of polarization. By using homogeneous light, and first placing the analyser so as completely to extinguish one of the two images of the diaphragm, I have witnessed the reappearance of this image by the influence of magnetism; but whatever might have been the relative situations of the electro-magnet and of the transparent substance, I was always able to extinguish this new image by a convenient rotation of the analyser. By using white light I have seen the image reappear with variable colours, and by changing the position of the analyser, I have always found the tints to succeed each other in the order peculiar to these phænomena of rotation, whatever might have been the arrangement of the apparatus. In common with my predecessors, I have always observed that no effect was produced when the magnetic action was perpendicular to the direction of the rays of light.

Thus these phænomena manifest themselves in a less complicated manner than might have been expected; when the direction of the rays of polarized light is parallel to that of magnetic action, there is a certain rotation of the plane of polarization produced; when these two rotations are perpendicular to one another, the rotation is zero; when the angle enclosed by both directions varies from 0° to 90°, the rotation decreases in a continuous manner. This general fact once established, all that remains is to determine, by exact measurements, the law of the decrease of the rotations. The apparatus and optical methods described in my preceding memoir remain, therefore, entirely applicable. For reasons stated in another place, I have always preferred the use of white light and the observation of the tint of passage, to the use of homogeneous light.

The measurement of the phænomena have again led me to a very simple law. *The rotation of the plane of polarization is proportional to the cosine of the angle enclosed between the direction of the luminous rays and that of magnetic action*; consequently it is proportional to the component of magnetic action parallel to the direction of the luminous rays.

I have experimented on the same substances as in my former researches, *i. e.* on *heavy glass*, flint, and bisulphide of carbon. The bisulphide of carbon was always enclosed in the same small vessels described in the first part of this memoir; the specimen of flint was the same as that I had already used, but besides the two specimens of heavy glass, of which I have before given the dimensions, I was enabled, through the kindness of Mr. Faraday, to add a third of somewhat smaller size.

In the following tables I shall merely record the results of two experiments, both of which were made by the method of observing the tint of passage; in these tables α represents the angle enclosed by the rays of light and the direction of magnetic action, R and R′ the *complete** rotations corresponding, respectively, to positions on the right and on the left of the luminous rays; M the mean of these rotations; Q the ratio of this mean, expressed in minutes, to the cosine of the angle α; M_1 the calculated values of M, supposing the law of the cosine to be exact, and adopting, for the ratio of the rotation to the cosine of the angle α, the mean of the values of Q; and Δ the difference between M and M′†.

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* I repeat that by this term, the difference between the two azimuths of the tint of passage, corresponding to two opposite directions of the electric current, is intended. As in my former researches, each of these azimuths was determined from four separate observations made at intervals of five or six minutes at most.

† In order to avoid the effect of variations in the electric current, I have adopted the method of alternatives; the rotation corresponding to $x=0$ being always taken as a term of comparison; the values in the table are those which would have been observed had the rotation corresponding to $\alpha=0$ remained constant. I have calculated them by means of a proportion.

Experiments on Heavy Glass. Thickness 40 millims.

α.	R.	R′.	M.	Q.	M_1.	Δ.
0°	° ′ ″	° ′ ″	8° 55′ 45″	535·75	8° 55′ 45″	0° 0′ 0″
15	8 26 30	8 32 0	8 29 15	527·25	8 37 50	−0 8 35
30	7 38 0	7 42 0	7 40 0	531·25	7 44 0	−0 4 0
45	6 17 45	6 22 15	6 20 0	537·50	6 18 45	+0 1 15
60	4 22 30	4 35 0	4 28 45	537·50	4 27 45	+0 1 0
75	2 9 30	2 29 30	2 19 30	539·00	2 18 45	+0 0 45
Mean.	....	....	....	535·65		

Experiments on Bisulphide of Carbon. Thickness 44 millims.

α.	R.	R′.	M.	Q.	M_1.	Δ.
0°	° ′ ″	° ′ ″	5° 58′ 0″	358·0	5° 56′ 0″	+0° 2′ 0″
15	5 44 0	5 47 30	5 45 45	357·25	5 44 0	+0 1 45
30	5 5 2	5 10 30	5 7 45	355·25	5 8 15	−0 0 30
45	4 62 0	4 16 0	4 9 0	352·0	4 11 45	−0 2 45
60	2 52 0	3 5 30	2 58 45	357·50	2 58 0	+0 0 45
Mean.	....	....	....	356·00		

The law just established explains a circumstance which I had occasion to notice in my former researches; when the ray of light is parallel to the direction of magnetic action, the adjustment of the apparatus may be deranged to a sensible extent, and consequently the angle α made to vary from 3 to 4 degrees, without altering the rotation of the plane of polarization to any appreciable extent.

If we adopt the theoretical notions of Fresnel with respect to the rotation of the plane of polarization, we must imagine the polarized ray, falling perpendicularly upon the transparent substance which is submitted to the influence of magnetism, transformed into two rays circularly polarized, and in opposite directions, and propagated with unequal velocities. If the velocities of propagation be represented by v and v', it follows from the law above enunciated, that the expression $\frac{1}{v} - \frac{1}{v'}$ varies proportionally to the cosine of the angle enclosed between the direction of the luminous rays and that of magnetic action.

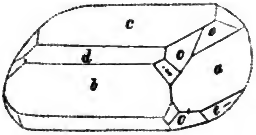
I shall confine myself to this theoretical remark, and for the present abstain from any further reflections on the law I have just established.

LXIX. *On the Crystalline Form of Leucophane.*
By R. P. Greg, *Esq.*[*]

MINERALOGISTS have not hitherto satisfactorily determined the crystalline form of *leucophane,* from the want probably of good crystals. In the last edition (fourth) of Dana's 'Mineralogy' it is described as *prismatic,* with cleavages in three directions inclined to each other at angles of 53½° and 36½°; while in Brooke and Miller's late edition of 'Phillips,' *leucophane* is stated as belonging to the *anorthic* system.

As this mineral, from its chemical composition, locality, and associated minerals, forms an interesting and important species, I propose briefly to describe a large crystal in my possession, and which I believe is the only tolerably perfect crystal of *leucophane* yet obtained. The accompanying figure is drawn so as to represent the most perfect portion of the crystal, the dotted lines indicating those faces or edges which are imperfect or broken in the crystal itself.

	Greg.	Miller.
	° ′	° ′
a : *c*	90 0	89 54 89 28
c : *b*	90 0	89 51 89 11
a : *b*	90 0 93 0	91 3
c : *d*	118 30	119 7 117 57
c : *e*	117 0	117 38 117 57
a : *d*	90 0	
b : *o*	101 30	100 9
c : *o*	140 30	
a : *o*	126 30	
a : *e*	151 0	151 50
b : *d*	154 0	153 48
e : *o*	153 0	
d : *o*	122 0	
d : *i*	147 0	
o : *i*	157 0	
b : *i*	124 0	
a : *i*	128 30	
c : *i*	126 0	



The measurements given by Professor Miller were made with a reflective goniometer constructed for measuring the angles of large crystals; those given by myself were taken with the common goniometer, for applying which the crystal is well adapted. From the nature of the crystal it is not easy to determine all its forms, or ascertain precisely to what system they lead; but the discovery by Descloiseaux and Brooke, that leucophane shows distinctly two systems of rings* when examined by polarized light in a direction perpendicular to the face of principal cleavage (*c*), renders the reading of this crystal a matter of less uncertainty; and I am inclined to consider that leucophane belongs to the prismatic system, having a right rectangular prism for its primitive form. This view supposes that $ab=90°$, as well as *cb* and *ac*.

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[*] Communicated by the Author.

I have observed three cleavages of leucophane. One parallel to *c* is the most readily obtained; a second is apparently parallel to *i*; and the third, *t*, truncates the edge between *a* and *b*, at an angle of about 135° with each of those faces. The three cleavage planes are in one zone, *ci* being $= 126° 25'$, and $ct=90°$.

LXX. *On an Analysis of the Potash and Soda Felspars of one of the Dublin Mountains.* *By* Francis M. Jennings, *M.R.I.A., F.G.S.*†

At the January meeting of the Geological Society, Dublin, 1855, a discussion took place on a paper, by the Rev. Joseph Galbraith, Professor of Natural and Experimental Philosophy in the University of Dublin, entitled "The Composition of the Felspars of the Granite of the Dublin and Wicklow Mountains," which appeared in the January Number of the Philosophical Magazine.

I regret much that the reverend essayist was not present, as all those who spoke, except myself, appeared to agree in the opinion expressed in the paper, namely that the granite of the Dublin and Wicklow Mountains was a potash granite, the analyses of the felspars by the author appearing conclusive to them.

I expressed doubts, not as to the correctness of the analyses, but as to the opinions of those who, from the analyses of the felspars, drew such inferences. It appeared to me a great defect in a paper drawing such important conclusions, that portions of the specimens which were analysed were not exhibited to the meeting, for it to judge if they were selected with that required discretion and care that would justify him in drawing from them the conclusions he had come to.

To ascertain the relative proportions of potash and soda of the felspar of those mountains, and thence to infer the composition of the granite, I consider it necessary that the specimens to be analysed should be selected so as to represent as closely as possible, in lithological character, the mass of rock or mountains the composition of which is to be inferred from such analyses; and if they do not fulfil these conditions, I consider them worthless as far as inferring from them the constitution of the mass of the mountain.

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* According to M. Descloiseaux, exactly resembling those exhibited by topaz.

† Communicated by the Author; having been read at the meeting of the Geological Society of Dublin, May 9, 1855.

As the opinion of those members who spoke were all against me, I therefore resolved, as soon as possible, to test by analysis, from one of those localities given by the essayist, the correctness of my views; and learning that the "Three Rock Mountain" represented in general lithological characters most nearly the granite of the Dublin and Wicklow Mountains (being too much occupied myself), I got a skilful professional chemist of high character in London, Mr. Dugald Campbell, of Quality Court, Chancery Lane, to analyse for potash and soda a portion of the granite of the "Three Rock Mountain," and took care that the specimen should be as similar as possible in general character to the granite of those mountains. Mr. Walter Wilson, of the Geological Survey, who is thoroughly conversant with the district, undertook very kindly to procure for me as fair an average specimen as he could obtain, and which accompanies this paper, *minus* that portion used for analysis.

From this analysis it will be perceived that a great difference exists between it and the analysis of the specimens of felspar from the same mountain by Professor Galbraith, as far as regards potash and soda.

Analysis by Professor Galbraith.

	Per cent.	Atomic quotients.		Proportionals.
SiO^3	65·40	1·444		4·19
Al^2O^3	17·71	0·344		1·00
MgO	1·77	0·088	0·420	1·22
CaO	trace	0·000		
KO	10·68	0·227		
NaO	3·26	0·105		
Loss by ignition	0·69			
	99·51			

Specific gravity =2·562.

Analysis of the amount of Potash and Soda by Mr. Dugald Campbell.

KO	3·23 per cent.
NaO	3·75 ...

There is in this analysis of the felspar about 4 per cent. of mica, which of course increases the relative amount of potash belonging to the felspar.

Now assuming, what I have no doubt to be the case, that both the analyses are correct, how is the difference to be accounted for? Either that there is in the felspar of the mountains a great diversity of constitution, or that the felspar selected by Professor Galbraith was not similar to that composing the general mass of the mountain.

To enable him and the Society to judge as to the specimens from which the analysis was made by Mr. Campbell, it accompanies this paper; and any part not exceeding one-half is at the disposal of the Professor if he wishes to examine it.

I have since received the following letter:—

"London, May 10, 1855.

"Estimation of the potash and soda in a specimen of granite received from F. M. Jennings, Esq., of Cork, on March 16, 1855.

"In my first experiments, before reducing the mineral to a fine powder, I only removed the larger pieces of mica, and the residue gave,—

Potash . . .	3·23 per cent.
Soda	3·75 ...

"In a second set of experiments just finished the mica was carefully picked out, scarcely a trace being left, and the residue gave,—

Potash . . .	3·02 per cent.
Soda	3·98 ...

Dugald Campbell."

"*Francis M. Jennings.*"

LXXI. *On the Porism of the In-and-circumscribed Triangle, and on an Irrational Transformation of two Ternary Quadratic Forms each into itself.* By Arthur Cayley, *Esq.**

THERE is an irrational transformation of two ternary quadratic forms each into itself, based upon the solution of the following geometrical problem,

Given that the line

$$lx+my+nz=0$$

meets the conic

$$(a, b, c, f, g, h \between x, y, z)^2=0$$

in the point (x_1, y_1, z_1); to find the other point of intersection.

The solution is exceedingly simple. Take (x_2, y_2, z_2) for the coordinates of the other point of intersection, we must have identically with respect to x, y, z,

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* Communicated by the Author.

$$(a\,..\between x, y, z)^2 >\!\!< (\mathfrak{A}\,..\between l, m, n)^2 - k(lx+my+nz)^2$$
$$=(a,\,..\between x_1, y_1, z_1\between x, y, z) >\!\!< (a,\,..\between x_2, y_2, z_2\between x, y, z)$$

to a constant factor *près*.

Assume successively $x, y, z=\mathfrak{A}, \mathfrak{H}, \mathfrak{G}$; $\mathfrak{H}, \mathfrak{B}, \mathfrak{F}$; $\mathfrak{C}, \mathfrak{F}, \mathfrak{C}$; it follows that

$$\begin{aligned} x_2 : y_2 : z_2 = y_1 z_1 &\{\mathfrak{A}(\mathfrak{A}\,..\between l, m, n)^2 - (\mathfrak{A}l+\mathfrak{H}m+\mathfrak{G}n)^2\} \\ : z_1 x_1 &\{\mathfrak{B}(\mathfrak{A}\,..\between l, m, n)^2 - (\mathfrak{H}l+\mathfrak{B}m+\mathfrak{F}n)^2\} \\ : x_1 y_1 &\{\mathfrak{C}(\mathfrak{A}\,..\between l, m, n)^2 - (\mathfrak{G}l+\mathfrak{F}m+\mathfrak{C}n)^2\}\,; \end{aligned}$$

or, what is the same thing,

$$\begin{aligned} x_2 : y_2 : z_2 = y_1 z_1 &(bn^2+cm^2-2fmn) \\ : z_1 x_1 &(cl^2+an^2-2gnl) \\ : x_1 y_1 &(am^2+bn^2-2hlm). \end{aligned}$$

It is not necessary for the present purpose, but it may be as well to give the corresponding solution of the problem.

Given that one of the tangents through the point (ξ, η, ζ) to the conic

$$(a, b, c, f, g, h\between x, y, z)^2=0$$

is the line $l_1 x+m_1 y+n_1 z=0$; to find the equation to the other tangent.

Let $l_2 x+m_2 y+n_2 z=0$ be the other tangent, then

$$(a\,..\between \xi, \eta, \zeta)^2 (a\,..\between x, y, z)^2 - \overline{(a\,..\between \xi, \eta, \zeta\between x, y, z)\,}|^2$$
$$=(l_1 x+m_1 y+n_1 z)(l_2 x+m_2 y+n_2 z)$$

to a constant factor *près*. Assume successively $y=0$, $z=0$; $z=0$, $x=0$; $x=0$, $y=0$, we have

$$\begin{aligned} l_2 : m_2 : n_2 = m_1 n_1 &\{a(a\,..\between \xi, \eta, \zeta)^2 - (a\xi+h\eta+g\zeta)^2\} \\ n_1 l_1 &\{b(a\,..\between \xi, \eta, \zeta)^2 - (h\xi+b\eta+f\zeta)^2\} \\ l_1 m_1 &\{c(a\,..\between \xi, \eta, \zeta)^2 - (g\xi+f\eta+c\zeta)^2\}\,; \end{aligned}$$

or, as they may be more simply written,

$$\begin{aligned} l_2 : m_2 : n_2 = m_1 n_1 &(\mathfrak{B}\zeta^2+\mathfrak{C}\eta^2+2\mathfrak{F}\eta\zeta) \\ n_1 l_1 &(\mathfrak{C}\xi^2+\mathfrak{A}\zeta^2-2\mathfrak{G}\zeta\xi) \\ l_1 m_1 &(\mathfrak{A}\eta^2+\mathfrak{B}\xi^2-2\mathfrak{H}\xi\eta). \end{aligned}$$

Returning now to the solution of the first problem, I shall for the sake of simplicity consider the formulæ obtained by taking for the equation of the conic,

$$\alpha x^2+\beta y^2+\gamma z^2=0.$$

We see, therefore, that if this conic be intersected by the line

$lx+my+nz=0$ in the points (x_1, y_1, z_1) and (x_2, y_2, z_2), then

$$\begin{aligned} x_2 : y_2 : z_2 = {} & y_1z_1(\gamma m^2+\alpha n^2) \\ & : z_1x_1(\alpha n^2 + \beta l^2) \\ & : x_1y_1(\beta l^2+\alpha m^2). \end{aligned}$$

We have, in fact, *identically*

$$\begin{aligned} & l y_1z_1(\beta n^2+\gamma m^2)+mz_1x_1(\gamma l^2+\alpha n^2)+nx_1y_1(\alpha m^2+\beta l^2) \\ & \quad =(\alpha mnx_1+\beta nly_1+\gamma lmz_1)(lx_1+my_1+nz_1) \\ & \quad -lmn(\alpha x_1{}^2+\beta y_1{}^2+\gamma z_1{}^2) \end{aligned}$$

$$\begin{aligned} & \alpha y_1{}^2z_1{}^2(\beta n^2+\gamma m^2)^2+\beta z_1{}^2x_1{}^2(\gamma l^2+\alpha n^2)^2+\gamma x_1{}^2y_1{}^2(\alpha m^2+\beta l^2)^2= \\ & \quad \alpha\beta\gamma\{-l^3x_1{}^3-m^3y_1{}^3-n^3z_1{}^3+\overline{my_1+nz_1}l^2x_1{}^2+\overline{nz_1+lx_1}m^2y_1{}^2 \\ & \quad +\overline{lx_1+my_1}n^2y_1{}^2-2lmnx_1y_1z_1\}(lx_1+my_1+nz_1) \\ & \quad -(l^4\beta\gamma x_1{}^2+m^4\gamma\alpha y_1{}^2+n^4\alpha\beta z_1{}^2)(\alpha x_1{}^2+\beta y_1{}^2+\gamma z_1{}^2)=0\,; \end{aligned}$$

which show that if $lx_1+my_1+nz_1=0$ and $\alpha x_1{}^2+\beta y_1{}^2+\gamma_1 z^2=0$, then also $lx_2+my_2+nz_2=0$ and $\alpha x_2{}^2+\beta y_2{}^2+\gamma z_2{}^2=0$: this is, of course, as it should be.

I shall now consider l, m, n as *given functions* of x_1, y_1, z_1 satisfying identically the equations

$$\begin{cases} lx_1+my_1+nz_1=0 \\ l^2bc+m^2ca+n^2ab=0, \end{cases}$$

equations which express that $lx+my+nz=0$ is the tangent from the point x_1, y_1, z_1 to the conic $ax^2+by^2+cz^2=0$. And I shall take for α, β, γ the following values, viz.

$$\begin{aligned} \alpha &= ax_1{}^2+by_1{}^2+cz_1{}^2-a(x_1{}^2+y_1{}^2+z_1{}^2) \\ \beta &= ax_1{}^2+by_1{}^2+cz_1{}^2-b(x_1{}^2+y_1{}^2+z_1{}^2) \\ \gamma &= ax_1{}^2+by_1{}^2+cz_1{}^2-c(x_1{}^2+y_1{}^2+z_1{}^2)\,; \end{aligned}$$

so that x_1, y_1, z_1 continuing absolutely indeterminate, we have *identically* $\alpha x_1{}^2+\beta y_1{}^2+\gamma z_1{}^2=0$. Also taking Θ as a function of x_1, y_1, z_1, the value of which will be subsequently given, I write

$$\begin{aligned} x_2 &= \Theta y_1z_1(\beta n^2+\gamma m^2) \\ y_2 &= \Theta z_1x_1(\gamma l^2 + \alpha n^2) \\ z_2 &= \Theta x_1y_1(\alpha m^2+\beta l^2)\,; \end{aligned}$$

so that x_1, y_1, z_1 are arbitrary, and x_2, y_2, z_2 are taken to be determinate functions of x_1, y_1, z_1. The point (x_2, y_2, z_2) is geometrically connected with the point (x_1, y_1, z_1) as follows, viz. (x_2, y_2, z_2) is the point in which the tangent through (x_1, y_1, z_1) to the conic $ax^2+by^2+cz^2=0$ meets the conic passing through the point (x_1, y_1, z_1) and the points of intersection of the conics

$ax^2+by^2+cz^2=0$ and $x^2+y^2+z^2=0$. Consequently, in the particular case in which (x_1, y_1, z_1) is a point on the conic $x^2+y^2+z^2=0$, the point (x_2, y_2, z_2) is the point in which this conic is met by the tangent through (x_1, y_1, z_1) to the conic $ax^2+by^2+cz^2=0$.

It has already been seen that $lx_1+my_1+nz_1=0$ and $\alpha x_1^2+\beta y_1^2+\gamma z_1^2=0$ identically; consequently we have identically $lx_2+my_2+nz_2=0$ and $\alpha x_2^2+\beta y_2^2+\gamma z_2^2=0$. The latter equation, written under the form

$$(ax_1^2+by_1^2+cz_1^2)(x_2^2+y_2^2+z_2^2)$$
$$-(x_1^2+y_1^2+z_1^2)(ax_2^2+by_2^2+cz_2^2)=0,$$

shows that if x_2, y_2, z_2 are such that $x_2^2+y_2^2+z_2^2=x_1^2+y_1^2+z_1^2$, then that also $ax_2^2+by_2^2+cz_2^2=ax_1^2+by_1^2+cz_1^2$. I proceed to determine Θ so that we may have $x_2^2+y_2^2+z_2^2=x_1^2+y_1^2+z_1^2$. We obtain immediately

$$\frac{1}{\Theta^2}(x_2^2+y_2^2+z_2^2)=(l^4x_1^2+m^4y_1^2+n^4z_1^2)(\alpha^2x_1^2+\beta^2y_1^2+\gamma^2z_1^2)$$
$$-(\alpha^2l^4x_1^4+\beta^2m^4y_1^4+\gamma^2n^4z_1^4-2\beta\gamma m^2n^2y_1^2z_1^2-2\gamma\alpha n^2l^2z_1^2x_1^2$$
$$-2\alpha\beta l^2m^2x_1^2y_1^2).$$

Write for a moment $ax_1^2+by_1^2+cz_1^2=p$, $x_1^2+y_1^2+z_1^2=q$, so that $\alpha=p-aq$, $\beta=p-bq$, $\gamma=p-cq$, then

$$\alpha^2x_1^2+\beta^2y_1^2+\gamma^2z_1^2=qp^2-2p\,pq+(a^2x_1^2+b^2y_1^2+c^2z_1^2)q^2$$
$$=q\{(a^2x_1^2+b^2y_1^2+c^2z_1^2)q-p^2\}$$
$$=q(\overline{b-c}^2y_1^2z_1^2+\overline{c-a}^2z_1^2x_1^2+\overline{a-b}^2x_1^2y_1^2),$$

$$\alpha^2l^4x_1^4+\beta^2m^4y_1^4+\gamma^4n^4z_1^4-2\beta\gamma m^2n^2y_1^2z_1^2-2\gamma\alpha n^2l^2z_1^2x_1^2$$
$$-2\alpha\beta l^2m^2x_1^2y_1^2$$
$$=p^2(l^4x_1^4+m^4y_1^4+n^4z_1^4-2m^2n^2y_1^2z_1^2-2n^2l^2z_1^2x_1^2-2l^2m^2x_1^2y_1^2)$$
$$-2pq(al^4x_1^4+bm^4y_1^4+cn^4z_1^4-\overline{b+c}\,m^2n^2y_1^2z_1^2-\overline{c+a}\,n^2l^2z_1^2x_1^2$$
$$-\overline{a+b}\,l^2m^2x_1^2y_1^2)$$
$$+q^2(a^2l^4x_1^4+b^2m^4y_1^4+c^2n^4z_1^4-2bcm^2n^2y_1^2z_1^2-2can^2l^2z_1^2x_1^2$$
$$-2abl^2m^2x_1^2y_1^2),$$

the first line of which vanishes in virtue of the equation $lx_1+my_1+nz_1=0$, we have therefore

$$\frac{1}{\Theta^2}(x_2^2+y_2^2+z_2^2)\div(x_1^2+y_1^2+z_1^2)=$$
$$(l^4x_1^2+m^4y_1^2+n^4z_1^2)(\overline{b-c}^2y_1^2z_1^2+\overline{c-a}^2z_1^2x_1^2+\overline{a-b}^2x_1^2y_1^2)$$
$$+2(ax_1^2+by_1^2+cz_1^2)(al^4x_1^4+bm^4y_1^4+cn^4z_1^4-\overline{b+c}\,m^2n^2y_1^2z_1^2$$
$$-\overline{c+a}\,n^2l^2z_1^2x_1^2-\overline{a+b}\,l^2m^2x_1^2y_1^2)$$
$$-(x_1^2+y_1^2+z_1^2)(a^2l^4x_1^4+b^2m^4y_1^4+c^2n^4z_1^4-2bcm^2n^2y_1^2z_1^2$$
$$-2can^2l^2z_1^2x_1^2-2abl^2m^2x_1^2y_1^2).$$

Hence reducing the function on the right-hand side, and putting $x_2{}^2+y_2{}^2+z_2{}^2 \div x_1{}^2+y_1{}^2+z_1{}^2=1$, we have

$$
\begin{aligned}
\frac{1}{\Theta^2} &= a^2l^4x_1{}^6+b^2m^4y_1{}^6+c^2n^4z_1{}^6 \\
&+(c^2m^4-2b^2m^2n^2)y_1{}^4z_1{}^2+(a^2n^4-2c^2n^2l^2)z_1{}^4x_1{}^2+(b^2l^4-2a^2l^2m^2)x_1{}^4y_1{}^2 \\
&+(b^2n^4-2c^2m^2n^2)y_1{}^2z_1{}^4+(c^2l^4-2a^2n^2l^2)z_1{}^2x_1{}^4+(a^2m^4-2b^2l^2m^2)x_1{}^2y_1{}^4 \\
&+\{l^4(b-c)^2+m^4(c-a)^2+n^4(a-b)^2 \\
&+2m^2n^2(bc-ca-ab)+2n^2l^2(-bc+ca-ab) \\
&\qquad\qquad +2l^2m^2(-bc-ca+ab)\}x_1{}^2y_1{}^2z_1{}^2
\end{aligned}
$$

The value of Θ might probably be expressed in a more simple form by means of the equations $lx_1+my_1+nz_1=0$ and $l^2bc+m^2ca+n^2ab=0$, even without solving these equations; but this I shall not at present inquire into.

Recapitulating, l, m, n are considered as functions of x_1, y_1, z_1 determined (to a common factor *près*) by the equations

$$
\begin{aligned}
&lx_1+my_1+nz_1=0 \\
&l^2bc+m^2ca+n^2ab=0.
\end{aligned}
$$

Θ is determined as above, and then writing

$$
\begin{aligned}
\alpha&=ax_1{}^2+by_1{}^2+cz_1{}^2-a(x_1{}^2+y_1{}^2+z_1{}^2) \\
\beta&=ax_1{}^2+by_1{}^2+cz_1{}^2-b(x_1{}^2+y_1{}^2+z_1{}^2) \\
\gamma&=ax_1{}^2+by_1{}^2+cz_1{}^2-c(x_1{}^2+y_1{}^2+z_1{}^2),
\end{aligned}
$$

we have

$$
\begin{aligned}
x_2&=\Theta y_1z_1(\beta n^2+\gamma m^2) \\
y_2&=\Theta z_1x_1(\gamma l^2+\alpha n^2) \\
z_2&=\Theta x_1y_1(\alpha m^2+\beta l^2).
\end{aligned}
$$

And these values give

$$
\begin{aligned}
lx_2+my_2+nz_2&=0 \\
x_2{}^2+y_2{}^2+z_2{}^2&=x_1{}^2+y_1{}^2+z_1{}^2 \\
ax_2{}^2+by_2{}^2+cz_2{}^2&=ax_1{}^2+by_1{}^2+cz_1{}^2.
\end{aligned}
$$

In connexion with the subject I may add the following transformation, viz. if

$$3\sqrt{\alpha}x'=\sqrt{3\beta}(y-z)+\sqrt{(3\alpha-2\beta)(x^2+y^2+z^2)+2\beta(yz+zx+xy)}$$

:

Then reciprocally

$$3\sqrt{\beta}x=-\sqrt{3\alpha}(y'-z')+\sqrt{(3\beta-2\alpha)(x'^2+y'^2+z'^2)+2\alpha(y'z'+z'x'+x'y')}.$$

:

Also
$$x^2+y^2+z^2=x'^2+y'^2+z'^2$$

$$\beta(x^2+y^2+z^2-yz-zx-xy)=\alpha(x'^2+y'^2+z'^2-y'z'-z'x'-x'y').$$

Suppose $1+\rho+\rho^2=0$, then

$$x^2+y^2+z^2-yz-zx-xy=(x+\rho y+\rho^2 z)(x+\rho^2 y+\rho z);$$

and in fact

$$3\sqrt{\alpha}(x'+\rho y'+\rho^2 z')=-\sqrt{3\beta}(1+2\rho)(x+\rho y+\rho^2 z)$$

$$3\sqrt{\alpha}(x'+\rho^2 y'+\rho z)=\sqrt{3\beta}(1+2\rho)(x+\rho^2 y+\rho z).$$

The preceding investigations have been in my possession for about eighteen months.

2 Stone Buildings,
April 18, 1855.

LXXII. *Contributions to our Knowledge of the Nature of the so-called Coercive Force.* *By* Professor Plücker[*].

1. In a former memoir I proved that the magnetism excited by paramagnetic or diamagnetic induction in various substances is not proportional to the inducing force, but in each particular substance, according to a peculiar law, approaches a maximum which is the point of saturation of the substance. I have shown empirically, that for each of the substances examined by me, the induced magnetism, after assuming the unit of inducing force, could be determined by means of two other constants which are independent of each other. One of these constants I have named the *constant of induction*, and the other the *constant of resistance*[†]. If it should be found that the constant of resistance in any substance is zero, then for this substance the induced magnetism would be proportional to the inducing force, and the receptivity of the substance for magnetism would be shown by the constant of induction alone. The induced magnetism of the substances examined by me (nickel, cobalt, iron, oxide of iron, oxide of nickel, bismuth, phosphorus, oxygen, hydrated oxide of cobalt) always falls short, in different degrees, of the proportionality mentioned; and the more so, the greater is the constant of resistance. The few observations made at the time rendered it to me probable that for those substances which, after the inducing force to which they were exposed had ceased to act upon them, still retained a considerable portion of their magnetism, the constant of resistance was comparatively small; so that the magnetism of such substances, when the inducing force is augmented, increases more quickly, and more slowly approaches the point of saturation. As far as regards hard steel in comparison with iron, I could not doubt the correctness of

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[*] Extract from a paper in Poggendorff's *Annalen*, vol. xciv. p. 28.

[†] Some other term might have been chosen with advantage.—Ed. Phil. Mag.

this conclusion. This was proved by a simple experiment which I mentioned several years ago, and which showed that two bars, a thick one of iron and a thin one of steel, formed into a cross and suspended horizontally before the pole of a magnet, were directed at a distance by the bar of iron, but near to the magnet by the bar of steel. Numbers, however, were wanting which I could not obtain from this experiment. The solid bodies examined by me in the memoir referred to were all in the form of powder. It appeared to me therefore very desirable to bring the above question to the test of experiment, operating in a manner which permitted of the examination of iron and steel in a massive form.

2. For this purpose I had prepared a button of iron (A), and three buttons of steel (B, C, D), as nearly as possible alike in shape and form; the three latter were made as hard as possible, and afterwards the button B was tempered to blue, the button C to yellow, while the button D retained its full hardness. These three buttons were cut from the same bar of fine English steel, and were rounded hemispherically and polished at the ends. Their thickness was 8 millims.; their length, excluding a small hook for suspension at the other end, was 14 millims.; their weight about 6·3 grms. From the same bar of steel, still smaller bars E were cut 53 millims. long, and also hardened and rounded at the ends. The button A was formed from a bar of iron of equal thickness; and again from the same bar, a smaller one F was taken, equal in size and form to the steel bar E.

3. Five series of weighings were undertaken in order to determine the weights which were necessary to separate the different buttons from the inducing magnetic pole. For the three first series of weighings, three of the steel bars E were taken and carefully magnetized in different degrees. These bars were then successively fixed vertically by screws upon a foot; upon them the different buttons were caused to rest, and the weight which was necessary to effect their separation was determined. In the first series of weighings, in which the button was attracted by the bar which was most feebly magnetized, I made use of a fine Geissler's balance, and the weight was determined by a rider of platinum which was moved along the arm of a lever. In the second series, in which the different buttons were attracted by the bar of medium magnetic strength, the same balance was made use of, but the amount of the attraction was determined by pouring dry sand out of a small orifice upon the scale-pan at the opposite end of the balance-beam. The sand after each determination was weighed with another balance. In the third series of experiments, the glass balance of Geissler had to be changed for a larger chemical balance; the determinations were

made in the same manner as in the second series, only that weights were first laid on before the sand was added.

In the last two series of weighings the large electro-magnet was made use of; a very blunt cone, whose base was of the same diameter as the electro-magnet, and whose point was rounded, being placed upon the flat pole of the magnet. The buttons were hung upon one of the arms of a common balance, and after partially balancing the attraction by weights placed upon the opposite scale-pan, the separation was finally effected by the addition of small grains of shot. The electro-magnet was at one time excited by a single cell of Grove, and again by a battery of six cells.

4. *First Series of Experiments.*

The numbers express the weights in grammes necessary to produce the separation; those side by side are the weighings of the same button, repeated immediately one after the other. The succession in which the different buttons were weighed is stated in the first vertical column, the substance of the button used in each case being indicated.

	grm.	grm.
Soft iron	0·395	0·395
Blue steel	0·33	0·34
Yellow steel	0·26	0·27
Glass hard steel . .	0·120	0·125
Yellow steel	0·26	0·27
Blue steel	0·35	0·34
Soft iron	0·415	0·405

5. *Second Series.*

	grms.	grms.
Soft iron	2·60	2·70
Blue steel	2·10	2·20
Yellow steel	1·80	1·80
Glass hard steel . .	1·00	1·00
Yellow steel	1·70	1·60
Blue steel	2·00	2·10
Soft iron	2·55	2·65

6. *Third Series.*

The attraction of the different buttons by the strongest of the three steel magnets was determined twice on different days.

	grms.	grms.	grms.	grms.
Soft iron	35·8	36·0	35·9	36·4
Blue steel	29·8	29·9	30·7	29·2
Yellow steel . . .	25·4	25·8	25·8	25·0
Glass hard steel .	18·0	18·1	19·0	18·5

	grms.	grms.	grms.	grms.
Soft iron	34·0	34·9	35·0	
Blue steel	29·2	29·8	30·2	
Yellow steel	24·2	24·7	24·1	
Glass hard steel	17·9	18·2	18·6	
Soft iron	35·2	34·9	36·0	34·5

7. *Fourth Series.*

	grms.	grms.
Soft iron	205 +	131 131 127 124
Blue steel	295 +	3 2 2·5
Yellow steel	255 +	7 5 6
Glass hard steel	250 +	2·5 2·3 3·0 5·8
Soft iron	225 +	7·6 4·5 4·7
	205 +	22 25

8. *Fifth Series.*

	grms.	grms.
Soft iron	1553 +	21 18 19
Blue steel	1423 +	24 9 12
Yellow steel	1233 +	20 6·5 6·5 4
Glass hard steel	1133 +	15·5 14·5 18·5
Soft iron	1543 +	28
Yellow steel	1233 +	16 16·5

In the fourth and fifth series, the greater numbers which stand forward denote the weight not yet sufficient to effect separation, and the smaller numbers behind the brackets denote the weight of shot which was added.

9. Taking the mean values of the single determinations of each series we obtain the following result:—

	Glass hard steel. grms.	Yellow steel. grms.	Blue steel. grms.	Soft iron. grms.
I.	0·1225	0·268	0·340	0·405
II.	1·1000	1·725	2·210	2·625
III.	18·4	25·0	29·9	36·0
	18·2	24·3	29·7	34·6
IV.	233	261	298	331
V.	1149	1245	1438	1572

Setting the attraction of the button of glass hard steel equal unity, we obtain for the attraction of the three other buttons the following proportionate numbers:—

	Yellow steel. grms.	Blue steel. grms.	Soft iron. grms.
I.	2·18	2·78	3·31
II.	1·72	2·21	2·62
III.	1·36	1·63	1·96
	1·34	1·63	1·90
IV.	1·12	1·28	1·42
V.	1·084	1·25	1·37

10. This tabular statement shows, as was to be expected, in a striking manner, that the harder the steel is, the more feebly it is magnetized; but that with increasing force, its magnetism increases in a so much quicker ratio. We see that when high power is applied, the yellow steel is not much more strongly attracted than the glass hard, but is more than twice as strongly attracted when the force is feeble; and that the magnetic deportment of the blue steel forms the transition to the attraction of the soft iron. By the feeblest of the forces applied this is attracted $3\frac{2}{3}$ times, by the highest power only $1\frac{1}{3}$ time more strongly than the glass hard steel*.

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* This result seems to be an important contribution towards a physical theory of magnetism. The word "resistance" appears to us unhappily chosen by M. Plücker; in the case of iron it is an *exhaustion* of the quality on which its attraction depends, rather than a resistance to further magnetization, which seems to be indicated by the experiments.—J. T.

LXXIII. *On the Dynamical Theory of Heat.*—Part V. *On the Quantities of Mechanical Energy contained in a Fluid in Different States, as to Temperature and Density.* *By* WILLIAM THOMSON, *M.A., Professor of Natural Philosophy in the University of Glasgow*[*].

[Continued from vol. iv. p. 434.]

81. A BODY which is either emitting heat, or altering its dimensions against resisting forces, is doing work upon matter external to it. The mechanical effect of this work in one case is the excitation of thermal motions, and in the other the overcoming of resistances. The body must itself be altering in its circumstances, so as to contain a less store of work within it by an amount precisely equal to the aggregate value of the mechanical effects produced; and conversely, the aggregate value of the mechanical effects produced must depend solely on the initial and final states of the body, and is therefore the same whatever be the intermediate states through which the body passes, provided the *initial* and *final* states be the same.

82. The total mechanical energy of a body might be defined as the mechanical value of all the effect it would produce in heat emitted and in resistances overcome, if it were cooled to the utmost, and allowed to contract indefinitely or to expand indefinitely according as the forces between its particles are attractive or repulsive, when the thermal motions within it are all stopped; but in our present state of ignorance regarding perfect cold, and the nature of molecular forces, we cannot determine this "total mechanical energy" for any portion of matter, nor even can we be sure that it is not infinitely great for a finite portion of matter. Hence it is convenient to choose a certain state as standard for the body under consideration, and to use the unqualified term, *mechanical energy*, with reference to this standard state; so that the "mechanical energy of a body in a given state" will denote the mechanical value of the effects the body would produce in passing from the state in which it is given, to the standard state, or the mechanical value of the whole agency that would be required to bring the body from the standard state to the state in which it is given.

83. In the present communication, a system of formulæ founded on propositions established in the first part of my paper on the Dynamical Theory of Heat, and expressing relations between the pressure of a fluid, and the thermal capacities and mechanical energy of a given mass of it, all considered as functions of the temperature and volume; and Carnot's function of

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[*] From the Transactions of the Royal Society of Edinburgh, vol. xx. part 3; read December 15, 1851.

the temperature; are brought forward for the purpose of pointing out the importance of making the *mechanical energy* of a fluid in different states an object of research, along with the other elements which have hitherto been considered, and partially investigated in some cases.

84. If we consider the circumstances of a stated quantity (a unit of matter, a pound, for instance) of a fluid, we find that its condition, whether it be wholly in the liquid state or wholly gaseous, or partly liquid and partly gaseous, is completely defined, when its temperature, and the volume of the space within which it is contained, are specified (§§ 20, 53, ... 56), it being understood, of course, that the dimensions of this space are so limited that no sensible differences of density in different parts of the fluid are produced by gravity. We shall therefore consider the temperature, and the volume of unity of mass, of a fluid, as the independent variables of which its pressure, thermal capacities, and mechanical energy are functions. The volume and temperature being denoted respectively by v and t, let e be the mechanical energy, p the pressure, K the thermal capacity under constant pressure, and N the thermal capacity in constant volume; and let M be such a function of these elements, that

$$\mathrm{K}=\mathrm{N}+\frac{\frac{dp}{dt}}{-\frac{dp}{dv}}\mathrm{M} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

or (§§ 48, 20), such a quantity that

$$\mathrm{M}dv+\mathrm{N}dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

may express the quantity of heat that must be added to the fluid mass, to elevate its temperature by dt, when its volume is augmented by dv.

85. The mechanical value of the heat added to the fluid in any operation, or the quantity of heat added, multiplied by J (the mechanical equivalent of the thermal unit), must be diminished by the work done by the fluid in expanding against resistance, to find the actual increase of mechanical energy which the body acquires. Hence (de of course denoting the complete increment of e, when v and t are increased by dv and dt) we have

$$de=\mathrm{J}(\mathrm{M}dv+\mathrm{N}dt)-pdv \quad . \quad . \quad . \quad . \quad (3).$$

Hence, according to the usual notation for partial differential coefficients, we have

$$\frac{de}{dv}=\mathrm{JM}-p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

$$\frac{de}{dt}=\mathrm{JN} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

Lastly, if we denote by μ, as formerly, Carnot's function of the temperature t, we have (§ 21)

$$\frac{dp}{dt}=\mu M \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

86. The use that may be made of these formulæ in investigations regarding the physical properties of any particular fluid must depend on the extent and accuracy of the general data belonging to the theory of the mechanical action of heat that are available. Thus, if nothing be known by experiment regarding the values of J and μ, we may, in the first place, use equations (4) and (5), or the following deduced from them (§ 20), by eliminating e,

$$\frac{dp}{dt}=J\left(\frac{dM}{dt}-\frac{dN}{dv}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

and equation (6), as tests of the accuracy of experimental researches on the pressure and thermal capacities of a fluid, on account of the knowledge we have from theory that J is *certainly* an absolute constant, and that in all probability, if not with absolute certainty, we may regard μ as independent of v, and as the same for all fluids at the same temperature; and with experimental data of sufficient extent, we may use these equations as means of actually determining the values of J and μ. No other way than this has yet been attempted for determining μ; and if we except a conceivable, but certainly not at present practicable mode of determining this element by experiments on thermo-electric currents, no other way is yet known. Carnot's original determination of μ was effected by means of an expression equivalent to that of equation (6) applied to the case of a mass of air; and the determinations by Clapeyron, and those shown in Table I. of my 'Account of Carnot's Theory,' were calculated by the formula which is obtained when the same equation is applied to the case of a fluid mass, partly liquid and partly in the state of saturated vapour (§ 55).

87. As yet experiments have not been made on the pressure and thermal capacities of fluids to a sufficient extent to supply data for the evaluation, even in the roughest manner, of the expression given for J by equation (7); and it may be doubted whether such data can even be had with accuracy enough to give as exact a determination of this important element as may be effected by direct experiments on the generation of heat by means of friction. At present we may regard J as known, probably within $\frac{1}{300}$ of its own amount, by experiments of this kind.

88. The value of J being known, equations (4) and (5) may be used for determining the mechanical energy of a particular fluid mass in different states, from special experimental data

regarding its pressure and thermal capacities, but not necessarily comprehending the values of each of these elements for all states of the fluid. The theory of the integration of functions of two independent variables will, when any set of data are proposed, make it manifest whether or not they are sufficient, and will point out the methods, whether of summation or of analytical integration, according to the forms in which the data are furnished, to be followed for determining the value of *e* for every value of *v*. Or the data may be such, that while the thermal capacities would be derived from them by differentiation, values of *e* may be obtained from them without integration. Thus, if the fluid mass consist of water and vapour of water at the temperature *t*, weighing in all one pound, and occupying the volume *v**, and if we regard the *zero* or "standard" state of the mass as being liquid water at the temperature 0°, the mechanical energy of the mass in the given state will be the mechanical value of the heat required to raise the temperature of a pound of water from 0° to *t*, and to convert $\frac{v-\lambda}{\gamma-\lambda}$ of it into vapour, diminished by the work done in the expansion from the volume λ to the volume *v*; that is, we have

$$e=\mathrm{J}\left(ct+\mathrm{L}\frac{v-\lambda}{\gamma-\lambda}\right)-p(v-\lambda) \quad . \quad . \quad . \quad . \quad (8).$$

The variables *c*, L, and *p* (which depend on *t* alone) in this expression have been experimentally determined by Regnault for all temperatures from 0° to 230°; and when γ is also determined by experiments on the density of saturated steam, the elements for the determination of *e* in this case will be complete. The expressions investigated formerly for M and N in this case (§ 54) may be readily obtained by means of (4) and (5) of § 85, by the differentiation of (8).

89. If Carnot's function has once been determined by means of observations of any kind, whether on a single fluid or on different fluids, for a certain range of temperatures, then according to (6) of § 85, the value of $\frac{\frac{dp}{dt}}{\mathrm{M}}$ for any substance whatever is

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* The same notation is used here as formerly in § 54, viz. *p* is the pressure of saturated vapour at the temperature *t*, γ the volume, and L the latent heat of a pound of the vapour, λ the volume of a pound of liquid water, and *c* the mean thermal capacity of a pound of water between the temperatures 0 and *t*. A mass weighing a pound, and occupying the volume *v*, when at the temperature *t*, must consist of a weight $\frac{v-\lambda}{\gamma-\lambda}$ of vapour, and $\frac{\gamma-v}{\gamma-\lambda}$ of water.

known for all temperatures within that range. It follows that when the values of M for different states of a fluid have been determined experimentally, the law of pressures for all temperatures and volumes (with an arbitrary function of v to be determined by experiments on the pressure of the fluid at one particular temperature) may be deduced by means of equation (6); or conversely, which is more likely to be the case for any particular fluid, if the law of pressures is completely known, M may be deduced without further experimenting. Hence the second member of (4) becomes completely known, the equation assuming the following form, when, for M, its value according to (6) is substituted:—

$$\frac{de}{dv}=\frac{\mathrm{J}}{\mu}\frac{dp}{dt}-p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

The integration of this equation with reference to v leads to an expression for e, involving an arbitrary function of t, for the determination of which more data from experiment are required. It would, for instance, be sufficient for this purpose to have the mechanical energy of the fluid for all temperatures when contained in a constant volume; or, what amounts to the same (it being now supposed that J is known), to have the thermal capacity of the fluid in constant volume for a particular volume and all temperatures. Hence we conclude, that when the elements J and μ belonging to the general theory of the mechanical action of heat are known, the mechanical energy of a particular fluid may be investigated without experiment, from determinations of its pressure for all temperatures and volumes, and its thermal capacity for any particular constant volume and all temperatures.

90. For example, let the fluid be atmospheric air, or any other subject to the "gaseous" laws. Then if v_0 be the volume of a unit of weight of the fluid, and 0 the temperature, in the standard state from which the mechanical energy in any other state is reckoned, and if p_0 denote the corresponding pressure, we have

$$p=\frac{p_0v_0}{v}(1+\mathrm{E}t), \quad \frac{dp}{dt}=\frac{p_0v_0\mathrm{E}}{v},$$

and

$$\int_{v_0}^{v}\left(\frac{\mathrm{J}}{\mu}\frac{dp}{dt}-p\right)dv=p_0v_0\left\{\frac{\mathrm{JE}}{\mu}-(1+\mathrm{E}t)\right\}\log\frac{v}{v_0}.$$

Hence if we denote by N_0 the value of N when $v=v_0$, whatever be the temperature, we have as the general expression for the mechanical energy of a unit weight of a fluid subject to the gaseous laws,

$$e=p_0v_0\left\{\frac{\mathrm{JE}}{\mu}-(1+\mathrm{E}t)\right\}\log\frac{v}{v_0}+\mathrm{J}\int_0^t\mathrm{N}_0dt \quad . \quad . \quad (9).$$

91. Let us now suppose the mechanical energy of a particular fluid mass in various states to have been determined in any way, and let us find what results regarding its pressure and thermal capacities may be deduced. In the first place, by integrating equation (8), considered as a differential equation with reference to t for p, we find

$$p=\epsilon^{\frac{1}{J}\int_0^t \mu dt}\int_0^t \mu\frac{de}{dv}\epsilon^{-\frac{1}{J}\int_0^t \mu dt}dt+\psi(v)\epsilon^{\frac{1}{J}\int_0^t \mu dt} \quad . \quad . \quad (10),$$

where $\psi(v)$ denotes a constant with reference to t, which may vary with v, and cannot be determined without experiment. Again, we have from (5), (4), and (1),

$$\left.\begin{aligned} N&=\frac{1}{J}\frac{de}{dt} \\ K&=\frac{1}{J}\frac{de}{dt}+\frac{1}{J}\left(\frac{de}{dv}+p\right)\frac{\frac{dp}{dt}}{-\frac{dp}{dv}} \end{aligned}\right\} \quad . \quad . \quad (11).$$

From the first of these equations we infer, that, with a complete knowledge of the mechanical energy of a particular fluid, we have enough of data for determining for every state its thermal capacity in constant volume. From equation (9) we infer, that with, besides, a knowledge of the pressure for all volumes and a particular temperature, or for all volumes and a particular series of temperatures, we have enough to determine completely the pressure, and consequently also, according to equation (11), to determine the two thermal capacities, for all states of the fluid.

92. For example, let these equations be applied to the case of a fluid subject to the gaseous laws. If we use for $\frac{de}{dv}$ its value derived from (9), in equation (10), we find

$$p=\frac{p_0 v_0}{v}(1+Et)+\chi(v)\epsilon^{\frac{1}{J}\int \mu dt} \quad . \quad . \quad . \quad . \quad (12),$$

where $\chi(v)$, denoting an arbitrary function of v, is used instead of $\psi(v)-\frac{p_0 v_0}{v}$. We conclude that the same expression for the mechanical energy holds for any fluid whose pressure is expressed by this equation, as for one subject to the gaseous laws. Again, by using for $\frac{de}{dt}$ and $\frac{de}{dv}$, their values derived from (9), in equation (11), we have

$$N = N_0 + \frac{1}{J} p_0 v_0 \log \frac{v}{v_0} \frac{d\left\{\frac{JE}{\mu} - (1+Et)\right\}}{dt} \quad . \quad . \quad . \quad . \quad (13),$$

$$K = N_0 + \frac{1}{J} p_0 v_0 \log \frac{v}{v_0} \frac{d\left\{\frac{JE}{\mu} - (1+Et)\right\}}{dt} + \frac{E^2 p_0 v_0}{\mu(1+Et)} \quad (14).$$

The first of these equations shows, that unless Mayer's hypothesis be true, there is a difference in the thermal capacities in constant volume, of the same gas at the same temperatures for different densities, proportional in amount to the difference of the logarithms of the densities. The second, compared with the first, leads to an expression for the difference between the thermal capacities of a gas in constant volume, and under constant pressure, agreeing with results arrived at formerly. [Account of Carnot's Theory, Appendix III.; and Dynamical Theory of Heat, § 48.]

93. It may be that more or less information, regarding explicitly the pressure and thermal capacities of the fluid, may have been had as the data for determining the mechanical energy; but these converse deductions are still interesting, as showing how much information regarding its physical properties is comprehended in a knowledge of the mechanical energy of a fluid mass, and how useful a table of the values of this function for different temperatures and volumes, or an empirical function of two variables expressing it, would be, whatever be the experimental data from which it is deduced. It is not improbable that such a table or empirical function, and a similar representation of the pressure, may be found to be the most convenient expression for results of complete observations on the compressibility, the law of expansion by heat, and the thermal capacities of a vapour or gas.

94. The principles brought forward in a former communication "On a Means of discovering experimentally, &c." (which is now referred to as Part IV. of a series of papers on the Dynamical Theory of Heat), may be expressed in a more convenient and in a somewhat more comprehensive manner than in the formulæ contained in that paper, by introducing the notations and principles which form the subject of the present communication. Thus, let t be the temperature, and u the volume of a pound of air flowing gently in a pipe (under very high pressure it may be) towards a very narrow passage (a nearly closed stopcock, for instance), and let p be its pressure. Let t', u', and p' be the corresponding qualities of the air flowing gently through a continuation of the pipe, after having passed the "rapids" in

and near the narrow passage. Let Q be the quantity of heat (which, according to circumstances, may be positive, zero, or negative) emitted by a pound of air during its whole passage from the former locality through the narrow passage to the latter; and let S denote the mechanical value of the sound emitted from the "rapids." The only other external mechanical effect besides these two produced by the air, is the excess (which, according to circumstances, may be negative, zero, or positive) of the work done by the air in pressing out through the second part of the pipe above that spent in pressing it in through the first; the amount of which, for each pound of air that passes, is of course $p'u'-pu$. Hence the whole mechanical value of the effects produced externally by each pound of the air from its own mechanical energy is

$$\mathrm{JQ}+\mathrm{S}+p'u'-pu \quad . \quad . \quad . \quad . \quad . \quad (15).$$

Hence if $\phi(v, t)$ denote the value of e expressed as a function of the independent variables v and t, so that $\phi(u, t)$ may express the mechanical energy of a pound of air before, and $\phi(u', t')$ the mechanical energy of a pound of air after passing the rapids, we have

$$\phi(u', t')=\phi(v, t)-\{\mathrm{JQ}+\mathrm{S}+p'u'-pu\} \quad . \quad . \quad (16).$$

95. If the circumstances be arranged (as is always possible) so as to prevent the air from experiencing either gain or loss of heat by conduction through the pipe and stopcock, we shall have $\mathrm{Q}=0$; and if (as is perhaps also possible) only a mechanically inappreciable amount of sound be allowed to escape, we may take $\mathrm{S}=0$. Then the preceding equation becomes

$$\phi(u', t')=\phi(u, t)-(p'u'-pu) \quad . \quad . \quad . \quad (17).$$

If by experimenting in such circumstances it be found that t' does not differ sensibly from t, Mayer's hypothesis is verified for air at the temperature t; and as $p'u'$ would then be equal to pu, according to Boyle and Mariotte's law, we should have

$$\phi(u', t)=\phi(u, t),$$

which is, in fact, the expression of Mayer's hypothesis, in terms of the notation for mechanical energy introduced in this paper. If, on the other hand, t' be found to differ from t*, let values of

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* If the values of μ I have used formerly be correct, t' would be less than t for all cases in which t is lower than about 30° Cent.; but, on the contrary, if t be considerably above 30° Cent., t' would be found to exceed t. (See 'Account of Carnot's Theory,' Appendix II.) It may be shown, that if they are correct, air at the temperature 0° forced up with a pressure of ten atmospheres towards a small orifice, and expanding through it to the atmospheric pressure, would go down in temperature by about 4°·4; but that if it had the temperature of 100° in approaching the orifice, it would leave at a temperature about 5°·2 higher, provided that in each case there is no appreciable expenditure of mechanical energy on sound.

p, p', t, and t' be observed in various experiments of this kind, and, from the known laws of density of air, let u and u' be calculated. We then have, by an application of (18) to the results of each experiment, an equation showing the difference between the mechanical energies of a pound of air in two particular specified states as to temperature and density. All the particular equations thus obtained may be used towards forming, or for correcting, a table of the values of the mechanical energy of a mass of air at various temperatures and densities.

96. If, according to the plan proposed in my former communication (§ 72), the air on leaving the narrow passage be made to pass through a spiral pipe immersed in water in a calorimetrical apparatus, and be so brought back exactly to the primitive temperature t, we should have, according to Boyle's and Mariotte's law, $p'u'-pu=0$; and if H denote the value of Q in this particular case (or the quantity of heat measured by means of the calorimetric apparatus), the general equation (16) takes the form

$$\phi(u', t)=\phi(u, t)-(\mathrm{JH}+\mathrm{S}) \quad . \quad . \quad . \quad . \quad (18).$$

If in this we neglect S, as probably insensible, and if we substitute for $\phi(u, t)$ and $\phi(u', t)$ expressions deduced from (9), we find

$$\mathrm{H}=\left\{\frac{1}{\mathrm{J}}-\frac{\mathrm{E}}{\mu(1+\mathrm{E}t)}\right\}pu\log\frac{u'}{u} \quad . \quad . \quad . \quad (19),$$

which agrees exactly with the expression obtained by a synthetical process, founded on the same principles, in my former communication (§ 76).

LXXIV. *On the Electro-statical Capacity of a Leyden Phial and of a Telegraph Wire insulated in the axis of a cylindrical Conducting Sheath. By* Professor W. Thomson*.

THE principles brought forward in the preceding articles On the Uniform Motion of Heat, &c., enable us with great ease to investigate the "capacity†" of a Leyden phial with either air, or any liquid or solid dielectric, and of other analogous arrangements, such as the copper wires in gutta-percha tubes under water, with which Faraday has recently performed such remarkable experiments*.

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* Communicated as an Additional Note to two papers "On the Uniform Motion of Heat in Homogeneous Solid Bodies, and its connexion with the Mathematical Theory of Electricity," and "On the Mathematical Theory of Electricity in Equilibrium;" only not in time to be appended to the reprints of those papers which appeared in the Philosophical Magazine, vol. vii. p. 502, June 1854, and vol. viii. p. 42, July 1854.

† Defined (Phil. Mag. June 1853) for any conductor (subject or not to the influence of other conductors), as the quantity of electricity which it takes to charge it to unit potential.

Thus, for a Leyden phial; let us suppose a portion S of the surface of a conductor A to be everywhere so near the surface of a conductor A′, that the distance between them at any point is a small fraction of the radii of curvature of each surface in the neighbourhood; and let z be the distance between them at a particular position, P. Then, by the analogy with heat, it is clear that if the two surfaces be kept at different electrical potentials, V and V′, the potentials at equidistant points in any line across from one to the other will be in arithmetical progression. Hence $\frac{V-V'}{z}$ will be the rate of variation of the potential perpendicularly across in the position P. If, in the first place, the dielectric be air, the electric force in the air between the two about the position P will consequently be $\frac{V-V'}{z}$, and therefore the electrical density (according to the theorem proved in the first article) on one surface must be $+\frac{1}{4\pi}\frac{V-V'}{z}$, and on the other $-\frac{1}{4\pi}\frac{V-V'}{z}$. The quantity of electricity in the position P, on an area ds of the surface S, is therefore $\frac{1}{4\pi}\frac{V-V'}{z}ds$, and therefore the whole quantity on S is

$$\frac{V-V'}{4\pi}\int\frac{ds}{z},$$

which is Green's general expression for the electrification of either coating of a Leyden phial. If the thickness of the dielectric be constant and equal to τ, it becomes

$$\frac{V-V'}{4\pi}\frac{S}{\tau}.$$

Now if A′ be uninsulated, we have $V'=0$; and then, to charge S to the potential V, it takes the quantity $V\times\frac{S}{4\pi\tau}$. Hence the "capacity" of S is

$$\frac{S}{4\pi\tau}.$$

If instead of air there be a solid or liquid dielectric of inductive capacity, k, occupying the space between the two surfaces, the quantity of heat conducted across, in the analogous thermal circumstances, would be k times as great as in the case corresponding to the air dielectric, with the same difference of temperatures; and in the actual electrical arrangement, the quantity of electricity on each of the conducting surfaces would be k times as great as with air for dielectric and the same difference of potentials. The expression for the capacity of an actual Leyden phial is therefore

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* Described in a lecture at the Royal Institution, Jan. 20, 1854, and subsequently published in vol. vii. p. 197 of the Philosophical Magazine.

$$\frac{kS}{4\pi\tau},$$

k being the inductive capacity of the solid non-conductor of which it is formed, τ its thickness, and S the area of it which is coated on each side.

To investigate the capacity of a copper wire in the circumstances experimented on by Faraday, let us first consider the analogous circumstances regarding the conduction of heat; that is, let us consider the conduction of heat that would take place across the gutta-percha, if the copper wire in its interior were kept continually at a temperature a little above that of the water which surrounds it. Here the quantity of heat flowing outwards from any length of the copper wire, the quantities flowing across different surfaces surrounding it in the gutta-percha, and the quantity flowing into the water from the same length of gutta-percha tube, in the same time, must be equal. But the areas of the same length of different cylindrical surfaces are proportional to their radii, and therefore the flow of heat across equal areas of different cylindrical surfaces in the gutta-percha, coaxial with the wire, must be inversely as their radii. Hence, in the corresponding electrical problem, with air as the dielectric instead of gutta-percha, if R denote the resultant electrical force at any point P in the air between an insulated, electrified, infinitely long cylindrical conductor, and an uninsulated, coaxial, hollow cylindrical conductor surrounding it, and if x be the distance of P from the axis, we have

$$R=\frac{A}{x},$$

where A denotes a constant. But if v be the potential at P; by the definition of "potential" we have

$$\frac{dv}{dx}=-R.$$

Hence

$$\frac{dv}{dx}=-\frac{A}{x};$$

and, by integration,

$$v=-A\log x+C.$$

Assigning the constants A and C so that the potential may have the value V at the surface of the wire, and may vanish at the hollow conducting surface round it, if r and r' denote the radii of these cylinders respectively, we have

$$v=\mathrm{V}\frac{\log\frac{r'}{x}}{\log\frac{r'}{r}},$$

and

$$-\frac{dv}{dx}=\mathrm{R}=\frac{\mathrm{V}}{\log\frac{r'}{r}}\frac{1}{x}.$$

Taking $x=r$, we find by this the electric force in the air infinitely near the inner electrified conductor; and dividing the value found, by 4π (according to the general theorem), we have

$$\frac{1}{4\pi}\frac{\mathrm{V}}{r\log\frac{r'}{r}}$$

for the electrical density on the surface of the conductor. Multiplying this by $2\pi rl$, the area of a length l of the surface, we find

$$\frac{1}{2}\frac{\mathrm{V}l}{\log\frac{r'}{r}}$$

for the whole quantity of electricity on that length. Hence, if k be the specific inductive capacity of gutta-percha, the electricity resting on a length l of the wire in the actual circumstances will amount to

$$\frac{1}{2}\frac{kl}{\log\frac{r'}{r}}\mathrm{V}.$$

Or if S denote the surface of the wire, we have, for the quantity of electricity which it holds,

$$\mathrm{V}.\frac{k\mathrm{S}}{4\pi r\log\frac{r'}{r}};$$

and therefore its capacity is the same as that of a Leyden phial with an equal area of coated glass of thickness equal to $\frac{\mathrm{I}}{k}r\log\frac{r'}{r}$, if I denote the specific inductive capacity of the glass. In the case experimented on by Mr. Faraday, the diameter of the wire was $\frac{1}{16}$th of an inch, and the exterior diameter of the gutta-percha covering was about four times as great. Hence the thickness of the equivalent Leyden phial must have been

$$\frac{I}{k}\cdot\frac{1}{32}\log_e 4=\frac{I}{k}\cdot\frac{1}{23\cdot 08}.$$

As the surface of the wire amounted to 8300 square feet, we may infer that if the gutta-percha had only the same (and it probably has a little greater) inductive capacity as glass, the insulated wire, when the outer surface of the gutta-percha was uninsulated, would have had an electrical capacity equal to that of an ordinary Leyden battery of 8300 square feet of coated glass $\frac{1}{23}$rd of an inch thick.

Inverclog, Arran,
June 1854.

LXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 474.]

March 8, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

THE following paper was read:—

"On Circumstances modifying the Action of Chemical Affinity." By J. H. Gladstone, Ph.D., F.R.S.

The question intended to be solved in this communication is,—what takes place when two binary compounds AB and CD are brought together under such circumstances that both they themselves and the products of their mutual action remain free to react? Do they, according to a generally received opinion, remain unaltered, or, should the affinities so preponderate, become simply AB and CB? Or do A and C, according to Berthollet's view, divide themselves in certain proportions between B and D, the said proportions being determined not solely by the difference of energy in the affinities, but also by the difference of the quantities of the bodies? And, supposing the latter to be the correct view, do the amounts of AD and CB produced by the reaction, increase progressively with the relative increase of AB, or do sudden transitions occur, such as Bunsen and Debus have recently observed in certain cases where the products were removed at once from the field of action?

A reply was sought in the colours produced upon mixing different salts in aqueous solution. There were not many coloured salts suitable for the purpose, as it generally happens that a base gives the same colour with whatever acid it is combined, and *vice versâ*; but the compounds of sesquioxide of iron were peculiarly adapted to the requirements of the experiment, as some are intensely coloured while others are nearly colourless.

The circumstances that attended the formation of the blood-red sulphocyanide were first fully examined. On mixing known quantities of different ferric salts with known quantities of different sulphocyanides, it was found that the whole of the iron was never converted into the red salt; that the amount of it so converted depended on the nature both of the acid combined with the ferric oxide, and of the base combined with the sulphocyanogen; and that it mattered not how the bases and acids had been combined previous to their mixture, so long as the same quantities were brought together into solution. The effect of mass was fully tried by mixing equivalent proportions of ferric salts and sulphocyanides, and then adding known amounts of either one or the other compound. It was found that in either case the amount of red salt was increased; and that when the numbers of equivalents of the salt added were taken as abscissæ, and the amounts of red sulphocyanide produced, as ordinates, the numbers observed in the experiments gave regular curves, though not belonging to the second order. The curves representing the experiments in which sulphocyanide of potassium was mixed with ferric nitrate, chloride, or sulphate, appeared to be the same, but hydrosulphocyanic acid gave a different curve. The deepest colour was given when nitrate of iron was mixed with the sulphocyanide, but even upon the admixture of one equivalent of the former with three of the latter, only 0·194 equiv. of the intensely red ferric salt was formed, and when 375 equivalents of sulphocyanide of potassium had been added there was still a recognizable amount of nitrate of iron undecomposed. It was found that the addition of a colourless salt not only reduced the colour of a solution of ferric sulphocyanide, but also that the reduction increased in a regularly progressive ratio according to the mass of the salt.

Other ferric salts were likewise examined. The black gallate gave results precisely analogous to those obtained by means of the sulphocyanide; the red meconate also confirmed Berthollet's views, but the action of mass was rendered obscure by the formation of double or of acid salts; the red pyromeconate resembled the meconate; the red acetate bore similar testimony; the blue solution of the ferric ferrocyanide in oxalic acid gave results fully corroborative of the influence both of the nature and of the mass of every substance present at the same time in the mixture; the purple and the red comenamate afforded similar results; while the red bromide (not the oxybromide), though somewhat indistinct in its testimony, corroborated to a certain extent the preceding observations.

Experiments were performed with a view to determine what effect the mass of water might have on the salts operated upon; its influence in reducing the colour of the ferric sulphocyanide was found to be very great, but the nature of it could not be exactly determined. As however it was uniform in its action in whatever manner the sulphocyanide had been produced, it could not affect the results of the preceding experiments. Water did not appear to act in any similar manner upon the other ferric salts.

From the mass of quantitative observations made during the investigation, it was possible to deduce not only the order of affinity of the various acids for sesquioxide of iron as compared with potash, but also to assign approximative numbers. Doubt may rest on the position of some terms in the series, but hydrosulphocyanic acid certainly had the least affinity for ferric oxide in comparison with potash: it was represented by unity: the other acids followed in the order—nitric, 4; hydrochloric, 5; sulphuric, 7; gallic, 10; pyromeconic; meconic; acetic, 20; hydrobromic; comenamic; citric, 100; hydroferrocyanic, 170.

Other coloured salts were submitted to a more cursory investigation. The scarlet bromide of gold when treated with an alkaline chloride gave a striking instance of the effect of mass in gradually overcoming a strong affinity. The intensely red iodide of platinum afforded results which, though somewhat obscure, were not opposed in their testimony. So did the blue sulphate of copper when treated with different chlorides. The "manganoso-manganic oxide" dissolves in sulphuric or phosphoric acid of a red, and in other acids of a deep brown colour; and it was found that hydrochloric acid was capable of changing the colour of the sulphate according to its mass, while on the other hand sulphuric or phosphoric acid altered in like manner the tint of the chloride. Somewhat similar results were obtained by means of the green chloride and the purple fluoride of molybdenum; and the blue solution that forms when gallic acid is brought in contact with both the oxides of iron at once, bore testimony to the same general laws. The peculiar optical character of certain salts of quinine was also taken advantage of for determining what changes took place among the compounds in solution. The amount of fluorescence exhibited by a solution of acid sulphate of quinine was found to be affected by the admixture of a chloride, bromide, or iodide according to the nature and the mass of the salt added, and the addition of sulphuric, phosphoric, nitric and other acids was found to produce a fluorescence in solutions either of hydrochlorate of quinine, or of sulphate which had been rendered non-fluorescent by hydrochloric acid. Similar results were obtained with quinidine; and somewhat analogous ones with the organic bases contained in horse-chestnut bark, and in tincture of stramonium. An experiment is also narrated showing that the same laws hold good in respect to compound æthers as to salts having metallic bases, alcohol being employed as the solvent.

Beside the very diversified substances already mentioned in this abstract, several others, such as lead, mercury, zinc, potash, soda, baryta, lime, and ammonia, are shown by a more indirect proof to enter into compounds which obey the same laws. Hence it is concluded that what was observed in reference to the ferric salts holds good very generally, if not universally.

The bearing of certain other phænomena upon the question at issue was also examined. The fact that precipitation, when it occurs, gives rise to a perfect interchange of bases and acids, is equally consistent with either Bergmann's or Berthollet's theory; but not so is the fact that two soluble salts cannot be mixed without the occurrence of precipitation, if one of the products that may be formed is an insoluble salt. The only recorded exception to this law, which occurs with oxalate of iron in the presence of a salt of yttria, under peculiar circumstances, was found on close examination to be in perfect accordance with the principles laid down by Berthollet. Besides the argument founded on this universal fact, several experiments were devised for the purpose of proving that the complete precipitation of an insoluble salt on the mixing of two soluble salts, was due to the insoluble compound being removed at once out of the field of action on the first distribution of the elements, thus necessitating a redivision, and so on until no more of it could possibly be formed. The phænomena attending volatilization have the same bearing as those connected with precipitation. If by the mutual action of two salts a substance be formed, which, though soluble in water, requires more water for its solution than is present, it crystallizes out: certain experiments were noted where this action occurs, and it was found that they gave testimony in favour of the same views as have been supported by the preceding observations. The bearing of the phænomenon of diffusion of salts upon the point at issue was also examined: Malaguti's experiments were discussed; and they, as well as some observations on the solution of certain bodies by others set at liberty, were found to bear testimony also in the same direction.

During the whole of the experiments on this subject, most of which were performed quantitatively, no unequivocal instance occurred of two substances having so strong an affinity for one another, that they combined to the exclusion of other bodies of like kind present in the same solution. After showing that some reputed exceptions are really not capable of being proved to be so, and after suggesting some probable limitations of the action of the general law, the paper concludes with the following deductions:—

I. That where two or more binary compounds are mixed under such circumstances that all the resulting compounds are free to act and react, each electro-positive element enters into combination with each electro-negative element in certain constant proportions.

II. That these proportions are independent of the manner in which the different elements were primarily arranged.

III. That these proportions are not merely the resultant of the various strengths of affinity of the several substances for each other, but are dependent also on the mass of each of the substances present in the mixture.

IV. That an alteration in the mass of any one of the binary compounds present alters the amount of every one of the other binary compounds, and that in a regular progressive ratio; sudden transitions only occurring where a substance is present which is capable of combining with another in more than one proportion.

V. That this equilibrium of affinities arranges itself in most cases in an inappreciably short space of time, but that in certain instances the elements do not attain their final state of combination for hours.

VI. That totally different phænomena present themselves where precipitation, volatilization, crystallization, and perhaps other actions occur, simply because one of the substances is thus removed from the field of action, and the equilibrium that was first established is thus destroyed.

VII. That consequently there is a fundamental error in all attempts to determine the relative strength of affinity by precipitation,—in all methods of quantitative analysis founded on the colour of a solution in which colourless salts are also present,—and in all conclusions as to what substances exist in a solution, drawn from such empirical rules as, that "the strongest acid combines with the strongest base."

March 15, 1855.—The Lord Wrottesley, President, in the Chair.

The following communications were read :—

"Researches on Organo-metallic Bodies." By E. Frankland, Ph.D., F.R.S., Professor of Chemistry in Owens College, Manchester. Second Memoir.—Zincethyle.

This compound, whose existence was mentioned in a previous memoir*, is formed by the action of zinc upon iodide of ethyle, or a mixture of iodide of ethyle and anhydrous ether, at a temperature exceeding 100° C. The materials are enclosed in a copper digester capable of resisting great pressure. When purified by rectification in an atmosphere of carbonic acid, zincethyle possesses the following properties :—At ordinary temperatures it is a colourless, transparent and mobile liquid, refracting light strongly and possessing a peculiar odour, rather pleasant than otherwise, and therefore differing greatly from that of zincmethyle. Its specific gravity is 1·182 at 18° C. Exposed to a cold of $-22°$ C. it exhibits no tendency to become solid. Zincethyle boils at 118° C., and distils unchanged. The specific gravity of its vapour is 4·251. Several analyses of zincethyle prove its formula to be

$$C^4 H^5 Zn.$$

The vapour volume of zincethyle is highly remarkable, and almost compels us to conclude that the vapour volume of the double atom of zinc is only equal to that of oxygen, instead of corresponding with the volume of hydrogen, in accordance with the generally received supposition. Zincethyle, therefore, appears to belong to the so-called water type, and to consist of two volumes of ethyle and one volume of zinc vapour; the three volumes being condensed to two: for if we were to assume that an equivalent of zinc occupies the same vapour volume as an equivalent of hydrogen, we should then have the anomaly of the combination of equal volumes of two radicals being attended by condensation.

Although zincethyle is remarkable for the intense energy of its affinities, which place it nearly at the head of the list of electropositive bodies, yet it does not appear to be capable of forming any true compounds with electro-negative elements, its reactions being all double decompositions in which the constituents of the zincethyle separate. Zincethyle is spontaneously inflammable in atmospheric air or oxygen: but when a few drops, diluted with ether to prevent inflammation, are passed into a mercurial eudiometer containing dry atmospheric air, a rapid absorption of oxygen takes place, with the formation of a white amorphous solid composed of zinc, ethyle, and oxygen. This reaction, which is also common to zincmethyle and zincamyle, led me to suppose that, like cacodyle, these bodies combined directly with oxygen; but the results of a closer study of the action of oxygen upon zincethyle prove that no such compound is formed; the white body being ethylate of zinc, and containing no organo-metallic compound, in the strict sense of the term. The action of oxygen upon zincethyle is expressed in the following equation:—

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* Philosophical Transactions, 1852, p. 436.

$$\left.\begin{matrix} C^4H^5Zn \\ O, O \end{matrix}\right\} = C^4H^5O\,ZnO.$$

The ethylate of zinc thus produced is decomposed by water into hydrated oxide of zinc and alcohol—

$$\left.\begin{matrix} ZnO\,C^4H^5O \\ 2HO \end{matrix}\right\} = \left\{\begin{matrix} C^4H^5O\,HO \\ ZnO\,HO. \end{matrix}\right.$$

Zincethyle is acted upon with great energy by iodine; when the violence of the reaction is moderated, by the application of intense cold and the intervention of ether, the sole products are iodide of zinc and iodide of ethyle—

$$\left.\begin{matrix} C^4H^5Zn \\ I, I \end{matrix}\right\} = \left\{\begin{matrix} C^4H^5I \\ ZnI. \end{matrix}\right.$$

Bromine acts with explosive violence on zincethyle, but the action may be moderated by adding the bromine in the form of diffused vapour and cooling to 0° C. The sole products of the reaction are then bromide of ethyle and bromide of zinc—

$$\left.\begin{matrix} C^4H^5Zn \\ Br, Br \end{matrix}\right\} = \left\{\begin{matrix} C^4H^5Br \\ ZnBr. \end{matrix}\right.$$

Zincethyle burns with a lurid flame spontaneously in chlorine gas; the zinc and hydrogen are converted into chlorides, whilst carbon is deposited in the form of soot. I have not studied the products of a more moderate action, as it is difficult to bring the materials together without too great an elevation of temperature. There can be no doubt, however, that the moderated action of chlorine would be analogous to that of bromine or iodine, and that the products would be chloride of ethyle and chloride of zinc—

$$\left.\begin{matrix} C^4H^5Zn \\ Cl, Cl \end{matrix}\right\} = \left\{\begin{matrix} C^4H^5Cl \\ ZnCl. \end{matrix}\right.$$

Carefully dried flowers of sulphur have only a slight action upon an ethereal solution of zincethyle, but the application of a gentle heat suffices to produce a brisk reaction; the sulphur gradually disappears, a white flocculent precipitate is formed, and a strong odour of sulphide of ethyle developed. The chief product of this reaction is the double sulphide of ethyle and zinc (mercaptide of zinc), which is produced as follows:—

$$\left.\begin{matrix} C^4H^5Zn \\ S, S \end{matrix}\right\} = C^4H^5S + ZnS.$$

A little free sulphide of ethyle is also formed—

$$\left.\begin{matrix} C^4H^5Zn \\ S, S \end{matrix}\right\} = \left\{\begin{matrix} C^4H^5S \\ ZnS. \end{matrix}\right.$$

Finally, zincethyle is decomposed by water into oxide of zinc and hydride of ethyle—

$$\left.\begin{matrix} C^4H^5Zn \\ HO \end{matrix}\right\} = \left\{\begin{matrix} C^4H^5H \\ ZnO. \end{matrix}\right.$$

It is also similarly acted upon by the hydrated acids and by the hydrogen compounds of chlorine, bromine, iodine, fluorine and sulphur.

The behaviour of zincethyle in contact with the electro-negative elements is highly remarkable, and cannot fail to have an important influence upon our views of the condition of bodies at the moment of chemical change,—a subject so ably discussed by Brodie*, whose ingenious views I consider to receive a new support in these reactions of zincethyle, by the singular way in which ethyle, a body low down in the electro-positive series, unites with oxygen, chlorine, &c., in the presence of a large excess of the intensely electro-positive zincethyle. This behaviour also strikingly confirms the suggestions I ventured to make in my former memoir†, relative to the moleculo-symmetrical form of organo-metallic compounds. In the inorganic combinations of zinc, this metal unites with one atom only of other elements; a very unstable peroxide, not hitherto isolated, being the only exception. The atom of zinc appears, therefore, to have only one point of attraction, and hence, notwithstanding the intense affinities of its compound with ethyle, any union with a second body is necessarily attended by the expulsion of the ethyle.

"Note on the Magnetic Medium." By Prof. A. W. Williamson.

In a letter to Mr. Faraday recently published in the Philosophical Magazine, Dr. Tyndall brings forward some important considerations on the subject of magnetic philosophy.

It has been known for some time that the phænomena of diamagnetism may be produced artificially in bodies which are usually considered magnetic. For this purpose it is only necessary to plunge the magnetic body into a yielding medium more magnetic than itself. When thus exposed to the action of a magnet it recedes from the poles, because the volume of the medium which it displaces is more powerfully attracted.

This fact naturally suggested the idea, that all repulsion by the magnet might be owing to the attraction exercised on the medium being stronger than that on the body repelled;—just as balloons are driven upwards by the superior weight of the displaced volume of air. And as phænomena of diamagnetism are observed in a so-called vacuum, it was thought that some "magnetic medium" might be present there.

I do not purpose on this occasion to enter upon the general question of the evidence which may be adduced for or against this important conclusion; for it could only be proved satisfactorily by considerations including phænomena of the most varied kind, such as electricity, light, chemical action, &c., to which it must necessarily apply. But it might be disproved by any one well-understood fact contradictory to it.

Now it appears to me, that the facts adduced by Dr. Tyndall are not inconsistent with the notion of a magnetic medium, but follow naturally from it; and that his argument involves a tacit assumption foreign to the theory under consideration.

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* Philosophical Transactions, 1850, p. 789.

† Ibid. 1852, p. 438.

The first fact adduced is, that compression increases the attraction of magnetic bodies, and the repulsion of diamagnetic bodies by the magnet, in the direction of the line of compression. Now it is evident, that a variation of pressure on a number of particles surrounded by a magnetic medium may alter the attraction of the mass by a magnet in two ways;—first, by altering the density of the matter*; secondly, by altering the density of the medium.

In a cubical mass of carbonate of iron the material particles are more magnetic than the medium which they displace, and the force with which it is attracted is proportional to this excess.

If it becomes more magnetic by compression, we must conclude that the loss of magnetic medium *from its interstices* is more than supplied by the magnetic matter which takes its place.

Carbonate of lime is less magnetic than the quantity of medium which its particles displace, and when these particles are brought closer together by pressure, with diminution of the intervening spaces occupied by the medium, the mass becomes more diamagnetic, because a certain quantity of the magnetic medium is thus replaced by the less magnetic matter.

Dr. Tyndall seems to have assumed, that on the compression of an aggregate of particles of a diamagnetic substance, the medium is not displaced by the particles in their change of position;—in which case, his conclusion, that compression must increase the magnetic functions of *every* substance, would no doubt follow from the notion of a magnetic medium.

The second fact adduced differs chiefly in form from the one just considered. Crystals of carbonate of iron are attracted most strongly by a magnet acting in the direction of the crystallographic axis. Crystals of carbonate of lime, possessing the same form, are most strongly repelled in the direction of the same axis. In this direction the functions of the matter predominate more over those of the medium than in other directions of the crystal; so that with carbonate of iron, we have the strongest magnetism; with carbonate of lime, the strongest diamagnetism in this axis. One crystal consists of magnetic medium with strongly magnetic matter; the other consists of the medium with matter of very slight magnetic force.

The crystallographic axis is in both crystals the direction in which the function of matter predominates most strongly over that of the medium; so that in the iron salt it is the most magnetic, in the lime salt the feeblest magnetic direction in the crystal.

March 22.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"Further observations on the Anatomy of *Macgillivraya*, *Cheletropis*, and allied genera of pelagic Gasteropoda." By John Denis Macdonald, Esq., R.N., Assistant-Surgeon H.M.S.V. 'Torch.'

The author states, that in a late voyage from Sydney to Moreton

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* The word "matter" is here used for brevity to denote *ponderable* matter.

Bay, specimens of *Macgillivraya*, *Cheletropis*, and a few other genera of minute pelagic Gasteropoda, apparently undescribed, were daily taken in the towing-net, and afforded him an opportunity of more precisely determining the mode of attachment of the ciliated arms which he had at first presumed to be naked branchiæ.

In his former paper* it was stated, more particularly of *Cheletropis Huxleyi*, that the gills were of two kinds, viz. "covered" and "naked;" the former, corresponding to those of the pectinibranchiate Gasteropoda generally, he has never found to be absent in any of the genera; but from further observation of the so-called naked gills, while the animals were alive in their native element, he is disposed to think that they are chiefly employed for prehension, and probably as auxiliary organs of natation. When these ciliated appendages are fully extended, the line of cilia is perfectly straight, so that the frilled border, noticed in the previous account, turns out to be a character depending simply on the partial contraction of the longitudinal muscular fibres, preparatory to complete retraction of the organs. They have no connexion with the mantle, but encircle the mouth together with the tentacula and eyes, and coalesce at their bases like the segments of a deeply-cleft calyx. In the specimens of *Macgillivraya* examined the arms were quite transparent, but marked at irregular intervals with cross streaks of brownish purple. In the extended state they were several times the length of the shell, and, like the arms of a polype, they rolled themselves up when touched, and started back into the shell with surprising rapidity. They appeared also to be exquisitely sensitive, exhibiting short twitching movements when minute particles suspended in the water came in contact with them.

In the specimens of *Macgillivraya* now referred to, the respiratory siphon consisted of a process of the mantle converted into a tube by the mere apposition of its borders without organic union; it was moreover much shorter than had been usually observed in previous examples, and the author thinks that those now under consideration may be a variety, if not a distinct species.

In his former examinations of this tribe of Gasteropoda, the author had never found more than four arms encircling the head, but he has since discovered six in a single genus with which he had been long familiar by external characters. In this case the operculigerous lobe of the foot is quite cylindrical and of some length, bearing the peculiar operculum on its truncated extremity with the clawed process pointing to the left side. The sucker-disc is very small, and presents an anterior and posterior lobe. The two tentacula bear each an ocellus on the outer side near the base, and the ciliated arms, in every respect save number, resemble those of *Macgillivraya* and its congeners. The clawed operculum is developed from a spiral nucleus situate near the internal thickened border; it seems to be a weapon of defence, and is wielded with great dexterity by the little animal, which makes skips and jerks by means of its complex foot, after the manner of *Nassa* or *Strombus*.

The author notices another member of this diminutive tribe which is very commonly met with in the South Pacific, and has almost an indefinite range. As regards both animal and shell, it in many points resembles a miniature *Natica*. The shell is few-whorled, with small compressed spire and ventricose mouth; the operculum paucispiral and well-marked with the lines of growth. The foot is not unlike a broad and square-toed shoe in form, receiving or bearing the remainder of the animal and the shell. The shoe-upper, as it were, presents two rounded lateral lobes which lie over the anterior part of the shell, like the mentum of *Natica*. The little animal creeps on its foot with great rapidity, appearing rather to slide along than progress by a vermicular movement, and by spreading out and hollowing this organ at the surface of the water, as a freshwater Lymnæad forms a boat of its foot, it buoys up its tiny body and is cast abroad on the face of the ocean.

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* Phil. Mag. p. 297.

LXXVI. *Intelligence and Miscellaneous Articles.*

EXPERIMENTS RELATING TO ENDOSMOSE. BY M. LHERMITE.

THE term *endosmose* has been applied by Dutrochet to the passage of one liquid towards another through a septum affording less resistance to the passage of one liquid than to that of the other.

From analogies which were erroneously supposed to exist between this phænomenon and the experiment made by Porret, endosmose was attributed to electrical action.

Poisson explained the phænomenon as a result of capillarity, but he did not ascribe to the attraction of solids for liquids any other function than that of determining the occupation of the numerous small channels, of which the septum may be regarded as constituted, by one liquid in preference to another, and of preventing the interruption of the liquid threads. He referred the ulterior action to the mutual attraction of the two liquids.

Dutrochet attributed, in his later memoirs on this subject, much to the reciprocal chemical action of the liquids, but he never attempted to explain the mode in which the membrane acted, always being inclined towards the electrical theory.

Mr. Graham has recently been led to the conclusion, that the alteration of the septum appears to be a condition indispensable for the manifestation of the osmotic force. According to him, one of the surfaces of the membrane is acid, the other basic, and this state is connected with the progressive decomposition of its substance.

M. Lhermite considers that he has demonstrated, by means of his own observations and those of his predecessors, that endosmose is not produced by any peculiar force, but that it is a result of affinity (chemical action), taking the widest signification of that term, and including capillary attraction.

Poisson, who treated the question as a mathematician, and not experimentally, admits that when the movement has once commenced, the septum has no further share in the action; but if the action takes place solely between the liquid A, which is actually absorbed by the septum, and the liquid B, which attracts it with a force superior to that exercised between the molecules of A, there is no reason why the movement should not take place as well in one direction as the other.

At the moment when the pores of the septum are charged with the liquid A, and if the passage of the liquid threads A towards the liquid B is considered to be more rapid than the passage of B towards the liquid threads A, on account of the smaller mass of A, it follows that when the septum is charged with the liquid B, the threads will be attracted by the mass of A, and the movement will take place in a contrary direction.

Dutrochet and Mr. Graham have pointed out, in opposition to this theory, that the elevation of liquids in capillary tubes is very slight compared with the height of the columns of liquid supported by endosmose; but these observers do not appear to have taken into account the circumstance that, in reality, the height of the liquid in ordinary tubes indicates merely the action of the liquid upon itself. This is proved by the circumstance, that the liquid column is always the same height in tubes of equal diameter, and made of any substance that is wetted by the liquid, as if the layer of liquid nearest to the surface of the tube were the seat of the action; whereas the capillary force essential in endosmose results from the action of the solid substance on the liquid.

In order that this may produce its greatest effect, the diameter of the tubes must not be more than double the distance at which the attraction is exercised; and in ordinary capillary tubes the conditions requisite for the production of an appreciable osmotic motion are far from being fulfilled.

The only weak point of Poisson's theory, so far at least as regards the porous septum, consists in the disregard of the elective action of the solid substance just at the point where its consideration becomes necessary. When the surfaces of the septum are in contact with two liquids which are miscible, one being more strongly attracted by the substance of the septum than the other, there would at the moment of contact between the liquids be an expulsion of one by the other, and a consequent motion which would cease so soon as the liquid was in contact with the septum throughout its entire extent.

Porous clay vessels have channels whose minuteness is evident from the slowness with which water passes through them, nevertheless they are inferior to liquids in the osmotic faculty. There is no doubt but that this is owing to the difference in the distance between the molecules of the latter compared with the mechanical or accidental interstices of the former. Liquids are osmotic agents *par excellence*.

When two liquids of different densities are placed in a cylindrical test tube, and separated by a third liquid of intermediate density which dissolves, in appreciable quantity, only one of the other liquids, this one will pass into the other. When, for instance, chloroform is at the bottom, above it a layer of water, and then a layer of æther, the chloroform layer gradually increases in volume; the æther layer diminishes and at last disappears; while the water layer is scarcely at all altered. Other analogous experiments might be given, and in all instances the result may be predicted according to the solubility of the substances.

It is true that the increase of pressure, which it is customary to regard as a character of osmose, is wanting in this experiment. In order that this may take place, it is necessary to render the intermediate layer fixed. For this purpose a porous vessel is saturated with the liquid intended to act as septum, and the experiment is made as if the porous vessel were the septum, the liquid which mixes most readily with the intermediate liquid being in preference placed outside, so that the principal osmotic movement may be more appreciable from the accumulation of liquid in the osmometer.

When the porous vessel is impregnated with castor oil, filled with water and immersed in alcohol, the osmose is towards the water; while in a vessel not charged with oil, the principal movement is in the contrary direction.

The absorption of a liquid by a tissue is the inverse of the solution of a solid by a liquid. Animal membrane, by virtue of the capability of absorbing water and mixing it with other liquids, is comparable with liquids themselves; but with the advantage that it may be fixed between two liquids without being separated and dispersed, as in the case of liquids with which a porous vessel is saturated.

The direction of the osmotic movement may be predicted equally well for porous vessels or animal membrane when the rapidity with which they are traversed by the respective liquids is known. The rapidity of filtration is not always proportionate to the mobility of the liquids. Membranes and even porous clay vessels are permeated by water more readily than by alcohol, notwithstanding the far greater fluidity of the latter.

M. Lhermite considers that his experiments furnish an evident demonstration of the error into which Mr. Graham has fallen in attributing osmotic motion to the chemical decomposition of the membrane, and in supposing that the motion always carries the acid towards the base. M. Lhermite has indeed found that a solution of oxalic acid, which produces the greatest effect, is a conservative agent; while, on the other hand, with a solution of alkali in alcohol and a very dilute solution of acid in water, the base is carried towards the acid through either animal membrane or porous clay impregnated with castor oil.—*Comptes Rendus*, No. 25, 1855.

ON THE LUMINOUS COAT OF THE INDUCTION SPARKS OF RUHMKORFF'S APPARATUS. BY T. DU MONCEL.

When examining these sparks in the dark, the author observed that they are surrounded with a greenish-yellow luminous coat, the thickness and form of which varies according to the strength of the current, and the nature of the wires forming the poles; when the sparks are not very long, however, the luminous coat is almost always of an oval form. It appears to belong principally to the negative pole, and is of a reddish colour on the side of this pole. It is remarkably intense, and resembles a flame when the poles are moistened with an essential oil, and its connexion with the negative pole is also then very distinct.

If the wires of the pole (which should be rather thick) be placed about five millimetres apart, and the intervening space be blown upon rather strongly with a bellows, the greenish-yellow atmosphere is seen to be drawn towards the opposite side, where it forms a large flame of a violet colour. It may also be blown along the wires of the pole, and it is then seen to be furrowed by a number of very sinuous, more or less white jets of fire, which are usually disposed in strata. The violet light appears to be circumscribed by two bundles of violet rays, which rise from the points of the poles and unite in irregular curves, like those of a flame driven by the wind. The true spark does not appear to be affected by the current of air. —*Comptes Rendus*, Feb. 5, 1855, p. 313.

ON COMPLEMENTARY COLOURS. BY H. MEYER.

Without going into the consideration of the proposed explanations of the complementary colours, I give a few experiments by which the observation of these subjective colours is much facilitated, reserving a further consideration of these phænomena, and the conclusions to be drawn from them, for a subsequent memoir.

If a narrow strip of gray paper be laid upon a coloured surface, this strip appears tinged with the complementary colour of the body on which it is lying. This experiment does not, however, always succeed equally well, and is best performed with a green surface; if the strip of paper be white and a little broader, the complementary colour is only observed after long watching, or perhaps not at all. The complementary colour may, however, be produced *immediately and quite distinctly*, even with white strips of such breadth, that, without further assistance, they cannot show the complementary colour (several inches broad) by laying a leaf of fine, transparent writing-paper over the coloured surface and white paper. The white strip *immediately* appears covered with a tolerably uniform pale tinge of the complementary colour.

A sheet of coloured paper laid beside one of white paper does not allow the complementary colour to be observed upon the latter; and it is only when the coloured sheet has been looked at for some time, and the eyes are then alternately directed from the coloured surface to the white, that the strip of the latter lying next to the coloured surface is tinged with a somewhat intense complementary colour. But if a transparent sheet of writing-paper be laid over the coloured and white surfaces, the complementary colour immediately makes its appearance upon the latter, without any previous removal of the eye from one surface to the other. If the eye be directed to the margin of the white and coloured surfaces, the portion of the white surface lying next to the coloured surface appears more intensely tinged with the complementary colour than the parts lying at a greater distance; but if the white surface be examined by moving the eye, so that the different parts of the white surface may be represented one after the other upon the same part of the retina, the surface appears covered with a more uniform tint of the complementary colour.—Poggendorff's *Annalen*, vol. xcv. p. 170.

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—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATUM.

Page 50, line 6, *for* PETER WILLIAM BARLOW *read* WILLIAM HENRY BARLOW.

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[FOURTH SERIES.]

JULY 1855.

I. *On the Induction Apparatus and its Effects.*

By J. C. POGGENDORFF*.

THE following investigation was suggested by an examination of an induction apparatus constructed by Ruhmkorff for the physical cabinet of the University of Berlin. There is no doubt that its construction and action are superior to those of any other instrument of the kind yet invented, but I feel convinced it is by no means so perfect as it may yet be made, nor have the phænomena which it manifests ever yet been sufficiently investigated in any published memoir. On this account the present investigation will be divided into two parts; the one will be devoted to the construction of the apparatus, the other to its effects.

1. *Construction of the Apparatus.*

In its present form the apparatus consists of no less than six parts; viz. 1, the coil of wire in which induction is developed, the induction coil; 2, the coil which conducts the inducing galvanic current, here called the primary coil; 3, the soft-iron core; 4, the current breaker; 5, the condenser; and 6, the voltaic battery or primary source of electricity; to which parts, lastly, some additional apparatus must be added. Each of these six elements acts, more or less, upon the remaining five; and the ultimate result, the induced current, depends upon a suitable distribution and arrangement of all.

Induction Coil.

On examining this coil, it appeared to me that hitherto it had not been constructed on proper principles. Usually the wire forms layers which extend, without interruption, from one end of the coil to the other; the number of these layers is generally even, so that the beginning and end of the wire are at the same extremity of the coil. But it is clear, that in a coil, whose points are all exposed to about the same degree of electrical excitation, the tension must increase from the middle towards each extremity. Hence, when the wire is coiled in the manner above described, points whose electrical difference is very great are brought very near one another, and consequently the springing of sparks between two such points is much facilitated, as well as the external action of the coil weakened.

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* From Poggendorff's *Annalen*, vol. xciv. p. 2.

This imperfection appeared susceptible of partial remedy by dividing the length of the coil into several parts, and by covering each part with wire in the same manner as the whole coil was before covered, using, however, an odd instead of an even number of layers in order to carry the wire from one part to another, and at last to let the ends run out at opposite extremities of the coil.

Further, in order to obtain effects of great tension, it was evident that the wire must be well insulated. As far as I know, whenever a second insulator has been employed, to aid the covering of silk generally used and never to be dispensed with, a solution of shell-lac in alcohol has been chosen. But in the present case, where the wire, owing to its great length, cannot well be varnished other than lengthwise, such a method only imperfectly fulfils its purpose, because under such circumstances the varnish dries extremely slowly, and perhaps never perfectly; so that a certain degree of conductibility, even greater than that with silk alone, always exists.

Instead of the alcoholic solution of shell-lac, an easily fusible insulator (spermaceti, stearic acid, or a mixture of wax and oil*) appeared preferable. In applying the same, it requires to be heated above its melting-point in order to prevent its immediate solidification when in contact with the cold layers of wire, and to allow it to permeate and thoroughly saturate the silk covering.

Again, in order to obtain a good exterior insulation, the wire was coiled around a glass cylinder, at whose extremities were rims of gutta-percha in which the ends of the wire could be well fastened; besides this, the whole coil was provided with a thick covering of wax; and lastly, after this had been varnished, two strong rings of gutta-percha were affixed, by means of which the whole could rest on a wooden stand.

According to these principles, I have constructed three induction coils, each of which is 5¾ inches long†, has an internal diameter of $1\frac{5}{6}$ inch, and an external one of $2\frac{2}{3}$ inches. Two of these contain an extremely thin wire, 0·15 millim. in diameter, and the third a considerably thicker one, 0·25 millim. in diameter. In each of the two first the wire is about 10,000 feet long, and makes about 16,000 windings, in eight parts, each of which contains 33 layers. The wire of the third coil is about 2400 feet long, and each of its parts contains 19 layers. For certain purposes a fourth coil containing about 1200 feet of the thicker wire was constructed; its diameter was such as to permit of its being inserted in one of the primary coils to be afterwards described.

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* Paraffine would be perhaps still better, as it may now be easily procured, easily melts, and is, according to Riess, a good insulator.

† Throughout the memoir the inches and feet are Parisian.

Although the greatest possible care was taken in the preparation of all these coils, and although they are, perhaps, more effective than others of equal dimensions constructed according to the usual method, still the desired object is not yet perfectly attained; on the contrary, the conviction forced itself upon me that the insulation was far from sufficient to protect the coil from the disturbing effects of internal sparks.

This was particularly perceptible in the second of the coils formed of fine wire. Immediately after its construction, the phænomena of sparks and tension which it exhibited were extraordinary, indeed wonderful, but after a quarter of an hour its action considerably decreased. This condition remained unchanged for months, and for my purposes was satisfactory enough, for by employing two pairs of Grove's battery I could obtain a spark at a distance of almost 12 millims. In all probability this condition would have been permanent had I not afterwards had occasion to use four pairs of Grove's battery. With this number I obtained sparks at a distance of 16 millims. and upwards, but only for a few minutes; afterwards the distance decreased considerably, and at length, when two pairs were again used, the distance, instead of being 12 millims. as before, was only 9·6 millims.

From this we may conclude, that in order to maintain an induction coil in good condition, it must not be strained too much,—at least when the current is broken so as to obtain sparks in air of ordinary density, because in such cases the electricity excited possesses great tension, and strives everywhere to break open a path between the windings of the wire. Of course such a deterioration of the coil need not be feared in experiments where the poles of the induction coil are connected by a pretty good conductor, for then the tension is either not so great, or is not at all present. In such cases, too, the wire need not be so perfectly insulated. For example, the phænomena of light in a partial vacuum are just as good when the wire is tolerably well insulated, as when the insulation is as perfect as possible;

nor does an increased length of wire add anything to the brilliancy of the phænomena as soon as the resistance of the partial vacuum is small in comparison to that of the induction wire. Without doubt the induction coils for obtaining sparks and other phænomena of tension would be more effective and durable if the thickness of the insulating medium were increased. But even if the length of wire might then be reduced to some extent, the coil would have to be large in size in order to produce the desired effects, the inducing coil would have to be increased to a corresponding extent, and a stronger voltaic battery would be requisite; in short, the apparatus would lose much of its present convenient dimensions for practical purposes.

I believe the above-mentioned imperfections might to a great extent be remedied,—1, by not using too thin a wire (for most purposes wire of $\frac{1}{4}$ millim. diameter would, perhaps, be best); 2, by giving the wire a very thick covering of silk; and 3, by using a liquid instead of a solid insulating medium, such as a fat or essential-oil, *e. g.* rectified oil of turpentine. By this means, at least, the gradual deterioration of the coil would be prevented, for the openings broken through the silk covering of the wire by the sparks would be always stopped up again by the liquid.

The number of parts on the coil might be greater than that I have chosen; the whole coil might be composed of loose and, of course, well insulated rings of wire. In one respect this would be advantageous; the rings when defective could be replaced by new ones, but the apparatus would be thereby rendered very complicated on account of the number of connecting clamps that would be required. Neither can we expect any particular advantage from different combinations of these rings of wire, for they are elements of unequal value in consequence of their different positions with respect to the inducing coil. If it be required to change the length and thickness of the wire in order to study their influence upon the phænomena, it will be found more convenient to prepare complete apparatus of smaller dimensions (say four similar to that of my third induction coil), and then to combine them as it may be found desirable.

Lastly, I am inclined to believe that the usual cylindrical form, which I have also adopted hitherto, is by no means the most advantageous for the induction coil. A solenoid or spindle shape would be better, because the inducing coil and the soft iron core are most effective in the middle. I have not yet had time, however, to try all these tedious changes in the apparatus.

Primary Coil.

I have generally used three inducing coils, all of which were exactly alike. In construction they differ little from the common form, except that they contain two insulated copper wires which can be used either singly or combined, side by side or one after the other.

The wire is 1 millim. thick, and each of its halves about 100 feet long. The tube upon which the wire is coiled in four layers is of mill-board in two instruments, and of glass in the third, and has an internal diameter of $1\frac{1}{3}$ inch.

In some cases I have also used a fourth primary coil with wire only 0·67 millim. in diameter, and nearly 400 feet long; this was coiled on a mill-board tube.

It may be here mentioned, that, under otherwise equal circumstances, the inducing action of a current, just as its magnetic action, is a product of its intensity and length; hence a short, thick wire and great intensity of current may sometimes be advantageously replaced by a long, thin wire, and small intensity of current*. The last is preferable when the action requires to be long sustained, because the voltaic battery is less attacked; on the other hand, however, the inner induction current, the so-called extra current, increases in intensity, and leads to disadvantages, of which more hereafter.

Soft Iron Core.

Usually this is formed of pretty thick wires, or we may almost say, small bars, held together by a common cap, but yet separated from each other by some insulating medium.

In some cases I have employed such a collection of bars, but oftener I used cores of much thinner wire; on the one hand, because this thinness must tend to increase the action, and on the other hand, because I was enabled to construct such cores for myself. The wire was only 0·25 millim. in diameter. It was heated to redness, cut into pieces of convenient length, bound in a bundle with silk thread, and lastly, surrounded with a paper covering.

I found it unnecessary to cover the wires with a varnish, partly because the stratum of protoxide which forms on its surface, when heated, is comparatively a bad conductor; partly because the irregularity of the spaces between the wires, none of which remain straight, is more than sufficient to destroy the pernicious continuity in the direction of the periphery of the core.

If care be taken not to diminish the interstices too much by unnecessary tightening, such a soft iron core will be as effective as, or more so than, one of equal weight prepared in the most careful manner by means of strings and individually insulated bars. Two of the cores which I prepared are a little longer than the induction coil, 6 inches long and 13 ounces in weight. A third is three times as long, *i. e.* 18 inches. Each of them contains 4200 wires of the given dimensions.

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* The voltaic battery and the volume of the inducing wire of an induction coil being given, the maximum inductive effect is obtained when the resistance of the inducing wire is equal to the residual resistance in the battery; the action of the extra current, however, is here neglected. Vide Poggendorff's *Annalen*, vol. lv. p. 45.

I convinced myself, however, that this number may be diminished without injury to the action of the coil, in which case, of course, the primary coil will remain partly unoccupied. In fact, I found that a hollow core of the above-mentioned wires, which contained an empty cylindrical space three-quarters of an inch in diameter, and which weighed only half as much as the massive ones, was just as effective as the latter. It surprised me to observe that when a massive bar of soft iron was introduced into the hollow axis of this core, the action of the induction coil was not thereby diminished; whereas the same soft iron bar introduced alone, in absence of the hollow core, unmistakeably diminished the action which the primary coil itself exercised upon the induction coil. This fact is evidently analogous to one observed several years ago by Magnus, viz. that a massive bar of iron, when introduced into a slit tin tube, weakened but little the action of the latter*.

Practically the soft iron core is of great use, theoretically it causes much complication. If it were dispensed with on this account, it would be necessary to give the apparatus a different form in order not to diminish its action. It would be necessary to increase the diameter of the inducing coil, and to arrange the induction wire half inside it and half outside it, in order to make use of both sides of the inducing wire. The soft iron core has a strengthening influence, because, by a kind of reflexion, the inner side of the inducing coil is made to act externally.

Current-Breaker.

For breaking the current I employed as usual a Neef's, or rather Wagner's hammer, of which I possess two. One is, on the whole, constructed in the usual manner, *i. e.* a fixed pin descends, and against it, from below, beats a spring tongue with a small plate fixed to it. According to an improvement introduced by Riess, there is a second pin underneath the tongue, in order to make use of the downward vibrations for closing the circuit; so that the primary current can not only be doubly broken, but also reversed†. These effects can be accomplished in my instrument by means of a single galvanic battery in consequence of the two wires around the primary coil, the combinations being so arranged that the current must pass alternately along the one and along the other wire, in the same or in opposite directions.

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* Poggendorff's *Annalen*, 1839, vol. xlviii. p. 105.

† Ibid. vol. xci. p. 290.

I have often employed the second pin, and a thick, angular-shaped wire, which could be screwed to it and made to press against the tongue in order to displace its fulcrum, to shorten the vibrating part, and thus quicken its motion. Too quick a motion of the tongue, however, combined with an amplitude so small that the vibrations are scarcely perceptible, and the noise similar to that of the humming of a bee, is prejudicial to the action of the induction wire.

This instrument also possesses a contrivance, by means of which, besides the first current, a second can be simultaneously, and at equal intervals, interrupted.

Another instrument was employed to break the current within a liquid. For this purpose the small electro-magnet was placed above the armature of the tongue. The tongue carries the pin or hammer; the anvil (a thick platinum wire) is placed in the middle of a small glass cylinder which rests on a brass pedestal. The platinum wire is screwed into this pedestal, and around it the glass cylinder is cemented with a layer of sulphur, which is well adapted to this purpose, because, when cold, it resists the greatest number of liquids*. In other respects this current breaker is constructed in the usual manner, and, as in the first instrument, the parts which strike against one another are of platinum, though for certain purposes I have used silver and other metals instead of platinum.

In most, if not all, induction apparatus the current-breaker forms an inseparable part of the whole; and in some, as in Ruhmkorff's, it is set in motion by the same soft iron core which increases the action of the inducing current. I preferred not only to produce this motion by a small separate electro-magnet, as in Halske's apparatus, but also to have the current-breaker constructed as a separate instrument, in order to connect it at will with other instruments, and, if need be, to use it under the air-pump.

Neef's hammer is, perhaps, the simplest contrivance for producing periodic interruptions of a galvanic current; but for certain purposes, of which I intend to speak another time, it has one imperfection,—it does not produce uniform interruptions, the time during which the circuit is broken is longer than that during which it is closed, and that because the tongue vibrates with its whole length downwards from the pin, and only with a part of its length upwards. In this respect Neef's hammer differs from the rotating current-breaker, where a straight electro-magnet oscillates horizontally on a point between the upright arms of a steel horseshoe magnet, and where the ends of the encircling wire dip into two semicircular mercury cups, connected with the poles of a galvanic battery, and separated by an insulating partition. Here the pauses are of much shorter duration than the moments of activity of the current, because the former occur only when the ends of the wire pass over the partition. The construction of this rotating current-breaker might, indeed, be so changed as to make the pauses equal in duration to the periods of activity; but to do so would be to render the instrument more expensive, and as this equality was not necessary in the present investigation, I have continued to use Neef's hammer.

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* For this reason I have for many years used sulphur for cementing the platinum in the covers of my Grove's battery.

Condenser.

At first my condenser was exactly similar to the one constructed by Ruhmkorff, probably according to Fizeau's own instructions; it consisted of a long piece of oiled silk covered on both sides with tinfoil, and folded to and fro in layers so as to occupy less space, and to allow only parts of one and the same sheet of tinfoil to touch each other. Each of these sheets of tinfoil was 8 feet long and 11 inches broad. Soon afterwards a second condenser, about one-fourth the size of the first, viz. 23 inches long and 11 inches broad, was used; the folded layers were separated from one another by mill-board in order to prevent contact between the posterior surfaces of the tinfoil, which, if present, would necessarily be prejudicial.

Afterwards I became acquainted with the simplification proposed by Halske, wherein the voluminous arrangement in Ruhmkorff's apparatus is replaced by a single covered sheet of mica not greater than the octavo form. I have had an opportunity of testing a condenser of this kind and of this magnitude prepared by Prof. Riess, and I thereby convinced myself, that, under the circumstances under which the test was applied, its effect was very nearly equal to that of the great oiled silk condenser.

This great superiority of the mica condenser, in comparison to its magnitude, is caused by one of the sides of the condensing surfaces being in free contact with the air, and by the two surfaces being nearer to one another than was the case with oiled silk, which, besides being somewhat thicker than the plate of mica, is so bent and folded as to prevent the close proximity of the sheets of tinfoil.

Mica is an excellent substance for such condensers, but it cannot readily be procured in plates of the requisite size. This caused me to search for substances capable of replacing it, amber varnish, or a solution of shell-lac in alcohol; thin wax-paper, such as may be procured from apothecaries, and covered with lac varnish. When rigidity, which these preparations possess only in a small degree, is not required, they are fully capable of replacing mica.

I have prepared many such paper condensers, some containing 54, others 30 square inches of covered surface. I have tested them singly and in pairs, connecting them in the latter case by a strip of tinfoil, and I have found that two together are not more effective than one, and that even one of the smaller kind was almost as effective as the great condenser of oiled silk. In order to discover how far this diminution of the condensers could be carried without injury to their actions, others were prepared containing on each side 16, 9, 4, and 1 square inches of covered surface; some were made of varnished wax-paper, others of gutta-percha, and others of oiled silk, upon which, owing to their small size, the sheets of tinfoil could be easily and effectually fastened by means of shell-lac varnish. To my astonishment, I found that all these small condensers, the smallest not excepted, were, with respect to the *length of the induction spark*, just as effective as the two great ones of oiled silk; at least the differences were very irregular and very trifling, so that, without difficulty, they could be ascribed to the changes which the parts of the contact-breaker which hammer against one another suffer, owing to the sparks which are produced between them. The sole advantage which the great condensers possessed over the small ones was, that the former produced less noisy sparks than the latter. With the former, too, the sparks followed one another more quickly when the distance between the poles approached the limit at which the sparks can be obtained singly, or even at all. On this account, therefore, the great condensers are not superfluous, nor are they so for other reasons. First, all the results mentioned were obtained with a current from one or from two pairs of Grove's battery; and the comparison of both cases appeared to indicate, that, by employing a more intense galvanic current (to which, however, I intentionally would not expose my induction coil), the relation between the actions of the different condensers would change. Secondly, in the induction apparatus, the action of the condensers is essentially influenced by the *extra current*, i. e. the induction current which is produced in the circuit of the inducing current itself, when it is broken.

All that has hitherto been said applies only to the case where this *inner* induction current is comparatively weak, consequently to the case where the primary current traverses a comparatively short and thick wire. If the former current is strengthened by conducting the latter through a long and thin wire, without at amongst which were post-paper, covered on both sides with the same time changing anything connected with the source of electricity or the soft iron core, the action of all small condensers considerably decreases, and that the more the smaller they are, until in the smallest no action whatever is observable; the great condensers, however, retain their force undiminished. I convinced myself of this by so combining the wires of the primary coil as to cause the current, instead of traversing two wires side by side, to traverse one after the other; therefore, instead of a wire of double section and single length, one of single section and double length was employed. The phænomenon was still more perceptible when the latter wire, which was now 1 millim. thick and 200 feet long, was replaced by a wire ⅔rds of a millim. thick and 400 feet long, without changing anything else about the apparatus. In both cases where the *primary current* was comparatively weak, and the inner *induction current* comparatively strong, the condenser of 1 square inch surface, which was before so effective, was now almost without effect.

Thirdly, the action of condensers depends essentially upon the *induction coils* on which their influence is to be exerted. All the results before mentioned were obtained with a very thin induction wire, 10,000 feet long; when the thicker wire, 2400 feet long, was employed, the small condensers again manifested a very small action, and sometimes none whatever; indeed, the two smallest condensers appeared not only not to strengthen the development of sparks between the poles of the induction coil, but actually to weaken the same. This phænomenon manifested itself equally when the galvanic current traversed a short and thick, or a long and thin wire. On the contrary, however, the two large condensers of oiled silk still retained, in both cases, their former energetic action; indeed it was clearly perceptible that the condenser 8 feet in length was superior to the one 2 feet long.

Hence the stronger the galvanic battery, the longer and more conducting the circuit of its current, and the thicker and more moderate the induction wire; in short, the more intense and energetic the inner induction or extra current is made, the greater the condenser must be in order to obtain an energetic development of sparks between the poles of the apparatus*. When the extra current is very intense, it passes through the condenser the more easily the smaller its surface; and from the sparks on the current-breaker, it is easy to see that the actual outer induction current has an influence upon the weakening of the extra current, for under all circumstances these sparks are smaller with a long thin induction wire than with a short thick one.

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* I may also add, that the degree of insulation of the induction wire appeared to exercise an influence upon the action of the condenser; this action appeared to increase the more imperfect the insulation became.

What has here been said of sparks in free air, is partially true also for the phænomena of light in a partial vacuum. Under otherwise equal circumstances they are not so fully developed with small, as with large condensers.

Nevertheless it must be here remarked, that in general the strengthening action of condensers is less prominent the more intense the induction current itself, and the more perfect or conducting the vacuum. When the current in the induction wire was excited by two of Grove's elements, and the receiver exhausted to a few lines of mercury pressure, the great condenser itself scarcely exerted any influence upon the light produced; on the other hand, however, this influence immediately manifested itself when a little air was introduced, or when the long thin induction wire was replaced by the short thick one.

In all condensers, the wires which connected the same with the hammer were always at a short distance from, and on opposite sides of, the point of interruption. They can be connected with other and more distant points of the circuit, but by so doing their action is weaker.

Voltaic Battery.

On this subject I have only to remark, that, owing to the small dimensions of my apparatus, I never found it necessary to use more than two of Grove's elements, all the phænomena which I wished to study being sufficiently developed with that number.

Additional Apparatus.

Amongst these may be mentioned the small instrument, which I will call a discharger. It is requisite in order to observe many of the phænomena of the induction apparatus with convenience and without being exposed to electric shocks. On the whole, my discharger has somewhat the form of a spark micrometer, but its use is more general; the platinum wires, carried by glass rods and connected with the poles of the induction coil, are, in the horizontal position, capable of a horizontal, vertical, and rotatory motion; and, in the vertical position, they can be placed side by side, or one above another. These wires, on whose extremities thinner wires, knobs, or discs can be fastened, serve also, when in a vertical position, as electrodes in the chemical decompositions which can be effected by means of the induction current.

The second apparatus of this kind is known as the electric egg, whose construction needs no description.

I have employed two, of unequal dimensions. The axes of the greater are $10\frac{1}{2}$ and 7 inches in length, and in it points, spheres, and discs can be introduced. The axes of the smaller one are $4\frac{1}{2}$ and 3 inches in length. For most experiments the latter is sufficient, and besides this, it possesses the great advantage of being exhausted, to a few lines of pressure, by means of a few strokes of a good air-pump.

I have also employed an extra plate, such as is furnished with most air-pumps, but modified so as to suit the institution of electric experiments in a vacuum. A glass disc is cemented upon the brass disc; in both two holes are made, in which two thick copper pins are fastened so as to be air-tight, and yet not to touch the brass; these pins project above and below, are 6 inches distant from one another, and their extremities are provided with holes and clamp-screws, so as to receive the necessary connecting wires. By means of the glass disc, many experiments can be made which it would be impossible to make in the egg; for upon it several kinds of apparatus can be placed, and over all a bell-shaped receiver can be inverted in order to surround all by a vacuum. The arrangement is also more air-tight than the egg, as it contains no stuffing-box, which, however, should motion be required during the experiment, can be added to the glass receiver.

[To be continued.]

II. 1. *Description of a new Meteoric Iron from Chili, containing* Native Lead. *By* R. P. Greg, *Esq.*[*]

2. *Fall of a large mass of Meteoric Iron at Corrientes in South America*[†].

1. A SHORT time since I purchased a mass of meteoric iron weighing upwards of 17 lbs.; its shape was irregular and cup-like, considerably convex or hollowed out on one side, and the external surface more or less covered with small angular and conchoidal projections. It was found by Mr. Greenwood, Reporter of Mineral Properties, on the 26th of February, 1840, on the desert of Tarapaca, 80 miles N.E. of Talcahuaxo, and 46 miles from Hemalga; and was afterwards analysed, in 1853, by Mr. George Darlington, of the Museum of Practical Geology, with the following results:—

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[*] Communicated by the Author.

[†] From the Liverpool Literary and Philosophical Society's Journal.

	a.	*b.*
Iron	93·41	93·48
Nickel	4·62	4·56
Cobalt	0·36	0·37
Manganese	0·20	0·18
Phosphurets	1·21	1·26
Chromium	trace	trace
	99·80	99·85

In general composition it therefore closely resembles the majority of meteoric irons hitherto analysed. I am unable at present to say if it contains *Schreibersite*.

The specific gravity of a slice weighing 6 oz., containing, however, cavities, and other matter, I found to be about 6·5. For meteoric iron it is perhaps unusually soft; and though it shows no regular *crystalline* or Widmannstättian figures when a polished surface is treated with nitric acid, yet there is apparent a slightly welded or mottled texture, brighter in some parts than in others.

It was not, however, until the iron had been cut up into slices for the purpose of polishing and for exchange, that I discovered it had not a perfectly homogeneous structure, but was in many places more or less honeycombed with cavities, some of which actually contained what appeared to be pure *lead*! In some the lead was not larger than a pellet, and did not fill the entire cavity which contained it; in others the entire cavity was filled with lead, in size equal to a pea. Professor Shepard of America, who is so well acquainted with meteorites, along with Dr. Heddle and myself, saw some slices of this iron slit in the workshop of Mr. Young the lapidary, at Edinburgh, and we took lead out of the cavities immediately after they left the lathe, so that there could be no deception whatever.

To be quite certain, my friend Dr. Heddle examined some of it, and found it to be chemically pure lead; when the tarnished surface was not scraped off, small quantities of iron and alumina, and mere traces of phosphorus and magnesia, were found.

There are also two other substances in some of the cavities of this singular iron, which Dr. Heddle, when he has analysed, will separately report upon; the one a very hard, grayish-black, semimetallic mineral; the other yellowish-brown, insoluble in acids, and with an earthy texture.

This is the first authentic instance of the existence of lead in meteoric bodies, and to find it so closely allied with, and buried, as it were, in metallic iron, is not only in itself singular, but difficult to account for. It is, however, probable that the lead was originally held in alloy along with the nickel and cobalt, and on intense heating or partial fusion of the iron mass, "sweated" out into vesicular cavities.

Should this be a correct view, it is a proof of the intense heat to which iron meteoric masses appear to have been subjected at the time of, if not previous to, their reaching the surface of the earth. Indeed meteoric *stones* seem to have been subjected to a much smaller degree of heat while falling, than *iron* masses, if we may judge by appearances, the only sign of fusion in *stones* being quite external, and merely marked by a thin, black, and shining crust.

Iron falls are extremely rare compared with what are called *stone* falls; so much so, indeed, that there are not more than three or four authentic accounts of the fall of *iron* masses, and these not large ones, bearing no comparison to the enormous masses weighing from 5 to 20 tons, which have been occasionally found on the plains of Mexico and South America. See Phil. Mag. for Dec. 1854.

2. Should any one be inclined to doubt the fact that such immense masses of iron have not, strictly speaking, a meteoric origin, I have added to this paper some particulars of the fall of one in South America in the year 1844, that first appeared in a philosophical journal some years ago, but which, from having a local* circulation, has not received the notice which it so eminently deserves.

It is important to notice the state of intense fusion exhibited by the entire mass at the time of falling; Mr. Symonds's description is most graphic. The account was given in a letter, read by Mr. Dickinson, from the observer of the phænomenon, a Mr. H. E. Symonds; and the following is an extract:—

"Having been deeply engaged in Argentine politics and wars in 1843 to 1844, I accompanied the Corrientine army in its invasion of the province of Entre Rios. This army returned from that expedition in January 1844. Our rear, in which I marched, was so continually harassed by Entrerian skirmishers, that for ten days before we had gained the Corrientine frontier we had no time to sleep or change clothes; but soon after passing this, in Carritas Paso, on the river Mocorita, we placed a guard in the pass, and deeming ourselves secure, the whole division abandoned itself to the profoundest sleep.

"From this sleep we were all simultaneously awakened at about two o'clock in the morning; and as if actuated by electricity, each individual of our division (about 1400 men) sprung on his feet at the same moment. An aërolite was falling. The light that accompanied it was intense beyond description. It fell in an oblique direction, probably at an angle of about 60° with the earth, and its course was from east to west.

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* See Proceedings of the Literary and Philosophical Society of Liverpool, No. 7. Session 1853.

"Its appearance was that of an oblongated sphere of fire, and its tract from the sky was marked by a fiery streak, gradually fading in proportion to the distance from the mass, but as intensely luminous as itself in its immediate vicinity. The noise that accompanied it, though unlike thunder, or anything else that I have heard, was unbroken, exceedingly loud and terrific. Its fall was accompanied by a most sensible movement of the atmosphere, which I thought at first repellent from the falling body, and afterwards it became something of a short whirlwind. At the same time I and my companions all agreed that we had experienced a violent electric shock; but probably this sensation may have been but the effect on our drowsy senses of the indescribably intense light and noise. The spot where it fell was about one hundred yards from the extreme right of our division, and perhaps four hundred from the place where I had been sleeping. Accompanied by our general (Dr. Joaquin Madauaga), I went within ten or twelve yards from it, which was as near as its heat allowed us to approach.

"The mass appeared to be considerably imbedded in the earth, which was so heated that it was quite bubbling around it. Its size above the earth was perhaps a cubic yard, and its shape was somewhat spherical; it was intensely ignited and radiantly light, and in this state it continued until early dawn, when the enemy, having brought his artillery to the pass, forced us to abandon it to continue our march. I may mention, that, at the time of its fall, the sky above us was beautifully clear, and the stars were perhaps more than usually bright; there had been sheet lightning the previous evening.

"I never afterwards had an opportunity of revisiting the Mocorita, for our permanent encampment was thirty-five leagues to the north of that pass, between which and our encampment the country was entirely depopulated by our long war; but as the spot where the aërolite fell was known to many of our subaltern officers, who were frequently sent to observe the frontier of Entre Rios, I have heard them describe it as a 'piedra de fiérro,' *i. e.* a stone of iron; and I once provided one of the most intelligent of them with a hammer in order that he might bring me a sample of it. On his return, he told me it was so excessively hard that the hammer bent, and was broken in unsuccessful attempts to break off a small piece of it."

III. *On the Relation of the Transmission of Radiant Heat through Crystals to the direction of Transmission.* *By* H KNOBLAUCH, *Professor of Natural Philosophy in the University of Halle**.

IN a memoir, a full translation of which is given in the last volume of the Scientific Memoirs, M. Knoblauch showed that radiant heat passed in different quantities in different directions through certain uniaxal crystals; and also that the quality of the heat, after having traversed the crystals in different directions, was different. In the present paper he extends his researches to biaxal crystals.

The transmission of radiant heat through a number of plates of biaxal crystals, which were all cut perpendicular to a line bisecting the acute angle enclosed by the optic axes (the "middle line"), was first examined. A beam of light was conducted by the mirror of a heliostat into a darkened room, and permitted to pass through a Nichol's prism before reaching the crystal.

When the principal section passing through the obtuse angle of the prism was vertical, the rays issuing from the prism were polarized in a horizontal plane; and when it was required to polarize them in a vertical plane, the prism was simply turned through an angle of 90 degrees. Having traversed the crystal, the rays fell upon a thermo-electric pile, and the deflection produced was a measure of the amount of heat transmitted.

Operating in this manner with *yellow heavy spar*, M. Knoblauch found that when the plane of polarization of the rays enclosed an angle of 90° with the plane of the optic axes, the heat passed in greater quantity in the direction of the middle line than when both planes were coincident, the ratio of the latter quantity to the former being 100 : 112.

The *quality* of the heat which had passed through the crystal was next examined, and proved to be the same, whatever the plane might be in which it was polarized. This result was obtained by introducing a diathermanous body in the path of the rays between the crystal and the pile: it was found that the quantity falling on the diathermanous body always bore the same ratio to the quantity transmitted by it, no matter how the rays might have been polarized.

The next crystal examined was a *reddish-yellow topaz*, and the result was different from that obtained in the case of heavy spar. In topaz the heat travels in greatest quantity in the direction of the middle line, where the plane of polarization coincides with the plane of the optic axes. The ratio of the respective quantities found by M. Knoblauch was as 100 : 96.

* Abstracted from Poggendorff's *Annalen*, vol. xciii.

Qualitative differences were also observed in this case, dependent on the position of the plane of polarization. Glasses of different colours were used to test the quality of the heat. Calling the quantity falling upon the pile previous to the introduction of the glass 100, the ratio of this quantity to that which passed through the glass in the respective cases is given in the following table:—

	Planes coincident*.	Planes crossed.
Blue glass.........	100 : 44	100 : 34
Yellow glass......	100 : 58	100 : 60
Green glass	100 : 11	100 : 16
Red glass	100 : 49	100 : 45

We must conclude from this table that the heat whose plane of polarization coincides with that of the optic axes, possesses, after its passage through the topaz, a greater power of traversing the blue glass than the heat whose plane of polarization is perpendicular to that of the optic axes. The former, however, passes through the green glass in less quantity than the latter.

These differences were exhibited in a far more striking manner in the case of a plate of *yellow-green hornblende*. For the sake of brevity, we will call the quantity of heat which passes through the crystal when the planes of the optic axes and that of polarization are coincident, Q, and the quantity which passes when the planes are at right angles to each other, Q′. In the case of hornblende, the ratio of Q to Q′ was as 100 : 133.

Qualitative differences also exhibited themselves here, as is manifest from the following table:—

	Planes coincident.	Planes crossed.
Red glass	100 : 61	100 : 53
Green glass	100 : 4	100 : 17
Blue glass	100 : 29	100 : 41
Yellow glass	100 : 97	100 : 71

We here observe, that the rays which pass in greatest quantity through the red and yellow glass are the most intercepted by the green and blue. We also see that a characteristic difference exists between the heat issuing from hornblende and that issuing from topaz. For the heat whose plane of polarization coincides with that of the optic axes, possesses, in the case of topaz, a higher power, and in the case of hornblende a lower power, to radiate through the blue glass, than the heat whose plane of polarization crosses that of the optic axes.

In the case of a plate of *pistacite* the ratio of Q to Q′ was found to be as 100 : 146.

* *i.e.* the plane of polarization and the plane of the optic axes.

The qualitative differences that were observed are exhibited in the following table :—

	Planes coincident.	Planes crossed.
Red glass	100 : 66	100 : 44
Green glass	100 : 12	100 : 16
Blue glass	100 : 47	100 : 36
Yellow glass	100 : 53	100 : 59

It is worthy of remark, that with the same position of the plane of polarization and of the optic axes, the heat, in the case of hornblende, passes worse through blue glass and better through yellow glass; and in the case of pistacite, better through blue glass and worse through yellow glass, than the rays for which the planes enclose an angle of 90°. In the case of red and green glass this antithesis is not observed.

M. Knoblauch next placed two plates of pistacite together, both cut at right angles to the middle line, and examined their effect upon the passage of heat in the direction of the said line, first, when the planes of the optic axes of both plates were coincident; and secondly, when they were at right angles to each other; the proportion of the quantity transmitted were as 100 : 130, the former representing the quantity transmitted when the planes were at right angles to each other, and the latter the quantity transmitted when they were parallel.

Qualitative differences were also exhibited, as shown by the following table :—

	Planes of the optic axes of the two plates.	
	Perpendicular.	Parallel.
Red glass	100 : 62	100 : 40
Green glass	100 : 10	100 : 24
Blue glass	100 : 42	100 : 27
Yellow glass	100 : 41	100 : 88

In these experiments the Nichol's prism was removed, and the light direct from the mirror of the heliostat was permitted to pass through both plates successively.

A plate of *brown mica* was next examined, in the same manner as the heavy spar, topaz and hornblende. In this case the ratio of Q to Q′ was as 100 : 109.

The power of the heat to pass through glasses of different colours is exhibited in the following table :—

	Planes coincident.	Planes crossed.
Red glass	100 : 52	100 : 43
Green glass	100 : 14	100 : 17
Blue glass	100 : 42	100 : 37
Yellow glass	100 : 61	100 : 67

In this case, the differences, both quantitative and qualitative, in both groups of rays are evident.

The ease with which plates of mica of any required thickness may be obtained, rendered this body peculiarly adapted to show the manner in which the differences exhibited above were affected by the thickness of the plates.

In the first place, a plate was chosen of about one-third the thickness of the plates made use of in the foregoing experiments. The proportion of Q to Q′, that is, of the quantity transmitted when the plane of polarization was parallel to that of the optic axes, to the quantity transmitted when both planes were at right angles to each other, was as 100 : 107.

When, however, instead of sending the heat through one plate of mica, it was sent through a number of plates whose total thickness amounted to about six times that of the single plate, under the same circumstances the proportion of Q to Q′ was as 100 : 119; a proof that the quantities of the two systems of rays polarized at right angles to each other differ the more from each other the greater the thickness of the crystal through which they have to pass.

To ascertain the influence of thickness upon the quality of the transmitted rays, M. Knoblauch proceeded as follows:—Having determined the rate of the quantities of heat transmitted direct from the plate of mica to the thermo-electric pile, a diathermanous body, a green glass, for example, was introduced between the mica and the pile, and the effects produced when the planes were coincident and at right angles to each other were compared. If the ratio observed, as before the introduction of the glass, showed itself here also, it would be a proof that equal proportions of both groups of rays were transmitted through the green glass. If the ratio were changed, it would show that the two systems of polarized heat had traversed the green glass in unequal quantities, and were therefore qualitatively different from each other after their passage through the mica.

It was found, in the case of a single plate of mica, that the proportion of the two quantities mentioned, before entering the glass, was as 100 : 107. After passing through the glass the proportion was precisely the same. After passing through about three times the thickness of mica, the proportion, before reaching the glass, was as 100 : 120; but after passing through the green glass, as 100 : 170. Doubling the last thickness, the proportion before entering the glass was as 100 : 126, but after leaving the glass it was as 100 : 284.

It thus appears that the difference of transmission of the two differently polarized groups of rays, through the same diathermanous substance, may be increased from zero to a very great amount, by simply augmenting the thickness of the crystal.

Similar experiments were made with red, blue and yellow glass. The results are contained in the following table:—

		Thin plate of mica.		Thick plate of mica.	
		Planes coincident.	Planes crossed.	Planes coincident.	Planes crossed.
Red glass	Before passage through ...	100	101	100	128
	After passage	100	99	100	117
Green glass	Before passage through ...	100	107	100	126
	After passage	100	107	100	284
Blue glass	Before passage through ...	100	111	100	125
	After passage	100	105	100	104
Yellow glass	Before passage through ...	100	112	100	124
	After passage	100	113	100	135

From this table we derive the general conclusion, that the qualitative difference of the rays which we have here compared become more evident as the thickness of the crystal through which they pass is increased.

In all the experiments hitherto recorded, the direction of the calorific rays was that of the line which bisected the acute angle enclosed by the two optic axes. M. Knoblauch next proceeded to compare the quantities of heat transmitted in different directions by the same crystal.

The Nichol's prism was removed, and the direct solar rays were permitted to fall upon a cube of *dichroite*; the rays first passed in the direction of the middle line just referred to, and afterwards in the direction of the line which bisected the *obtuse angle* enclosed by the optic axes, the "supplementary line." The quantity transmitted in the former case, compared to that transmitted in the latter, was as 100 : 36.

The crystal was next placed so that the supplementary line, and with it the plane of the optic axes, was vertical, and the quantity of heat transmitted along the middle line was compared with the quantity transmitted in a direction at right angles to the plane of the optic axes: these quantities were found to be in the ratio of 100 : 46.

In both these cases, therefore, the quantity transmitted along the middle line was greatest.

Comparing the direction of the supplementary line with the perpendicular to the optic axes, the quantities transmitted in these respective directions were found to be in the ratio of 100 : 139. Although the crystal here examined exhibited certain mechanical defects, M. Knoblauch considers it certain that the differences stated cannot be referred to this cause. In the case of dichroite, radiant heat passes in the direction of the middle line in the greatest quantity; in less quantity perpendicular to the plane of the optic axes, whereas the direction of minimum transmission is that of the supplementary line.

The qualities of the heat passing in these three directions were next examined. In the first compartment of the following table, the heat transmitted parallel to the middle line is compared with that transmitted parallel to the supplementary line; in the second compartment, the middle line and the perpendicular to the plane of the optic axes are compared; and in the third compartment, the supplementary line is compared with the perpendicular to the plane of the optic axes. In all cases the quantity falling *upon the glass* is set =100, and the ratio of this quantity to that which passes *through the glass* is stated.

	Parallel to the middle line.	Parallel to the supplementary line.
Yellow glass	100 : 71	100 : 25
Blue glass	100 : 18	100 : 60
Red glass	100 : 54	100 : 52
Green glass	100 : 31	100.: 14
	Parallel to the middle line.	**Perpendicular to the plane of the optic axes.**
Yellow glass	100 : 78	100 : 67
Blue glass	100 : 16	100 : 27
Red glass	100 : 51	100 : 54
Green glass	100 : 24	100 : 20
	Parallel to the supplementary line.	**Perpendicular to the plane of the optic axes.**
Yellow glass	100 : 34	100 : 71
Blue glass	100 : 60	100 : 20
Red glass	100 ; 51	100 : 54
Green glass	100 : 22	100 : 22

From these observations M. Knoblauch concludes, that the properties of calorific rays are different, that is, they possess different powers to pass through the same diathermanous body, according as they have traversed dichroite in the direction of the middle line, parallel to the supplementary line, or perpendicular to these two directions.

A cube of blue topaz was submitted to a similar examination:

here it was found that the calorific rays passed in greatest quantity parallel to the supplementary line, in less quantity perpendicular to the plane of the optic axes, and least of all in the direction of the middle line, a deportment antithetical to that of dichroite being thus exhibited. Small qualitative differences were also observed where the heat was transmitted through the various coloured glasses.

Remarkable quantitative and qualitative differences were shown in the case of a twin crystal of diopside, according as the calorific rays were transmitted through it in one or the other direction. From all the experiments made with biaxal crystals, M. Knoblauch considers the fact to be placed beyond doubt, that certain of these crystals transmit calorific rays with different facilities in different directions; and after this transmission exhibit different properties; further, that for polarized heat, differences are observed in one and the same direction according as the plane of polarization coincides with or is perpendicular to that of the optic axes.

In the memoir alluded to at the commencement of this abstract, M. Knoblauch showed that the transmission of heat through certain crystals possessing but one optic axis, was dependent on the direction of transmission. In addition to the observations there recorded, he has recently made others on amethyst and idocrase, and finds, in the case of amethyst, when polarized heat is used, that the calorific rays which pass perpendicular to the axis of the crystal are different, in quantity and quality, according as their plane of polarization occupies the one or the other position; but when the rays pass in the direction of the crystallographic axis, no such differences are observed.

The transmission of unpolarized heat was not examined in the case of amethyst, but in the case of idocrase the quantity of natural rays transmitted parallel to the axis was to the quantity transmitted perpendicular to the axis as 100 : 78; qualitative differences were also exhibited by the two groups of rays.

These differences were not observed in the rays which passed through the crystal in two directions at right angles to each other, and also at right angles to the optic axis.

When polarized heat was used, the quantities transmitted when the plane of polarization was respectively parallel and perpendicular to the axis, were found to be in the ratio of 100 : 79. Considerable qualitative differences were also observed in these rays on testing them by coloured glasses. When, however, the plane of polarization was preserved coincident with the optic axis, no matter how the direction of the ray through the crystal might be changed, neither qualitative nor quantitative differences were discernible. From all his observations on mineral crystals M. Knoblauch concludes, that with certain bodies of this class calorific rays exhibit both quantitative and qualitative differences dependent on the direction of the rays through the crystal. Only in directions which are perpendicular to the optic axis are the effects the same. If the heat is polarized, differences dependent on the position of the plane of polarization are observed in one and the same direction. Here also the radiations perpendicular to the axis are the same. Along the axis alone is the transmission of the heat, and its further deportment dependent on the plane of polarization. The differences spoken of are greater in the case of polarized than of natural rays, when the plane of polarization in the one case has the same direction as the axis, and in the other, encloses an angle of 90° with it; the differences disappear altogether when the plane of polarization is kept coincident with the crystallographic axis.

IV. *Notes on Mineralogy.*—No. II. *On the Chemical Composition of the Granites of the South-east of Ireland. By the* Rev. SAMUEL HAUGHTON, *Professor of Geology in the University of Dublin*[*].

THE granites of the south-east of Ireland occur in the counties of Dublin, Carlow, Kilkenny, Wicklow, and Wexford, and may be divided physically into two distinct groups:—

1st. The chain of granite hills extending from Booterstown and Dalkey, co. Dublin, in a N.N.E., S.S.W. direction, to Poulmounty in the south of the county of Carlow, within five miles of New Ross. This granite chain has a length of 68 miles, and a breadth varying from 8 to 15 miles.

2nd. The series of granite hills occurring at intervals in the slate of the counties of Wicklow and Wexford, isolated from each other, and rising like islands through the slate. This group of granite hills lies between the main chain and the sea, and appears to be arranged in lines parallel more or less to the axis of the main chain. These granite hills are about twenty in number, and extend for a distance of 43 miles from Ballinaclash, county Wicklow, to Camarus Hill, county Wexford.

Hitherto, so far as I am aware, no *decisive* proof of difference of geological age has been discovered between these two groups of granites; they are both newer than the Silurian slates, which they penetrate and metamorphose.

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[*] Communicated by the Author.

Such being the state of geological knowledge on this subject, it occurred to me that it might be useful to direct attention to a distinction of a chemical character which appears to exist between these two groups of granites,—a distinction to which I have been led in the course of a series of analyses of Irish granites, in which I have been for some time engaged. The distinction to which I have alluded is the following. The granites of the main chain contain more potash than soda; and *vice versâ*, the granites to the east of the chain, which are isolated from it and from each other, contain more soda than potash; showing that the circumstances, unknown to us, under which the isolated granites were formed, were such as to yield to the molten mass a quantity of soda greater than that possessed by the granites of the principl chain.

In illustration of the foregoing generalization, I offer analyses of granites from the following localities:—

Granitic chain.

1. Dalkey, Co. Dublin.
2. Foxrock, Co. Dublin.
3. Three Rock Mountain, Co. Dublin.
4. Enniskerry, Co. Wicklow.
5. Ballyknocken, Co. Wicklow.
6. Killballyhugh, Co. Carlow.
7. Blackstairs, Co. Wexford.
8. Ballyleigh, Co. Wexford.

Isolated granites.

1. Cushbawn, Co. Wicklow.
2. Croghan Kinshela, Co. Wicklow.
3. Ballymotymore, Co. Wexford.
4. Ballynamuddagh, Co. Wexford.

It will be seen, on reference to the Ordnance Map, or any good map of Ireland, that the localities selected extend from the north to the south of both the granite series; and on reference to the geological maps of Wicklow and Wexford, it may be observed by those unacquainted with the geological structure of this part of Ireland, that the granites of the second group examined are taken from the distinct and distant isolated patches of granite.

In order to investigate the relative proportions of quartz, felspar and mica, of which these granites were composed, I used the following method, which appears to be as simple as any that has been proposed for such a purpose.

Let the per-centage of silica in the granite be divided by the atomic weight of silica, and let the quotient be denoted by a.

Let the per-centages of alumina and peroxide of iron be divided by the atomic weights of alumina and peroxide of iron respectively, and let the sum of the quotients so found be denoted by b.

Let the per-centages of lime, magnesia, potash, and soda be divided by the atomic weights of these elements, and the sum of the quotients called c.

Then, on the hypothesis that the granite is composed exclusively of quartz, felspar, and mica (margarodite), since

$$\begin{aligned}
&\text{Quartz} &&= \mathrm{SiO^3}\\
&\text{Felspar} &&= \mathrm{RO,\ SiO^3 + R^2O^3,\ 3SiO^3}\\
&\text{Margarodite} &&= \mathrm{RO,\ SiO^3 + 2(R^2O^3,\ SiO^3) + 2HO,}
\end{aligned}$$

we find, if Q, F, M denote the number of atoms of quartz, felspar, and margarodite present in the granite, the following relations:—

$$\left.\begin{aligned}
a &= Q + 4F + 3M\\
b &= F + 2M\\
c &= F + M
\end{aligned}\right\} \quad . \ . \ . \ . \ . \ . \ . \quad (1)$$

In these equations, *a*, *b*, *c* are given by the analysis; and from these equations, Q, F, M may be found. Having determined Q, F, M, we can obtain the per-centages corresponding to them by multiplying Q, F, M by their respective atomic weights. The atomic weight of quartz is known, and is 46; but the atomic weights of felspar and mica vary with the relative proportions of the ingredients composing these minerals. Assuming the average of the analyses of micas from this granite range already given by me (Phil. Mag. vol. ix. p. 272), it is easy to infer from it an atomic weight of mica equal to 305. This atomic weight of mica has been used by me in the calculations made in this paper, and the per-centages of felspar found by difference.

The calculations just mentioned do not prove that the granites to which they are applied are composed of quartz, felspar and mica, as combinations of other minerals might equally well represent the analyses; but on the hypothesis that the granites are composed of these three minerals, they give their per-centages with a close degree of approximation: and further, if the equations cannot be satisfied with positive values of Q, F, M, it may be considered as proved that the granite under consideration cannot be simply a compound of quartz, felspar and margarodite.

It is to be observed, that if a granite be composed of four or more minerals, it is not possible to find by the process just described, the per-centages of each mineral, because in this case the number of unknown quantities is greater than that of the equations from which they are to be calculated.

The following Table contains the analyses of eight granites taken from north to south along the principal granite chain, and the notes appended contain a few interesting particulars respecting each granite.

Table I.—Analyses of Granites from principal Chain.

Localities.	Silica.	Alumina.	Peroxide of iron.	Lime.	Magnesia.	Potash.	Soda.	Loss by ignition.	Totals.
1. Dalkey	70·38	12·64	3·16	2·84	0·53	5·90	3·13	1·16	99·74
2. Foxrock.........	73·00	13·64	2·44	1·84	0·11	4·21	3·53	1·20	99·97
3. Three Rock ...	70·28	16·44	2·60	2·04	trace	5·79	2·82	......	99·97
4. Enniskerry ...	74·24	13·64	1·40	1·48	trace	3·95	2·72	1·20	98·63
5. Ballyknocken...	70·82	14·08	3·47	2·65	0·31	4·64	2·31	1·39	99·67
6. Killballyhugh...	73·24	15·45	1·60	0·99	trace	4·59	3·08	1·20	100·15
7. Blackstairs ...	73·20	15·48	1·72	0·96	trace	4·80	3·18	......	99·34
8. Ballyleigh	73·28	12·64	2·00	1·72	trace	4·70	2·97	1·04	98·35

No. 1. *Dalkey.*—Specific gravity, 2·647. A fine-grained granite containing black and transparent mica. This granite cannot be a ternary compound of quartz, felspar and margarodite. This granite was used in the construction of Kingstown Harbour.

No. 2. *Foxrock.*—Specific gravity, 2·638. A coarse granite, which strikes fire under the hammer; it forms a durable and strong building stone, and has been employed in the ring stones of Trinity College belfry, and in the construction of the O'Connell monument at Glasnevin.

No. 3. *Three Rock.*—Specific gravity, 2·652. This granite is rather coarse-grained; it was taken from Woodside Quarry, on the slope of the Three Rock Mountain, and like No. 2, has been used in the construction of the O'Connell monument.

No. 4. *Enniskerry.*—Specific gravity, 2·633. A rather coarse-grained granite, containing veins of black tourmaline.

No. 5. *Ballyknocken.*—Specific gravity, 2·636. This granite is the best building stone in the neighbourhood of Dublin, and has been extensively used in the public buildings of this city; it forms the principal part of the granite used in the belfry and museums of Trinity College. The quarries are situated beyond Blessington, in the county of Wicklow.

No. 6. *Killballyhugh.*—Specific gravity, 2·616. This is a fine-grained granite, and works freely; it has been employed in the construction of the Chapel of Ease in the town of Carlow.

No. 7. *Blackstairs.*—Specific gravity, 2·622. A mediumgrained granite from Kiltealy, on the Wexford slope of Blackstairs.

No. 8. *Ballyleigh.*—Specific gravity, 2·627. A fine-grained granite taken from near Poulmounty Bridge, at the south-west extremity of the granite chain.

Calculating the atomic quotients from Table I., we construct the following Table, containing the values of *a*, *b*, *c*, and of Q, F, M, calculated from equations (1).

Table II.—Atoms of Granitic Minerals.

	Atoms of silex $=a$.	Atoms of peroxides $=b$.	Atoms of protoxides $=c$.	Atoms of quartz.	Atoms of felspar.	Atoms of mica.
Dalkey …………	1·530	0·286	0·353			
Foxrock …………	1·587	0·296	0·274	0·513	0·252	0·022
Three Rock ……	1·528	0·352	0·287	0·445	0·222	0·065
Enniskerry ………	1·613	0·282	0·225	0·770	0·168	0·057
Ballyknocken ……	1·540	0·317	0·283	0·442	0·249	0·034
Killballyhugh ……	1·592	0·321	0·232	0·753	0·143	0·089
Blackstairs ………	1·587	0·322	0·239	0·714	0·156	0·083
Ballyleigh ………	1·593	0·271	0·257	0·579	0·243	0·014

Calculating, by the method already described, the per-centages of quartz, felspar and mica, contained in the different granites, we find the following:—

Table III.—Per-centages.

Locality.	Quartz.	Felspar.	Mica.
Dalkey …………			
Foxrock …………	23·60	69·66	6·71
Three Rock ……	20·47	59·68	19·82
Enniskerry ………	35·42	45·83	17·38
Ballyknocken ……	20·33	68·97	10·37
Killballyhugh ……	34·64	38·37	27·14
Blackstairs ………	32·84	41·19	25·31
Ballyleigh ………	26·63	67·45	4·27

From the foregoing calculations, it follows, that, with the exception of the Dalkey granite, the granites of the main chain examined might be represented by combinations of quartz, felspar and mica, in which the quartz is the most regular mineral, considered with reference to its per-centage.

The following Table contains analyses of granites belonging to the second group, and supposed to be newer than the others,

Table IV.—Isolated Granites.

Localities.	Silica.	Alumina.	Peroxide of iron.	Lime.	Magnesia.	Potash.	Soda.	Loss by ignition.	Totals.
1. Cushbawn	70·32	11·24	4·80	3·01	0·73	2·27	3·39	1·62	97·38
2. Croghan Kinshela..	80·24	13·24	0·72	0·89	trace	0·40	5·58	......	101·07
3. Ballymoty	66·60	13·26	7·32	3·36	1·22	2·31	3·60	2·34	100·01
4. Ballynamuddagh...	68·56	14·44	5·04	3·85	0·43	2·78	3·36	1·00	99·46

No. 1. *Cushbawn.*—Specific gravity, 2·671. A fine-grained granite, containing hornblende in addition to mica. Besides the constituents given in the table, the specimen examined by me contained 1·34 per cent. of carbonate of lime.

No. 2. *Croghan Kinshela.*—Specific gravity, 2·629. This granite is composed of quartz, felspar and chlorite; the specimen examined appeared to be composed exclusively of quartz and felspar. From the great quantity of soda, I infer that the felspar of Croghan Kinshela is probably albite. On the northern slope of this mountain are situated the old gold streams of Wicklow.

No. 3. *Ballymoty.*—Specific gravity, 2·659. A very fine-grained granite, but containing distinctly felspar, quartz and black mica.

No. 4. *Ballynamuddagh.*—Specific gravity, 2·670. A coarse-grained granite, with large plates of black mica.

Applying to the preceding granites the method of calculation already employed, we find, excluding the granite from Croghan Kinshela, which contains chlorite in place of mica,—

Table V.—Atoms of Granitic Minerals.

	Atoms of silex $=a$.	Atoms of peroxide $=b$.	Atoms of protoxide $=c$.	Atoms of quartz.	Atoms of felspar.	Atoms of mica.
Cushbawn	1·529	0·278	0·300			
Ballymoty	1·448	0·349	0·346	0·067	0·343	0·003
Ballynamuddagh...	1·490	0·344	0·325	0·209	0·306	0·019

From the foregoing Table, it appears that the granite of Cushbawn cannot be considered as a ternary compound of quartz, felspar and margarodite, and in fact it contains a sensible quantity of a mineral which is either hornblende or chlorite. Calculating the per-centages of quartz, felspar, and mica of the two granites, which may be represented as composed of these minerals, we find,—

Table VI.—Per-centages.

	Quartz.	Felspar.	Mica.
Ballymoty	3·08	96·02	0·91
Ballynamuddagh ...	9·62	84·05	5·79

Whatever doubt may attach to the preceding calculations, owing to the hypothesis on which they are of necessity founded, no such doubt or uncertainty can belong to the results of direct experiment contained in Tables I. and IV., which have a positive value, independent altogether of the inferences which may be deduced from them. From these tables I deduce the following as the average composition of the granites of the main chain and of the isolated granites:—

Table VII.—Average Composition of Granites.

	Main chain.	Isolated.
Silica	72·305	71·430
Alumina	14·251	13·045
Peroxide of iron	2·299	4·470
Lime	1·815	2·778
Magnesia	0·119	0·595
Potash	4·822	1·940
Soda	2·967	3·982
Loss by ignition	0·899	1·240
Totals.....................	99·477	99·480

On examining Tables I. and IV., several interesting differences present themselves in the composition of the two groups of granites, in addition to the important difference to which I have drawn attention with respect to their alkaline constituents; but I content myself at present with establishing this fundamental distinction between the two groups, and thus furnishing an additional proof of the service which may be rendered to geological science by the more exact and experimental sciences.

V. *On Liquid Diffusion.*

By Dr. Adolph Fick, *Demonstrator of Anatomy, Zürich**.

A FEW years ago Graham published an extensive investigation on the diffusion of salts in water, in which he more especially compared the diffusibility of different salts. It appears to me a matter of regret, however, that in such an exceedingly valuable and extensive investigation, the development of a fundamental law, for the operation of diffusion in a single element of space, was neglected, and I have therefore endeavoured to supply this omission.

It was quite natural to suppose, that this law for the diffusion of a salt in its solvent must be identical with that, according to which the diffusion of heat in a conducting body takes place; upon this law Fourier founded his celebrated theory of heat, and it is the same which Ohm applied with such extraordinary success, to the diffusion of electricity in a conductor. According to this law, the transfer of salt and water occurring in a unit of time, between two elements of space filled with differently concentrated solutions of the same salt, must be, *cæteris paribus*, directly proportional to the difference of concentration, and inversely proportional to the distance of the elements from one another.

In mathematical language this may be thus expressed:—In a volume of salt solution, let the concentration in each horizontal elementary stratum be constant and $=y$, a function of the height x of this stratum above any other stratum which may be assumed as the primary horizontal plane; the limitation being made, that the function y must diminish as x increases, that is, each higher stratum must be less concentrated, and therefore lighter, than all the subjacent ones, because it is only under this condition, that the diffusion will not be interfered with by gravity; then from the stratum between the horizontal planes at x and $x+dx$ (in which the concentration is y) there will pass, during an element of time dt, into the immediate superjacent stratum, bounded by the horizontal planes $x+dx$ and $x+2dx$ (in which the concentration $y+\frac{dy}{dx}dx$ prevails), a quantity of salt $=-\mathrm{Q}\,.\,k\,.\,\frac{dy}{dx}\,dt$, in which Q signifies the surface of the stratum, and k a constant dependent upon the nature of the substances. It is evident that a volume of water equal to that of the salt passes simultaneously out of the upper stratum into the lower.

Exactly according to the model of Fourier's mathematical development for a current of heat, we can obtain from this fundamental law for the diffusion-current, the differential equation

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* From Poggendorff's *Annalen*, vol. xciv. p. 59; abstracted and communicated by the Author.

$$\frac{\delta y}{\delta t} = -k\left(\frac{\delta^2 y}{\delta x^2} + \frac{1}{Q}\frac{dQ}{dx}\cdot\frac{\delta y}{\delta x}\right), \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

when the section Q of the vessel in which the current takes place is a function of its height above the bottom. If the section be constant (*i. e.* the vessel cylindrical or prismatic), the differential equation becomes simplified to

$$\frac{\delta y}{\delta t} = -k\,\frac{\delta^2 y}{\delta x^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

Several methods for the experimental confirmation of this differential equation, and consequently of the fundamental law above advanced, presented themselves. In the first place, by integration of equation (2) the expression $y=f(x, t)$ could be obtained, and the calculated value of y compared with its observed value. I have, however, entirely omitted this method, because even in those cases in which the integral has a finite form, the numerical calculation of a sufficient number of values would have been extremely troublesome, whilst other unequivocal proofs were possible. For the same reason I also here omit to develope the particular integrals of equation (2) for special cases of diffusion-currents.

The experimental proofs just alluded to, consist in the investigation of cases in which the diffusion-current has become stationary, in which a so-called dynamic equilibrium has been produced, *i. e.* when the diffusion-current no longer alters the concentration in the spaces through which it passes, or in other words, in each moment expels from each space-unit as much salt as enters that unit in the same time. In this case the analytical condition is therefore $\frac{dy}{dt}=0$. Such cases can be always produced, if by any means the concentration in two strata be maintained constant. This is most easily attained by cementing the lower end of the vessel filled with solution, and in which the diffusion-current takes place, into a reservoir of salt, so that the section at the lower end is always maintained in a state of perfect saturation by immediate contact with solid salt; the whole being then sunk in a relatively infinitely large reservoir of pure water, the section at the upper end, which passes into pure water, always maintains a concentration $=0$. Now, for a cylindrical vessel, the condition $\frac{dy}{dt}=0$ becomes by virtue of equation (2),

$$0=\frac{d^2 y}{dx^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

The integral of this equation $y=ax+b$ contains the following proposition:—"If, in a cylindrical vessel, dynamic equilibrium shall be produced, the differences of concentration of any two pairs of strata must be proportional to the distances of the strata in the two pairs," or in other words, the decrease of concentration must diminish from below upwards as the ordinates of a straight line. Experiment fully confirms this proposition.

For the determination of the decrease of concentration in the cylindrical vessel conducting the diffusion-current, I sunk into the stratum to be estimated a glass bulb suspended from the beam of a balance, and calculated the specific gravity from the weight which required to be placed in the other scale-pan to balance the glass bulb. This method creates little confidence at first sight, nevertheless preliminary experiments showed it to be sufficiently accurate. The quotation of the numerical results of one experiment may suffice here.

Depth of Stratum below the Surface.	*Specific Gravity* of Stratum.*
10 millims.	1·009
32·2	1·032
54·4	1·053
76·6	1·073
98·8	1·093
121·0	1·115
143·2	1·135
165·4	1·152
187·6	1·170
209·8	1·187
220·9.	1·196.

That the degrees of concentration in the lower layers decrease a little more slowly than in the upper ones, is easily explained by the consideration, that the stationary condition had not been perfectly attained.

A second case of dynamic equilibrium was also observed, by replacing the cylindrical vessel in the above-described arrangement, by a funnel-shaped one with the apex downwards. As the section was now no longer constant, the condition for the dynamic equilibrium was deduced from the more general equation (1) in the form

$$0=\frac{\delta^2 y}{\delta x^2}+\frac{1}{Q}\cdot\frac{dQ}{dx}\cdot\frac{\delta y}{\delta x}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

For a perfect cone with circular base (the funnel-shaped vessel), we have $Q=\pi\,.\,\alpha^2 x^2$, if the origin be placed in the apex of the cone, and we call α the tangent of half the angle of aperture. By the substitution of this value, equation (4) becomes

$$0=\frac{d^2 y}{dx^2}+\frac{2}{x}\cdot\frac{dy}{dx},$$

the integral of which $y+c'=-\frac{c}{x}$. The two constants c and c' are to be so determined, that for a certain x (where the cone is cut off and rests upon the salt reservoir), y is equal to perfect saturation; and for a certain other value of x which corresponds to the base of the funnel, y becomes $=0$. In an experiment made with this apparatus, the following numerical values were obtained:—

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* The excess of which over unity is proportional to the concentration.

Depth of Stratum below the Surface.							
27·7 millims.	55·5	72·1	88·8	105·4	122·1	138·7	155·4.
Observed excess of Specific Gravity above unity (proportional to the concentration).							
0·000 millim.	0·008	0·019	0·030	0·040	0·055	0·075	0·105.
Calculated excess of Specific Gravity above unity.							
0·006 millim.	0·015	0·023	0·031	0·043	0·057	0·078	0·107.

The determination of the constants for different salts could now be proceeded with,—a determination which measures their diffusibility, and is expressed by k in equation (1). It may be defined as the quantity of salt which, during a unit of time, passes through the sectional-unit, out of one stratum into the next adjacent one, when the rapidity of the diminution of concentration $\left(\frac{dy}{dx}\right)$ is equal to unity. Let the sectional-unit be the section of our tubes, *i. e.* the surface of a circle of 1 millim. radius. The diminution of concentration $\left(\frac{dy}{dx}\right)$ will then be equal to unity, if, through a column of liquid assumed permanently constant, and the height of which is equal to the linear-unit 1 millim., it has for its result a difference of concentration of both terminal surfaces of such a character, that the one possesses a concentration corresponding to absolute saturation, the other a concentration $=0$. Let one day be a unit of time.

Three tubes of different lengths were now arranged exactly as the cylinder and funnel above described, and after the establishment of the stationary condition, the amount of salt was determined which diffused out of the terminal section of the tube in measured spaces of time, and which therefore also, in the same time, passed through any single section of the cylindrical tube. In this case the whole of the values required for the determination of k were known, and the rapidity of the diminution of concentration $\left(\frac{dy}{dx}\right)$ was especially determinable, inasmuch as it produced permanently, throughout the entire length of the tube, the difference between perfect saturation below and 0 above.

It is easy to perceive, that if our law be correct, the amounts of salt, passed through the three tubes in the same time, must be inversely proportional to the length of the tubes; and that if these amounts be divided by the time and multiplied by the length (expressed in millimetres), the same value, viz. the above-defined *k*, must be obtained for all three tubes. I here annex a short table of the best experiments with common salt, to which I have hitherto confined my attention:—

Temperature Centigrade during the operation.	*k*, calculated from the quantity which passed through the *longest* tube.	*k*, calculated from the quantity which passed through the *medium* tube.	*k*, calculated from the quantity which passed through the *shortest* tube.
Not determined.	11·71	12·36	11·08
15°·8 — 14°·8	9·67	9·70	9·30
15°·5 — 16°·0	......	9·57	......
16°·0 — 16°·5	......	9·94	......
17°·5 — 18°·5	10·79	......	......
18°·0 — 19°·0	10·71	11·08	10·50
20°	11·14	......	11·02
19°·0 — 22°·0	11·44	11·33	......
20°·0 — 21°·0	11·89	......	11·12

If we take into consideration the unavoidable sources of error, a nearer identity of numbers could scarcely be expected. The table shows, as might be already expected from Graham's experiments, that the value *k* increases with increase of temperature; probably, however, this dependence upon temperature is not a simple one. On the relations of *k* to other values expressing the essential properties of bodies, as, for instance, to atomic weight, nothing can be said until extensive series of experiments with different substances have been made. It is now of importance to make the views thus obtained furnish an insight into the diffusion of salt solutions through porous partitions. Brücke* first made an attempt to give a mechanical explanation to these phænomena. He went upon the assumption that the substance of the partition attracted the particles of water more strongly than the particles of salt. This assumption gained important support from the experiments of Ludwig† and Cloetta‡, who found that the solution of salt, imbibed by an animal membrane, was richer in water, and poorer in salt, than the solution in which the membrane was immersed. Brücke's theory, which was only indeed obscurely intimated, can be more clearly determined by the help of our fundamental law as follows.

Let us imagine a cylindrical pore of the radius ρ, in a membrane which is immersed in a saturated solution of a salt, and let us assume with Brücke, a stronger attraction between water and the molecules of the membrane, than between the latter and the salt molecules; then the density of the solution in each con-

* Poggendorff's *Annalen*, vol. lviii. p. 77. † Ibid. vol. lxxviii. p. 307.

‡ Experiments on Diffusion through Membranes with two Salts. Zürich, 1851.

centric cylindrical film of radius r, of the porous partition will evidently be constant, and $=f(\rho-r)$. Respecting the nature of this function, it can only be said that its value for $r=\rho$, which is equal to 0, and that from this point it must increase (probably very rapidly) with the decrease of r; but if necessarily under all circumstances for $r=0$, it must be increased up to the density of the circumambient solution, is left undetermined, and appears to me, in fact, for very narrow pores very improbable. In a word, the density in the pore will increase from the wall towards the centre, and in the cylindrical film of the radius r there can certainly occur no higher concentration than $f(\rho-r)$ (but possibly a lower one). The radius ρ of the pores must be imagined so small, that a filtration of liquid, by virtue of their cohesion, together with the attraction of the membrane itself, is prevented, even under high pressure; and that also equalization of the differences of pressure when liquid is present on both sides of the porous membrane, if possible at all, is only so in a relatively long space of time; further, that currents of mixture, by virtue of the variations of specific gravity within the pore, cannot occur.

Let us now suppose that a membrane of this description separates a saturated solution of salt from pure water, and further, that the former is above, and the latter under this horizontal membrane. In any cylindrical pore of the said membrane, whose radius may be again $=\rho$, let us suppose a concentric elementary stratum, bounded by two cylindrical films infinitely near to each other, the radii of which are respectively r and $r+dr$. At the upper extremity, the highest possible concentration, $f(\rho-r)$, of the layer will immediately be produced. On the other hand, the lower extremity, by immediate contact with a relatively infinite quantity of pure water, will be constantly maintained at a concentration $=0$; and when a stationary condition is brought about, the degrees of concentration, within the assumed elementary stratum, must increase from 0 to $f(\rho-r)$, in proportion to the height above the lower boundary surface of the membrane. This arrangement would have the effect of producing a diffusion-current in the elementary stratum, which, according to our laws, would deliver a quantity of salt $2\pi \,.\, k \,.\, \frac{f(\rho-r)}{h} dr$ downwards, and an equal volume of water upwards, if we understand by k, as above, the diffusion-constant for the combination of salt and water in question, and by h the thickness of the membrane, consequently the length of the pore. No regard has been paid, however, to the retardation which the flowing molecules suffer from the attraction of the substance of the membrane along the sides of the pores; it is certain, however, that the total quantity of salt, passed through the entire pore, could not be greater than

$$2\pi\frac{k}{h}\int_0^\rho f(\rho-r)dr.$$

But the passage of the water to the other side requires a separate consideration. We have seen, namely, that at the upper extremity of the cylindrical stratum-unit, with the internal radius r, no higher concentration could take place than $f(\rho-r)$, which is certainly less than perfect saturation, and, in fact, becomes proportionally smaller the greater r is assumed to be. If, as we suppose, a relatively inexhaustible volume of saturated solution (obtained by the addition of crystals) be present on the upper side of the membrane, then, at the upper extremity of our elementary stratum, there must take place a sudden increase in the concentration, from $f(\rho-r)$ to perfect saturation. If we assume that this is the case for the first moment, there will now be, according to the general principles of diffusion, from the elementary stratum a relatively (in comparison with the amount which a continual transition of density requires) infinite quantity of water required, and an equally infinite amount of salt forced in. The latter will be inevitably hindered by the nature of the membrane, and the excess of salt forced against the pore must in some way glide off laterally; on the other hand, more water, than the arrangement of the densities in our elementary stratum requires, can easily, to a certain extent, be drawn through towards the denser solution, so that in the pore, the particles of water move upwards with a greater velocity than the particles of salt move downwards. The excess of water now spreads out on all sides, into the saturated solution (as the mouths of the pores must lie at a certain distance from each other), partly by diffusion, partly by mixing streams proceeding from difference of specific gravity; until a stationary condition has been in such a way produced, that a conical space increasing upwards is supported upon the upper annular section of the elementary stratum, in which space the concentration $f(\rho-r)$ increases to perfect saturation, and which determines a diffusion-current of such a strength, that thereby exactly as much water is passed upwards, as in the same time can diffuse itself into the reservoir of saturated solution from the upper end of the space, without changing the concentration. Then the above-mentioned space would evidently be immediately lengthened upwards, (and thereby the intensity of the diffusion-current be diminished) so soon as more water passed through, and therefore the concentration at the upper end of the space continues to vary; and if, on the contrary, less water passed upwards, sudden transition of concentration must immediately occur in certain places, which sudden changes determine, as it were, a diffusion-current of infinite strength, and therefore again immediately draw the requisite quantity of water. Now this quantity of water, which, *cæteris paribus,* during the unit of time, can diffuse itself into the saturated solution without sensibly altering the concentration at that point, depends upon the easy mobility of the particles of the solution. The space in which the compensation takes place must therefore also be, *cæteris paribus,* so much shorter, and consequently the stream of water towards the saturated solution stronger, the more easily mobile the particles of the solution are. I was unable to verify this theoretical conclusion by direct experiment. I expected that the excess of water passing through the membrane, above the quantity of salt passing in the contrary direction, must be found much smaller, when the mobility of the upper solution was impeded by the addition of chalk magma. No such diminution of this excess, however, could be remarked in the experiment.

A second conclusion from the hypothesis just developed can be thus expressed: the excess of the diffusing water, above the salt passing in the opposite direction, is smaller, the wider the pores of the separating partition. Or if, according to Folly's* method, we define, as the endosmotic equivalent, the quotient obtained by dividing the amount of water diffused by the quantity of salt contemporaneously passed, the endosmotic equivalent is smaller the larger the pores of the partition. This conclusion is confirmed by experiment. Two membranes, which differ only in the diameter of their pores, cannot, it is true, be obtained; nevertheless we are safely warranted in assuming, that a transparent film of collodion possesses pores so very much narrower, than an animal membrane formed of interlacing fibres, that all other differences between the two tissues influencing diffusion, disappear in this difference of the size of pores; in fact, an animal membrane was found to possess a very much smaller endosmotic equivalent than a collodion film; for whilst, by the use of pig's bladder, only from 4 to 6 times as much water as common salt passed through, there diffused through a collodion film, during the time that an imponderable trace of chloride of sodium passed, a considerable quantity of water, probably many thousand times greater than that of salt.

Let us now suppose, that, instead of pure water, a solution of a certain concentration c of the same salt which is present on the upper side of the membrane in concentrated solution, be placed on the under side of that membrane, then the following considerations present themselves. All the elementary strata, from the wall of the pore, to an imaginary cylindrical film of the radius r, that $f(\rho-r)=c$, can only be filled from the top to the bottom with solution of the there *highest* possible concentration, and cannot therefore give origin to a usual double diffusion-current. Water will, however, pass over from the lighter to the denser solution, because a force of suction comes into play on each side of the membrane, proportional to the difference of concentration, consequently a stronger force at the upper side corresponding to the saturated solution. On the other hand, all those strata lying nearer to the axis, behave exactly in the above manner, with the single difference, that in them the concentration, instead of increasing from 0, now increases from below upwards from c to the respective maxima, and consequently an absolutely weaker diffusion-current is produced. It therefore follows in this case, that in places where, in the first instance, salt passed to the one side and water to the other, only a single diffusion of water takes place, consequently the proportion between water and salt (the endosmotic equivalent) must be *greater* here than there. If, for instance, $c > (\rho - r)$ (which is possible in the case of very fine pores), *no salt whatever* could pass through; the endosmotic equivalent would be $= \infty$, or the current one-sided.

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* Poggendorff's *Annalen,* vol. lxxviii. p. 361.

This conclusion is also confirmed by experiment. Even the above-cited experiments of Ludwig and Cloetta intimate this to be the case. I have myself obtained still more decided results. A membrane which gave an equivalent between 5 and 6, when it separated a saturated solution from pure water, yielded an equivalent $= 11{\cdot}05$, in another case even $= 17{\cdot}05$, when the pure water was replaced by a solution of common salt of 22 per cent. Lastly, if we analyse the case in which, instead of a saturated solution, a dilute one be placed above, and again pure water below. Let us again express the concentration of the upper solution by c. Evidently in an axial cylinder whose radius r is so great, as exactly to make $f(\rho - r) = c$, the concentration from the lower to the upper end, will now regularly increase from 0 to c, and a usual diffusion-current take place, which will pass a volume of salt downwards, exactly equal to the volume of water propelled upwards. Only in those layers situated nearer to the wall, can the concentration at the upper end never rise to c, and there will therefore pass through these layers alone, according to the previous considerations, more water than salt. The radius r, of the above-mentioned axial cylinder, is evidently as much greater as c is smaller, therefore the endosmotic equivalent must also decrease very rapidly with c; and we might expect that, for moderately small values of c, the endosmotic equivalent would be found smaller than unity (because a volume of salt weighs more than an equal volume of water). Ludwig's experiments with common salt are perfectly in accordance with this conclusion; in me cases in his experiments, the equivalent sunk from 4 to 1 when the concentration of the upper solution was decreased from saturation to 2 per cent. My own experiments in the same direction with common salt gave a purely negative result, as the equivalent was not perceptibly decreased.

The comparison of the experiments adduced above with the hypothesis developed on the foundation of the diffusion law, shows, though not absolutely, that the truth of this hypothesis may be determined; and it is in fact highly probable that, with or without modification, such an hypothesis may serve as the foundation of a subsequent theory of these very dark phænomena.

VI. *Analysis of Lunnite from Cornwall.*

By Dr. Heddle*.

THIS mineral was sent me for analysis by Mr. Greg, under the hope that it might prove to be a new *phosphate of copper*. It is doubtful from which of the Cornish mines it was procured, and the specimen in Mr. Greg's collection is, as far as is known, the only one which was found.

The specimen consists of aggregated congeries of minute spheres, the structure of which is radiated. The specific gravity is 4·25; little dependence can, however, be placed upon this determination, as it was found impossible to free the specimen from air, and minute crystals of *quartz* were imbedded in the mass.

The powdered mineral absorbs ·28 per cent. of moisture.

Analysis of 34 grs.:—

Oxide of copper . .	68·13
Phosphoric acid . .	22·73
Water	8·51
Silica	·48
	99·85

The silica was mechanically mixed.

This substance occurs in few localities. By this analysis a new one is given, and the mineral is now for the first time noticed as a British species.

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* Communicated by the Author.

VII. *On the Spontaneous Decomposition of certain Sulphomethylates.* *By* Arthur H. Church, *Esq.*[*]

IN the latter part of June 1853, I prepared a quantity of sulphomethylic acid, and neutralizing the mixture of sulphuric and sulphomethylic acids with finely-powdered native carbonate of baryta of unusually excellent appearance, expected to obtain sulphomethylate of baryta not far from pure; but the carbonate of baryta was contaminated with a considerable amount of carbonate of lime, consequently the product was a mixture of sulphomethylate of baryta with the corresponding lime-salt: that this was the case I did not discover, however, until the present year. The mixed salts, crystallized, and dried between bibulous paper, were set aside and forgotten until April last. The weight of the salts was not less than 380 grms. The general formula of the sulphomethylates, dried as just mentioned, is $C^2H^3M\,2SO^4+2aq$. The loosely-stoppered bottle containing the salts had been exposed in diffused daylight for a period of twenty-two months before it was disturbed. At the end of this time complete decomposition had taken place, the bottle no longer contained a trace of the original salts. At first three-fourths, the bottle was now little more than half full; the upper portion of its contents was a yellowish liquid of disagreeable odour and acid reaction; the lower was a white, apparently amorphous solid, but on this last-mentioned substance there rested a crystalline crust, and from this numerous distinct crystals sprang, their bases remote from the light, and their summits inclined towards it.

And first of these crystals, which were perfectly transparent and colourless. Their hardness exceeded 1°·5, but was less than 2°. The specific gravity of different specimens varied between 2·21 and 2·325. One of the largest crystals was 61 millims. in length and 2·5 millims. in breadth. The crystals belong to the oblique prismatic system, and exhibit many interesting modifications; their behaviour in polarized light was the same as that of selenite. In some aspects their lustre is pearly, in others vitreous. A qualitative examination indicated the presence of calcium, sulphuric acid, and water; while the quantitative estimation of these constituents in the crystals dried over sulphuric acid gave me the following numbers:—

·5015 grm. furnished ·2902 grm. of $Ca\,CO^3$.
·3031 grm. furnished ·41 grm. of $Ba\,SO^4$.
·2448 grm. lost on ignition ·0514 grm. of HO.

These results correspond with the following per-centage composition:—

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[*] Communicated by the Author.

	Experiment.	$CaSO^4 + 2aq.$
Ca	23·150	23·256
SO^4	55·724	55·814
HO	21·000	20·930
	99·874	100·000

The crystals were not altered by an exposure of two hours to a temperature of 100° C.; heated a few degrees above this point they lose water, becoming milk-white.

The white amorphous powder filling the lower part of the bottle was sulphate of baryta, but occurring here and there in it were botryoidal masses of granular sulphate of lime. An analysis of this sulphate gave numbers approaching somewhat closely to the formula $2CaSO^4 + aq$; a sulphate of lime having this formula has been observed occurring as a grayish crystalline deposit from a boiler working under a pressure of two atmospheres*.

The crystalline crust from which rose the prismatic crystals already described, was also sulphate of lime, of the formula $CaSO^4 + 2aq$.

The yellow fluid previously mentioned was thoroughly examined. Its reaction was strongly acid: on the addition of pure precipitated carbonate of baryta, an escape of carbonic acid took place and a heavy powder was precipitated, this was separated by filtration and proved to be sulphate of baryta. The filtrate, now perfectly neutral, was introduced into a retort and distilled; water having a slightly aromatic odour collected in the receiver; the residue in the retort solidified on cooling to a mass of colourless crystals. From this substance, recrystallized by means of alcohol, dissolved in the smallest possible quantity of water, and separated again by spontaneous evaporation of the water, the following analytical result was obtained, the crystals having been dried between folds of bibulous paper:—·415 grm. of salt gave ·2445 grm. of sulphate of baryta, corresponding to 58·91 per cent. of $BaSO^4$; theory, as $C^2H^3Ba\,2SO^4 + 2aq$, requires 59·02. A second analysis, for which a portion of the salt dried at 100° C. was employed, gave me the following numbers:—·5605 grm. of salt furnished ·3637 grm. of sulphate of baryta, corresponding to 64·88 per cent. of $BaSO^4$; theory, as $C^2H^3Ba2SO^4$, requires 64·9.

The saturated solution of this salt is not altered by prolonged ebullition, and the crystals may be heated above 100° C. without becoming acid or otherwise decomposing.

The final products of the spontaneous decomposition occurring in the mixture of the sulphomethylates of lime and baryta*, are thus well-crystallized sulphate of lime, sulphate of baryta, water, traces of an oily odorous body, with free sulphuric and sulphomethylic acids, the baryta salt of this last acid differing in many respects from the ordinary sulphomethylate of baryta although isomeric with it; I term it provisionally β-sulphomethylate of baryta.

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* Johnston, Phil. Mag. vol. xiii. p. 325.

This baryta salt evidently corresponds in the methyle series with the stable sulphæthylate (parathionate) of Gerhardt†, obtained from ordinary sulphæthylate of baryta by repeated ebullition and neutralization with carbonate of baryta of its concentrated aqueous solution, and which more recently has been identified by Berthelot‡ with the sulphæthylate of baryta obtained by saturating with carbonate of baryta the mixture of 4 vols. sulphuric acid and 1 vol. alcohol, which has been employed in making olefiant gas, or the solution of that gas in oil of vitriol. My opinion is also confirmed by the following experiments:—if to a strong solution of ordinary sulphæthylate or sulphomethylate of baryta a solution of protonitrate of mercury be added, a gray or slaty precipitate occurs—with the parathionate of Gerhardt, with the corresponding methyle compound which may be obtained by the same method§, and with the β-sulphomethylate of baryta the precipitate is white. The two last-mentioned salts are in fact in every respect identical.

If the analytical results did not forbid such a view, it might have been imagined that the modified or stable baryta salt was really not the sulphomethylate, but the hyposulphomethylate of baryta, for an aqueous solution of this latter salt, or indeed of any of its analogues and homologues, is quite unalterable. Sulphomethylic acid bears the same relation to hyposulphomethylic acid as sulphophenic acid does to sulphobenzolic acid, and it may not be uninteresting to present in a tabular form some relations between these two series. In the first column will be found the formulæ of a series of baryta salts, the aqueous solutions of which are not alterable by ebullition—a series where the proportion of sulphur to oxygen in the dry salts is as 2 to 6;

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* For an account of the spontaneous decomposition of certain sulphovinates, see Marchand in the *Journal für Praktische Chemie*, xliv. 122.

† *Compt. Rend. des travaux de Chimie*, 1845, p. 176.

‡ *Annales de Chimie et de Physique*, 1855, p. 392.

§ In 1853, at the suggestion and under the direction of Dr. Hofmann, I prepared this salt and submitted it to a complete analysis. It is identical in composition with ordinary sulphomethylate of baryta $C^2 H^3 Ba\ 2SO^4 + 2aq$. Although it is only recently that the metamorphoses of the sulphæthylates have been accurately studied, it would appear to have long been known that aqueous solutions of the sulphomethylates undergo by ebullition similar changes, and yield products exactly analogous to those of the æthyle salts.

in the second column another series is given of salts decomposable by ebullition of their aqueous solutions, with separation of sulphate of baryta, and formation (in most cases) of new acids, and in which the proportion of sulphur to oxygen is as 2 to 8: the observations were made in all cases with baryta salts; where an asterisk is prefixed, the experiment was either made for the first time or repeated by myself.

I.	II.
*$C^2 H^3 Ba\,2SO^3$. (Hyposulphomethylate of baryta.)	*$C^2 H^3 Ba\,2SO^4$. (Sulphomethylate of baryta.)
*$C^{12} H^5 Ba\,2SO^3$. (Sulphobenzolate of baryta.)	‡*$C^4 H^5 Ba\,2SO^4$. (Sulphæthylate of baryta.)
*$C^{14} H^7 Ba\,2SO^3$. (Sulphotoluolate of baryta.)	§$C^6 H^7 Ba\,2SO^4$. (Sulphopropylate of baryta.)
*$C^{16} H^9 Ba\,2SO^3$. (Sulphoxylolate of baryta.)	*$C^{10} H^{11} Ba\,2SO^4$. (Sulphamylate of baryta.)
†$C^{18} H^{11} Ba\,2SO^3$. (Sulphocumolate of baryta.)	*$C^{12} H^5 Ba\,2SO^4$. (Sulphophenate of baryta.)
†*$C^{20} H^{13} Ba\,2SO^3$. (Sulphocymolate of baryta.)	*$C^{14} H^7 Ba\,2SO^4$. (Sulphotoluenylate of baryta.)
*$C^{20} H^7 Ba\,2SO^3$. (Sulphonaphthalate of baryta.)	

The experiment might no doubt have been tried with many other salts of similar constitution with analogous results.

In column II., by the ebullition of solutions of the salts 1 and 2, modified stable salts may be obtained; a similar action probably takes place with salts 3 and 4, for the solution of ordinary sulphamylate of baryta gives a blue-gray precipitate with protonitrate of mercury, while the salt obtained by boiling, &c. the solution of sulphamylate of baryta gives a white precipitate. The metamorphosis of sulphamylate of baryta takes place readily on heating in an oil-bath the solution of this salt enclosed in a sealed tube; with sulphophenate of baryta under similar conditions, regeneration of phenylic alcohol takes place:

$$C^{12} H^5 Ba\,2SO^4 + 2HO = C^{12} H^6 O^2 + Ba\,SO^4 + HSO^4.$$

The relations of æthyle, &c. to phenyle are very remarkable. The consideration of these analogies is likely to lead to many interesting results; for instance, phenate of ammonia, when heated in a closed tube, is well known to be resolved into aniline and water, and I have found that an exactly analogous reaction takes place with æthylate of ammonia; the following equations represent these changes:—

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† Gerhardt and Cahours, Delalande. ‡ Gerhardt. § Berthelot.

$$C^{12}H^{5}NH^{4}O^{2}=C^{12}H^{7}N+2HO$$
$$C^{4}H^{5}NH^{4}O^{2}=C^{4}H^{7}N+2HO.$$

Æthylate of ammonia may be prepared by the action of sulphate of ammonia upon potassium-alcohol. Also we might hope to obtain sulphobenzolic acid, $C^{12}H^{6}2SO^{3}$, by the action of nitric acid on sulphocyanide of phenyle, just as hyposulphæthylic acid is obtained from the corresponding æthyle term. A more extended study too of sulphophenic acid and its derivatives would be of great interest. The action of sulphæthylate of silver upon iodide of methyle, and of sulphomethylate of silver upon iodide of æthyle, gives rise to products now under investigation, products which no doubt have their representatives in the phenyle series. A digestion of sulphæthylate of silver with iodide of æthyle appears to yield an oily substance, probably sulphæthylate of æthyle (sulphate of æthyle), to the presence of which in sulphæthylates undergoing decomposition, their peculiar odour is perhaps to be attributed, and from which by further change parathionic acid may be derived.

June 1855.

VIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. ix. p. 544.]

March 22, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Anatomy of *Nautilus umbilicatus*, compared with that of *Nautilus Pompilius*." By John Denis Macdonald, Esq., R.N.

During a visit of H.M.S.V. 'Torch' to the Isle of Pines in July 1854, a recent specimen of *Nautilus umbilicatus* was picked up on the outer reef off Observatory Island. It was alive when brought on board, but was too much exhausted to exhibit active movements. Part of the hood appeared to have been eaten away behind by some predaceous enemy, but in other respects the animal was perfect.

The body when retracted lay more deeply in the shell than that of *N. Pompilius*, so that no part was visible in a lateral view, and on account of the great depth of the chamber of occupation the orifice of the siphuncle in the last septum could not be seen when the soft parts were removed. As to this difference, however, the author observes that it may depend on the time elapsed since the formation of the last partition.

Apart from the shells, the author finds a close resemblance between the corresponding parts of the two species.

The specimen of *N. umbilicatus* examined proved to be a female;

a fact which may serve to modify the views of those who, adopting the speculations of D'Orbigny on the sexes of the Ammonites as indicated by the characters of their shells, apply them also to the several kinds of *Nautili* known.

The body of *N. umbilicatus* is larger and more elongated than that of *N. Pompilius* as it occurs in the South Seas, although the specimens of the latter species brought from the Chinese Seas much exceed both in size. In the *N. umbilicatus*, the longitudinal lamellæ on the median lobe of the external labial processes are divided by a wide groove into two lateral sets, and the corresponding lamellæ between the internal labial processes are about seventeen in number and of considerable thickness. In *N. Pompilius*, the latter lamellæ are much thinner and more numerous, and the lateral sets of the former are united together in the median line, commencing anteriorly with an azygos transverse lamina. In both kinds, however, the corresponding tentacula may be distinctly traced out, with only such minor differences as might be expected to occur in different specimens of either separately; the digital, labial and ocular groups agreeing sufficiently both as to number and character in the two cases, considering the liability of these parts to slight modifications, from arrest of development or redundance, in the same species.

Referring to former observations of his own on the eye of *N. Pompilius*, the author observes that they closely apply to *N. umbilicatus*, which affords confirmation of his opinion that the pigmentary coating is subjacent to the retina. He finds no vestige of a lens, and in place of vitreous humour, a mere viscous matter protecting the retina from the sea-water.

The organ of hearing, which had escaped detection in the specimen of *N. Pompilius* dissected by Professor Owen, altered as it doubtless had been by long immersion in spirit, was discovered in the example of *N. umbilicatus* examined by the author. It consists of two spheroidal acoustic capsules placed, one on each side, at the union of the supra- and subœsophageal ganglia, and measuring about one-twelfth of an inch in diameter. Each capsule rests internally against the nervous mass, and is received on its outer side into a little depression in the cephalic cartilage. It is enveloped in a kind of fibrous tissue and filled with a cretaceous pulp consisting of minute, elliptical, otoconial particles, presenting under a high power a bright point near each end, varying much in size, and sometimes combined into stellate, cruciform or other figures. Cilia were not observed within the capsules.

The inside of the mouth is furnished with three groups of papillæ, one of which occupies the median line between the orifice of the tongue-sac and commencement of the œsophagus. These lingual papillæ, as well as the rest, are clothed with long and slender columnar epithelium-particles.

The author agrees with Mayer in regarding the well-known follicular appendages of the afferent branchial vessels of the Cephalopoda, as performing the function of kidneys, but admits that they may also serve, by altering their capacity, to regulate the amount of blood passing through the branchiæ under changes of pressure to which the animal may be subjected at different depths. These follicles are subcylindrical in form, and somewhat dilated at the free extremity, to which is appended a folded and funnel-shaped process of membrane which expands rather suddenly and presents a jagged border. They open by an oval or slit-like orifice into the afferent branchial vessels, on each of which, as Professor Owen has observed, they are disposed in three clusters. The outer membrane is smooth and glossy, homogeneous in structure, and sprinkled over with minute, rounded, transparent bodies, resembling the nuclei of cells. Beneath this layer, flat bundles of fibres, apparently muscular, are traceable here and there, principally disposed in a longitudinal direction, and sometimes branched. The lining membrane consists of a loose epithelial pavement, similar in many respects to that of the uriniferous tubules of the higher animals, the cells containing, besides the nuclei, numerous minute oil-globules, or a substance much resembling concrete fatty matter. This membrane is thrown up into very numerous papillæ and corrugations, so as greatly to increase the extent of surface. The papillæ are more numerous towards the attached end, and a circlet of longitudinal folds, with transverse zigzag corrugations, radiate from the bottom of the follicle, in which a number of small pits or fenestrations are sometimes visible. The funnel-shaped membranous process above noticed is continuous with the lining membrane. The cavity of each follicle, therefore, communicates with the exterior through the centre of this process, and the aperture is thus guarded by a kind of circular valve permitting the escape of secreted matters, but effectually preventing the entrance of fluid from without.

Some considerations are next offered in support of the view adopted as to the functions of these vascular appendages.

Lastly, on the question whether the peculiarities of structure recognized respectively in *N. Pompilius* and *N. umbilicatus* are sufficient to establish a difference of species, or are attributable merely to variety, the author observes, that any tendency in a being to revert to an original type, when such has been determined, betrays variety; but this tendency is never manifested in the *Nautili* under consideration by the occasional occurrence of specimens presenting characters which place them intermediately between *N. Pompilius* and *N. umbilicatus*. Having visited the Fijii Islands since he formerly wrote on *N. Pompilius*, he finds that the umbilicated *Nautili* are not known to the natives, although *N. Pompilius* is very plentiful; but at Fatuna or Wallis's Island, where both are found, the people recognize the difference between them depending on the presence or absence of umbilical pits. On this the author remarks, that although particular localities, with all attending circumstances, may favour the production of varieties, yet the permanence of the distinctive characters of these *Nautili* without symptom of amalgamation, and the discovery of a female specimen of *N. umbilicatus*, are strong arguments in support of the view that they are distinct species, though very closely allied.

"On a Class of Differential Equations, including those which occur in Dynamical Problems."—Part. II. By W. F. Donkin, M.A., F.R.S., F.R.A.S. &c.

This is the second and concluding part of a paper of which the first part was printed in the Philosophical Transactions for 1854. In the fourth section (the first of this part) some of the most important results of the former part are recapitulated.

In the fifth section the theory of the Variation of Elements is considered under that aspect which belongs to it in connexion with the general methods of this paper; and the facility of its application is shown in two instances: (1) the expressions for the variations of the elliptic elements of a disturbed planet's orbit are deduced from the results of Art. 30 (Part I.), on undisturbed elliptic motion; (2) the problem of determining the motion of a free simple pendulum (omitting the effect of the earth's rotation) is treated by considering the orbit of the projection of the bob upon a horizontal plane as a disturbed ellipse. The differential equations which define the variations of the elements of the ellipse are given in a rigorous form, and integrated approximately so as to give the motion of the apsides of the mean ellipse in any case where the pendulum never deviates much from the vertical, and the motion is not very nearly circular. The result agrees with the conclusions of the Astronomer Royal (Proceedings of the Royal Astronomical Society, vol. xi. p. 160).

In the fifth section the transformation of the differential equations by the substitution of new variables is considered, and particularly that kind of transformation, called by the author a *normal transformation*, which leads to a new system of equations, not merely possessing the same general form as the old, but distinguished also by other common properties. A definition is given of those transformations which may be properly called, from analogy, *transformations of coordinates*, and it is shown that all transformations of coordinates are *normal*. General formulæ are given for transforming the equations of any dynamical problem from *fixed* or *moving* systems of axes of coordinates; and an illustration is drawn from the case of the motion of a planet referred to axes in the varying plane of its own orbit.

In the seventh and last section the principles of transformation developed in the preceding section are applied in a more general manner to the differential equations of the planetary theory; and it is shown that when the motions of a planetary system are referred to a system of rectangular axes having their origin in the sun, and otherwise moving in any arbitrary manner, the variations of the elements will still be determined by the same formulæ as if the axes were fixed, provided there be added to the disturbing function R, for each planet, the expression

$$\sqrt{\mu a(\iota - e^2)}\,.\,(\omega_0 \sin\nu \sin\iota - \omega_1 \cos\nu \sin\iota + \omega_2 \cos\iota),$$

in which ι is the inclination of the orbit to the (moving) plane of xy, the longitude of the node reckoned from the axis of x, and $\omega_0, \omega_1, \omega_2$; the angular velocities of the moving system of axes about the three axes themselves. In this expression ω_0, ω_1, ω_2 may be any arbitrary functions either of the time or of the elements; but in any case these functions are to be exempted from differentiation with respect to the elements in taking the partial differential coefficients of the disturbing function.

This result is illustrated by referring the motion of a system consisting of *two* planets to axes so chosen that the plane of xy shall always coincide with the *principal plane* of the system, and the axis of x, from which all longitudes are reckoned, shall always coincide with the line of nodes; there are thus obtained twelve rigorous simultaneous differential equations, of which nine form a system apart, containing only the major axes, excentricities, epochs, longitudes of perihelia, and mutual inclination of the two orbits, and afford an example of the so-called "elimination of the nodes;" whilst the remaining three (which contain also these nine elements, but not their differential coefficients) determine the motion of the principal plane and of the line of nodes, relatively to fixed space. The mutual inclination of the two orbits being supposed known, their several inclinations to the principal plane are given by simple relations; and the positions of the planets in their orbits (their longitudes reckoned from the line of nodes) being supposed known, their motions relatively to fixed space would thus be completely determined.

Extract of a Letter, dated January 6, 1855, from J. Mitchell, Esq., Quartermaster of Artillery, Bangalore, "On the Influence of Local Altitude on the Burning of the Fuses of Shells."

"In the early part of the year 1848, at the annual practice of the Artillery in this garrison, it was observed that the fuses burned too long a time. The regular burning of fuses being a matter of much importance, the circumstance was duly reported to Artillery Head Quarters, and a portion of each kind was directed to be sent to St. Thomas's Mount (eight miles from Madras and on the same level) for examination, where they were found to burn correctly, and as at that time no one suspected the real cause of the discrepance, it was concluded there had been some error in the length of our pendulum, and there for the time the matter rested.

"But as I was satisfied, for many reasons, that it was not owing to an error in the pendulum, I determined to keep the subject in mind, and for the practice in the following year (1849) I caused to be made an adjustable pendulum which beat seconds very correctly for several minutes. This pendulum was daily compared with two or more seconds' watches to make all safe, and, as our longest time of flight did not exceed twenty seconds, no error could possibly arise from this source. The result was that the fuses again burnt too long at Bangalore, and were again found to burn correctly at St. Thomas's Mount.

"This was a mystery to all; but after the matter had cost me much thought, it occurred to me that the cause was to be sought in the difference of altitude between St. Thomas's Mount and Bangalore, nearly 3000 feet; and as a means of putting this to the test, I suggested that some fuses should be burnt at the Mount, or at Madras, under a receiver, exhausted until the barometer stood at the Bangalore mean height, or about 27 inches. This, however, it was not found convenient to do; but, as an equally satisfactory way of testing the accuracy of my conclusions, a small number of fuses were prepared and burnt at St. Thomas's Mount, at Bangalore, and at two different altitudes on the Neilgherry Hills, as will be seen by the annexed copy of an official memorandum; and although this experiment was too limited to enable us to compile a scale of the probable times a certain length of fuse composition would burn at given altitudes, it amply proves the fact that combustion is retarded at considerable elevations.

"Memorandum of an experiment to ascertain whether the atmosphere influences the burning of fuses:—

"Eighteen 8-inch fuses, made of the same description of wood (Congo), were filled with composition made for the purpose. The same man drove the whole on the morning of the 2nd August 1849, using the same mallet and drifts. Six of the fuses were burnt at the Mount, six at Bangalore, and six on the Neilgherry Hills; all in the presence of artillery officers; the result is shown below":—

Time.	Station.	Length of fuse.	Time of burning.	Barometer.	Thermometer.	Height above the level of the sea.
		in.	secs.	in.	°	
August 2, 1849. ½ past 12 o'clock.	St. Thomas's Mount.	3	14·15	29·78	89	Artillery Depôt yard.
		3	14·30			
		3	14·30			
		3	14·30			
		3	14·15			
		3	14·30			
August 17, 1849. ½ past 5 P.M.	Bangalore.	3	16·00	26·89	82	3000 ft.
		3	0·0*			
		3	15·75			
		3	15·15			
		3	15·75			
		3	16·25			
August 31, 1849. ½ past 7 A.M.	Kotagherry, Neilgherries.	3	17·00	24·025	Att. 62·7	6500 ft.
		3	17·00		Det. 61·8	
		3	0·0*			
		3	17·30			
Sept. 8, 1849. 20 minutes past 8 A.M.	Cotacamund, Neilgherries.	3	18·00	23·030	54·2	7300 ft.
		3	18·25			

The writer attributes the result to the rarity of the atmospheric air, and of its constituent oxygen at the higher stations.

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* The fuses thus marked accidentally ignited at both ends.

March 29, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read:—

"On the existence of an element of Strength in beams subjected to Transverse Strain, arising from the Lateral Action of the fibres or particles on each other, and named by the author the 'Resistance to Flexure.'" By Peter William Barlow, Esq., F.R.S.

The author commences by observing, that under the existing theory of beams, which recognizes only two elements of resistance, namely tension and compression, the strength of a beam of cast iron cannot be reconciled with the results of experiments on the direct tensile strength, if the neutral axis is in the centre of the beam.

He then proceeds to describe experiments made on two solid beams of cast iron to determine the position of the neutral axis. The beams employed were 7 feet long, 6 inches deep and 2 inches thick, on each of which small vertical ribs were cast, 12 inches apart; nine small holes were drilled opposite to each other in each rib, for the purpose of inserting the pins of a delicate measuring instrument. The distances of the holes of the centre division of both beams were measured under various strains, and the results show that the extensions and compressions proceed in an arithmetical ratio from the centre to the upper and lower sides of the beam; and that at any given distance on either side of the centre, the amount of extension is equal to the amount of compression.

The position of the neutral axis being thus conclusively ascertained to be in the centre, it is shown that, not only the ultimate strength, but also the amount of extension and compression with a given strain, indicates the existence of another element of resistance, in addition to the resistances to extension and compression.

The author then points out, that in applying the law of "*ut tensio sic vis*" to contiguous fibres, under different degrees of extension and compression, the effect of the lateral adhesion has been omitted, and each fibre has been supposed to be capable of taking up the same degree of extension or compression as if it acted separately, and independently of the adjoining fibres.

It is then shown that this supposed independent action of the fibres is inconsistent with other practical results, and evidence is exhibited of a powerful lateral action when unequal strains are exerted.

From these and other considerations, the author is led to think that the effect of the lateral action, tending to modify the effect of the unequal and opposite strains in a beam, constitutes, in effect, a "resistance to flexure" acting in addition to the resistances of tension and compression.

In order to ascertain whether the apparent difference in the amount of tensile strength when excited by direct and transverse strains is due to *flexure*, the author caused open beams or girders to be made, each of which was formed by two bars of metal; the upper and lower bars of the same beam were in every case of the same form and dimensions; but the depth of metal and the distance to which the bars were separated vertically, was varied in the several forms of girder experimented upon. By these means the bar forming the lower side of each girder was torn asunder under different degrees of flexure. The different forms of girder experimented upon were of equal length, and were compared with solid beams and with bars of the same metal broken by direct tensile strain.

From the mean of *four* experiments on each form of girder, the value of the total resistance at the outer fibre is ascertained, and exhibits the following results:—

1. In girders having the same depth of metal, namely about 2 inches, but the total depth of the girder, and consequently the deflections different—

Form of beam or girder.	Depth of girder.	Deflection.	Total resistance at the outer fibre.
			lbs.
No. 1. Solid beam	2·02	·670	41709
No. 2. Open girder	2·51	·510	35386
No. 3. Open girder	3·00	·401	31977
No. 4. Open girder	4·00	·301	28032

2. In girders having the same total depth (namely 4 inches), and consequently nearly the same deflection, but differing in the depth of metal—

Form of beam or girder.	Depth of metal.	Total resistance at the outer fibre.
		lbs.
No. 5. Open girder	3·01	37408
No. 4. Open girder	1·97	28032
No. 7. Open girder	1·56	27908
No. 6. Open girder	1·48	25271
The tensile strength of the metal employed was found to be		18750

From these experiments, the particulars of which are fully detailed, the following facts are elicited:—

1. That in all cases the total resistance at the outer fibre, at the time of rupture, is greater than the tensile strength.

2. That in girders having the same depth of metal, it increases when the deflection increases; and

3. That in girders having the same total depth, and the same deflection, the resistance is greater when the depth of metal in the beam is greater.

And it follows that there is an element of strength depending on the depth of metal in connexion with the deflection; or in other words, dependent on the *degree of flexure* to which the metal forming the beam is subjected.

The author next proceeds to examine the law under which this resistance varies; and considering the total resistance in the solid beam to be composed of two resistances, one being constant and due to the tensile strength, and the other variable and depending on the depth of the metal in connexion with its deflection, the experiments indicate that the resistance to flexure varies, throughout all the girders, directly as the amount of deflection into the depth of the metal.

The paper concludes by pointing out the important amount of this resistance, the operation of which has been hitherto unknown, and which in cast iron exceeds the tensile strength of the metal, and shows that comparisons of the strength of different forms of section, based on the existing theory, which assumes the resistance at the outer fibre to be constant and equal to the tensile strength of the metal, must be entirely fallacious.

The paper is accompanied by full details of all the experiments, and the measurements for determining the position of the neutral axis.

"On the Metallic and some other Oxides, in relation to Catalytic Phænomena." By the Rev. J. Eyre Ashby.

I purpose to detail some experiments on the metallic (and a few other) oxides, made with a view to ascertain their powers to produce and maintain, catalytically, the combustion of various gases and vapours; and to annex such considerations as appear to be suggested by the facts. By catalysis I understand the operation of one body upon another, under favourable circumstances, whereby the second body is resolved into new chemical combinations, while the first (whatever may happen during the process) remains finally unchanged. This must be taken as not including explosion by percussion, in which the change takes place owing to the external application of dynamic force.

The apparatus for experimenting comprehends a variety of shallow capsules; wire-gauze, of iron, copper, and brass, of different degrees of fineness, cut into discs a little larger than the vessels on which they are to be superimposed; a spirit-lamp with large wick; a pair of pliers, and a few rings of wire to support the gauze, if necessary, while heating it in the spirit flame. The method of procedure is simple: the watch-glass, or capsule, is *nearly* filled with the liquid whose vapour is to be tried; on a wire-gauze disc is spread the oxide whose powers as a catalyser are to be tested, and this being warmed (more or less) over the lamp, is set down upon the upper rim of the capsule. Sometimes it is necessary to heat a layer of the oxide in the middle of a small combustion-tube, and pass over it the gas, or mixture of gases.

I tried the following substances with pyroxylic spirit (hydrated oxide of methyle) and alcohol separately.

1. CoO appeared to possess the power in some degree, but perhaps the specimen was in too dense agglomeration, which is not essentially reduced by trituration.

2. Co_2O_3 maintained the catalytic combustion very well.

3. AgO, reduced to metallic silver, which shows a strong tendency on gauze, and acts perfectly in the combustion-tube.

4. U_2O_3, HO became, at red heat, anhydrous mixture of UO and U_3O_4, showing strong tendency. A very pure specimen catalysed the vapour as it changed from yellow to green, after which it died away. Will not act below 570° (F.).

5. SnO; strong tendency.

6. SnO_2; slight tendency.

7. WO_3 apparently produces the effect if placed while glowing, over alcohol, but gradually dies away, as if very slowly cooling.

8. Pb_3O_4 changed to PbO, and showed a strong tendency, but quickly faded and grew cold.

9. CdO, placed while very warm over pyroxylic spirit, burst into glow and catalysed, but always died off after the lapse of from half a minute to two or three minutes, and then became incapable.

10. CaO (on the gauze), no effect.

11. SiO_2 exhibited a tendency.

12. Stourbridge clay; no effect.

13. Al_2O_3 appeared to have no effect in maintaining catalytic combustion on the gauze, but when made red-hot and quenched in absolute alcohol, it changed from pure white to a black substance and oxidized a portion of the alcohol. That this is not owing to carbon in the alcohol is evident, because the same change occurs when it is quenched in strong liquid ammonia. I suspect that it is a new oxide of aluminum.

14. Ni_2O_3, formed by heating carbonate of nickel nearly to redness, failed; prepared from the common nitrate, it acted for a short time; reduced as an intensely black velvety substance from the purest nitrate, then warmed but not made red-hot, it glowed and catalysed with alcohol or ether. With pyroxylic spirit, it was left at the end of the operation of a greenish drab, which I suspect to be a mixture of Ni_2O_3 with NiO, although it may be Ni_2O_3 changed only in appearance, for when treated with nitric acid no nickel is dissolved.

15. MnO_2 is changed at red heat into Mn_2O_3, which, with alcohol, ether, and pyroxylic spirit, continues the slow combustion very steadily. A specimen of very pure Mn_2O_3 acted extremely well, as did also a portion of "euchrome" (a hydrated sesquioxide of manganese (impure) dug from the estate of Lord Audley), after being heated in the air to drive away the carbonaceous matter with which it was mingled. Mn_2O_3 will, if sufficient care be taken, catalyse the moist gas arising from a strong solution of ammonia.

16. Fe_2O_3, when in the state of a light puffy powder, catalyses the vapour of ether, alcohol, and pyroxylic spirit, only requiring to be heated on the gauze before it is laid over the capsule. It is cheap, easily employed, and of invariable action. I have kept up the combustion for several hours on a surface of 120 square inches.

By means of a catalytic lamp in which the liquid employed is continually supplied from a reservoir and maintained at a constant level in the capsule, I have used 7 or 8 square inches continuously during thirty-six hours. This lamp I have occasionally used for laboratory purposes, where a gentle and equable heat was required for several hours.

Pursuing my experiments with the oxides of the metals, heated on wire gauze, I tried as many as I could procure or make, and by a tolerably wide induction I found that the *sesquioxides* have the strongest tendency to produce and maintain the catalytic glow, and do produce it in every case in which they are not decomposed by the amount of heat required to begin the operation.

When hydrated Fe_2O_3 is heated and placed over alcohol, its colour is deepened towards black, but not uniformly, and when cold the original colour returns. But if it be made red-hot and quenched in boiling alcohol out of contact with air, it is converted into hydrated Fe_3O_4, and remains permanently a deep black magnetic powder, soluble in acids. A strong solution of ammonia may be substituted for the alcohol with the same effect, but in this case some of the sesquioxide will remain unaltered and mixed with the black oxide. The alcohol or ammonia is correspondingly changed by oxidation derived from the oxygen which has been released from combination with the iron. If the hydrated Fe_3O_4 be heated in contact with air, it immediately (even when it has been kept for many months) becomes Fe_2O_3 by oxidation from the atmosphere, but if heated to redness *in vacuo*, it cools unchanged. [Can the black powder of alumina be Al_3O_4, formed in a similar way?] The process of catalysation by Fe_2O_3 is thus evident; the heated sesquioxide loses a portion of oxygen to the alcohol and becomes Fe_3O_4, which is instantly reconverted into Fe_2O_3 by receiving oxygen from the air, and this alternation is constantly going on in every portion of the glowing mass. It is not a mere *action de présence*, but alternate reduction and oxidation of the sesquioxide, producing a continuous oxidation of the alcohol.

This suggests a consideration apparently adverse to the atomic hypothesis of Dr. Dalton. How can a single compound molecule Fe_2O_3 be changed by deoxidation into another compound molecule Fe_3O_4, when, according to theory, there are in it but *two* combining proportions of iron, whereas the resultant contains *three*? and how (by deoxidation) can the resultant molecule contain *four* combining proportions of oxygen when the primary contained only *three*? We can indeed represent to the imagination that *three* molecules of the sesquioxide, acting as if they were one triple molecule, lose *one* combining proportion of oxygen, and are converted into *two* molecules of the black oxide; and conversely, that *two* molecules of the black oxide, acting as if they were *one* double molecule, combine with *one* atom of oxygen, and are converted into *three* atoms of sesquioxide. The only way to account for this, in accordance with the popular atomic theory, seems to be, to assume that the notation for these oxides is incorrect, and that

for Fe_2O_3 we should write Fe_6O_9,
and for Fe_3O_4 we should write Fe_6O_8.

If the current notation be retained, and any law be admitted, in virtue of which three molecules of sesquioxide may suffer reduction as if they were only one molecule, and divide into two molecules of the magnetic oxide, we might conceive a peculiar structure in the Fe_3O_4, with a tendency to separate again into FeO, Fe_2O_3; that it is really in combination as Fe_3O_4, but ready to yield to slight causes and become FeO, Fe_2O_3. This would explain Mr. Mercer's experiment (quoted by Brande, I. 716, edition 1848) of the chemical union of a mechanical mixture of protoxide and peroxide of iron.

Perhaps the sesquioxides occupy a middle place in the scale of effects. Take the case of iron; we have

$Fe+\mathbf{O}$, pyrophorus,—violent oxidation,
$Fe_2O_3+\mathbf{NH_3}+\mathbf{O}$, alternate reduction and oxidation,
$FeO_3+\mathbf{NH_3}$ **(in water)**, reduction.

To show the last, add ammonia to a solution of FeO_3, KO, and Fe_2O_3 will be precipitated.

A mixture of ten parts by weight of powdered chlorate of potassa with one part of Fe_2O_3 disengages oxygen with extreme facility and great œconomy of heat as compared with the oxides of copper and manganese; and it is the more convenient because *n* grains of the mixture will represent almost exactly *n* cubic inches of disengaged oxygen.

A state of mechanical division is not absolutely necessary for the catalysation of some inflammable vapours by Fe_2O_3; an old nail, entirely transmuted into rust, will perform the operation; and when we consider that in many cases of fermentation, decay and putrefaction, this oxide may be present, divided or aggregated, while heat is evolved, and inflammable gases and vapours are set free, we may hereafter be able to trace some instances of "spontaneous combustion" to the catalytic action of the sesquioxides of iron.

"Ocular Spectres and Structures as Mutual Exponents." By James Jago, A.B. Cantab., M.B. Oxon.

The present communication is a revised and modified version of a paper bearing the same title, which was read on the 18th of January and 1st of February, and which was, by permission, withdrawn. The chief modification applies to the author's views respecting the structure of the vitreous body as deduced from entoptical phænomena. He is now of opinion that the arborescent system of which he infers the existence in that part of the eye (Phil. Mag. vol. ix. p. 305) does not consist of *tubes* filled with globules or cells, as he at first supposed, but of cell-constituted filaments. In a note, dated March 27, 1855, he gives the following enunciation of his present views as to the structure of the vitreous body:—

"In the vitreous humour are innumerable transparent globules, beads or cells, of less specific gravity than the fluid, extremely minute, and of uniform size, which are arranged, *without exception*, in rows to form the threads of a lax, elongated, irregular web, springing from the general surface of the capsule by, commonly, exquisitely small meshes, and extending into the interior by others of increasing size, so that the innermost part of the web—which lies nearer the circumference of the vitreous body than its centre—consists of comparatively large ones.

"Whenever the eye rotates, this filamentous peripheral system will in its relative (counter) rotation, ***gradually, though soon,*** come to the end of its tether, and in this interval and when there, act as a check upon the relatively rotating fluid; perpetually reiterating obstruction, above all in the immediate vicinity of the capsule and (by the disposition of the threads to float vertically) most effectually in the most important or horizontal direction. The middle of the vitreous body being free of impediment, relative rotation of the fluid expends itself there, whilst a practical concurrence in the ocular rotation ensues near the capsule. And thus in the incessant movements of the eye, head and body, the wall that confines the fluid can suffer no severe concussions from eddies in the latter. In other words, we have herein a provision that the crystalline lens may not be shaken, the circulation in the retinal vessels may not be deranged, and sensations of light may not be ever assailing us from impulses of the vitreous fluid, perhaps that the retina may not itself suffer direct injury therefrom."

"An Account of some Experiments made with the Submarine Cable of the Mediterranean Electric Telegraph." By Charles Wheatstone, F.R.S.

The following results were obtained between May 24 and June 8 in last year, with the telegraphic cable manufactured by Messrs. Kuper and Co. of East Greenwich, for the purpose of being laid across the Mediterranean sea, from Spezia on the coast of Italy to the island of Corsica. The manufacturers, in conjunction with Mr. Thomson the engineer of the undertaking, kindly afforded me every facility in carrying on the experiments. The short time that elapsed between the opportunity presenting itself and the shipping of the cable for its destination, prevented me from determining with sufficient accuracy some points of importance, respecting which I was only able to make preliminary experiments, but the following, which I was able to effect with the means at hand, may possess sufficient interest to be made public. They present perhaps nothing theoretically new, but I am not aware that experimental verifications of some of these points have been made before. I assume that the reader is acquainted with the experiments of Dr. Faraday described in the Philosophical Magazine, Ser. 4, vol. vii. p. 197.

The cable was 110 miles in length, and contained six copper wires, one-sixteenth of an inch in diameter, each separately insulated in a covering of gutta percha one-tenth of an inch in thickness. The whole was surrounded by twelve thick iron wires twisted spirally around it, forming a complete metallic envelope one-third of an inch in thickness. A section of the cable presented the six wires arranged in a circle of half an inch diameter, and one-fifth of an inch from the internal surface of the iron envelope.

The cable was coiled in a dry well in the yard, and one of its ends was brought into the manufactory. The wires were numbered 1, 2, 3, 4, 5, 6, and the ends in the well were indicated by an accent;

the ends 1′2, 2′3, 3′4, 4′5, 5′6 were connected by supplementary wires, so that the electric current might be passed in the same direction through all the six wires joined to a single length, or through any lesser number of them, the connexions being made at pleasure in the experimenting room.

The rheomotor employed was an insulated voltaic battery consisting of twelve troughs, each of twelve elements, which had been several weeks in action.

First Series.

The following experiments show that the iron envelope of the compound conductor gives rise to the same phænomena of induction which occur when the insulated wire is immersed in water, as in Dr. Faraday's experiments.

Exp. 1. One end of the entire length, 660 miles, was brought in connexion with one of the poles of the battery, the other end remaining insulated. The wire became charged with negative electricity when its end touched the zinc pole, and with positive electricity when it communicated with the copper pole. A current, indicated by a galvanometer placed near the battery, existed as long as the charge was going on, and ceased when it arrived at its maximum. [The feeble current attributed to imperfect insulation, which continues as long as the contact with the battery remains, is here left out of consideration.] When the wire was charged, and the discharge effected by a wire communicating with the earth, the current produced was in the same direction, whether the discharge was made near the battery or at the opposite end; *i. e.* the current in both cases proceeded from the wire to the earth in the same direction.

Exp. 2. On bringing one end of the wire in contact with one of the poles of the battery, the other pole having no communication with the earth, the wire remained uncharged. A very slight and scarcely perceptible tremor was observed in the galvanometer needle interposed between the battery and the wire.

Exp. 3. To each of the poles of the battery was attached a wire 220 miles in length, and similar galvanometers were interposed between the two wires (the remote extremities of which remained insulated) and the battery. So long as one wire alone was connected with the battery no charge was communicated to it, but on connecting the other wire with the opposite pole both wires were instantaneously charged, as the strong deflection of both needles rendered evident. On bringing the free end of one of the wires in communication with the earth it alone was discharged, the other wire remaining fully charged.

Second Series.

Exp. 4. One pole of the battery was connected with the earth, and the other with 660 miles of wire, which had an earth communication at its opposite end; three galvanometers were interposed in the course of the conductor; the first near the battery, the second in the middle of the wire, *i. e.* 330 miles from each extremity, and the third at the remote end near the communication with the earth. When the connexion of the battery with the wire was completed, the galvanometers were successively acted upon in the order of their distances from the battery, as in the experiments recorded by Dr. Faraday. When the earth connexion at the remote extremity of the wire, on the contrary, was completed, the disturbance of equilibrium commenced at this end, and the galvanometers successively acted in the reverse order, *i. e.* the galvanometer which was the most distant from the battery was the first impelled into motion. In the latter case, before the completion of the circuit the needles of the galvanometers had assumed constant deflections to a limited extent, owing to a feeble current arising from the uniform dispersion of the static electricity along the wire.

Exp. 5. The two extremities of the 660 miles of wire were brought into connexion with the opposite poles of the battery. When one of the ends previously disconnected from the battery was united therewith, the galvanometers at the extremities of the wire, and consequently which were at equal distances from the poles of the battery, were immediately and simultaneously acted upon, while that which was in the middle of the wire was subsequently caused to move. When the wire disconnected in the middle instead of near one of the poles of the battery was again united, the middle galvanometer, which was the most remote from the battery, was the first acted upon, and those near the poles subsequently.

The comparison of the two above-mentioned experiments show that the earth must not be regarded simply as a conductor, which many suppose to be the case. Since in the first experiment there were not many yards' distance between the two earth terminations, did the extent of ground between them act only as a conductor, the two galvanometers at the extremities of the wire should have acted simultaneously, as in the second experiment, and as would have been the case had a short wire united the two extremities which proceeded to the earth.

Third Series.

Exp. 6. One pole of the battery was connected with the earth, and the opposite pole with one extremity of the 660 miles of wire, the other end remaining insulated; a delicate galvanometer was interposed near the battery. Notwithstanding there was no circuit formed, the needle showed a constant deflection of 33½°; the feeble current thus rendered evident is not so much to be attributed to imperfect insulation, as to the uniform and continual dispersion of the static electricity with which the wire is charged throughout its entire length, in the same manner as would take place in any other charged body placed in an insulating medium. The strength of the current thus occasioned appears to be nearly, if not exactly, proportional to the length of the wire added, as the following table will show: the first column indicates the number of miles of wire subjoined beyond the galvanometer, and the second the corresponding deflections of the needle:—

miles.	°
0	0
110	6½
220	12
330	18
440	23½
550	28
660	31

Exp. 7. One end of the 660 miles of wire was now allowed to remain constantly in contact with one of the poles of the battery; but the galvanometer was successively shifted to different distances from the battery. The strength of the current was now shown to be inversely as the distance of the galvanometer from the battery, becoming null at its extremity, as shown in the following table. The first column shows the distance from the battery at which the galvanometer was placed, and the second column the corresponding deflection of the needle.

miles.	°
Near the battery	33½
110	31
220	25
330	15
440	12
550	5
660	0

The deflections of the needle of the galvanometer employed in these experiments were, when they did not surpass 36°, very nearly comparable with the force of the current. This I ascertained in the following way. I took six cells of the small constant battery described in my paper "On new Instruments and Processes for determining the Constants of a Voltaic Circuit," printed in the Philosophical Transactions for 1843, and placed in the circuit formed of the 660 miles of wire, the earth, and the galvanometer, successively 1, 2, 3, 4, 5 and 6 cells. Leaving out of consideration the resistances in the cells themselves and in the earth, which were very inconsiderable in comparison with that in the long wire, the force of the current should be approximately proportionate to the number of the elements; and since the deflections of the needle nearly indicated this proportionality, as the following table will show, it may be assumed that the force of the current, when the deflection of the needle did not surpass 36°, nearly corresponded with the angular deviation.

cell.	°
1	6
2	14
3	19
4	28
5	32
6	36

From the preceding experiments (6. and 7.) it seems to result, that whatever length of wire is connected with the battery, if a galvanometer is placed at the farther extremity of the wire and a constant length added to the other termination of the galvanometer, its indication remains always nearly the same. Thus the galvanometer indicated 6½° when it was placed close to the battery and 110 miles of wire were subjoined beyond it; and 5° when 550 miles were interposed between the battery and galvanometer, the same length, 110 miles, being subjoined. In like manner, when 220 miles were added beyond the galvanometer placed near the battery, the indication was 12°; precisely the same as when 440 miles were interposed and 220 added. So also when 330 miles were added, the deviation of the galvanometer was 18°; and 15° when 330 miles were interposed and 330 added. I have no doubt that the correspondence would have been closer had it not been for the fluctuations of the battery.

It would appear from this, that whatever be the length of wire attached to the insulated pole of a battery, it becomes charged to the same degree of tension throughout its entire extent; so that another insulated wire brought into connexion with its free extremity exhibits precisely the same phænomena, in kind and measure, as when it is brought into immediate connexion with a pole of the battery. Some important practical consequences flow from this conclusion, which I will not develope at present, as I have not yet had an opportunity of submitting them to the test of experiment.

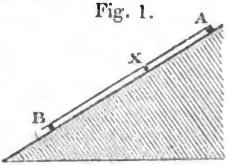
April 19, 1855.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Descent of Glaciers." By the Rev. Henry Moseley, M.A., F.R.S.

If we conceive two bodies of the same form and dimensions (cubes for instance), and of the same material, to be placed upon a uniform horizontal plane, and connected by a substance which alternately extends and contracts itself, as does a metallic rod when subjected to variations of temperature, it is evident that by the extension of the intervening rod each will be made to recede from the other by the same distance, and, by its contraction, to approach it by the same distance. But if they be placed on an inclined plane (one being lower than the other), then when by the increased temperature of the rod its tendency to extend becomes sufficient to push the lower of the two bodies downwards, it will not have become sufficient to push the higher upwards. The effect of its extension will therefore be to cause the lower of the two bodies to descend whilst the higher remains at rest. The converse of this will result from contraction; for when the contractile force becomes sufficient to pull the upper body down the plane it will not have become sufficient to pull the lower up it. Thus, in the contraction of the substance which intervenes between the two bodies, the lower will remain at rest whilst the upper descends. As often, then, as the expansion and contraction is repeated the two bodies will descend the plane until, step by step, they reach the bottom.

Fig. 1.



Suppose the uniform bar AB placed on an inclined plane, and subject to extension from increase of temperature, a portion XB will descend, and the rest XA will ascend; the point X where they separate being determined by the condition that the force requisite to push XA up the plane is equal to that required to push XB down it.

Let AX$=x$, AB$=$L, weight of each linear unit $=u$, $i=$ inclination of plane, $\phi=$ limiting angle of resistance,

$$\therefore \quad ux = \text{weight of AX},$$

$$u(\mathrm{L}-x) = \text{weight of BX}.$$

Now, the force acting parallel to an inclined plane which is necessary to push a weight W up it, is represented by

$$\mathrm{W}\frac{\sin(\phi+i)}{\cos\phi};$$

and that necessary to push it down the plane by

$$\mathrm{W}\frac{\sin(\phi-i)}{\cos\phi};$$

$$\therefore \quad ux\frac{\sin(\phi+i)}{\cos\phi}=u(\mathrm{L}-ux)\frac{\sin(\phi-i)}{\cos\phi}$$

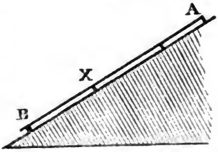
$$\therefore \quad x\{\sin(\phi+i)+\sin(\phi-i)\}=\mathrm{L}\sin(\phi-i)$$

$$\therefore \quad 2x\sin\phi\cos i=\mathrm{L}\sin(\phi-i)$$

$$\therefore \quad x=\frac{1}{2}\mathrm{L}\frac{\sin(\phi-i)}{\sin\phi\cos i}$$

$$\therefore \quad x=\frac{1}{2}\mathrm{L}\left\{1-\frac{\tan i}{\tan\phi}\right\}.$$

Fig. 2.



When contraction takes place the converse of the above will be true. The separating point X will be such, that the force requisite to pull XB up the plane is equal to that required to pull AX down it. BX is obviously in this case equal to AX in the other.

Let λ be the elongation per linear unit under any variation of temperature; then the distance which the point B (see fig. 1) will be made to descend by this elongation

$$=\lambda.\overline{\mathrm{BX}}$$

$$=\lambda(\mathrm{L}-x)$$

$$=\frac{1}{2}\lambda\mathrm{L}\left(1+\frac{\tan i}{\tan\phi}\right).$$

If we conceive the bar now to return to its former temperature, contracting by the same amount (λ) per linear unit; then the point B (fig. 2) will by this contraction be made to ascend through the space

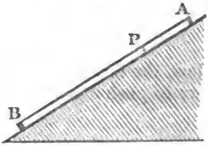
$$\begin{aligned} \mathrm{BX}.\lambda &= x\lambda \\ &= \frac{1}{2}\mathrm{L}\lambda\left\{1-\frac{\tan i}{\tan \phi}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1) \end{aligned}$$

Total descent l of B by elongation and contraction is therefore determined by the equation

$$l=\mathrm{L}\lambda\frac{\tan i}{\tan \phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

To determine the pressure upon a nail, driven through the rod at any point P fastening it to the plane.

Fig. 3.



It is evident that in the act of extension the part BP of the rod will descend the plane and the part AP ascend; and conversely in the act of contraction; and that in the former case the nail B will sustain a pressure upwards equal to that necessary to cause BP to descend, and a pressure downwards equal to that necessary to cause PA to ascend; so that, assuming the pressure to be downwards, and adopting the same notation as before, except that AP is represented by ρ, AB by a, and the pressure upon the nail (assumed to be downwards) by P, we have in the case of extension

$$\mathrm{P}=u\rho\frac{\sin(\phi+i)}{\cos\phi}-u(a-\rho)\frac{\sin(\phi-i)}{\cos\phi},$$

and in the case of contraction

$$\mathrm{P}=u(a-\rho)\frac{\sin(\phi+i)}{\cos\phi}-u\rho\frac{\sin(\phi-i)}{\cos\phi}.$$

Reducing, these formulæ become respectively

$$\mathrm{P}=\frac{u}{\cos\phi}\left\{2\rho\sin\phi\cos i-a\sin(\phi-i)\right\} \quad . \quad . \quad . \quad (3)$$

$$\mathrm{P}=\frac{u}{\cos\phi}\left\{a\sin(\phi+i)-2\rho\sin\phi\cos i\right\} \quad . \quad . \quad . \quad (4)$$

My attention was first drawn to the influence of variations in temperature to cause the descent of a lamina of metal resting on an inclined plane, by observing, in the autumn of 1853, that a portion of the lead which covers the south side of the choir of the Bristol Cathedral, which had been renewed in the year 1851, but had not been properly fastened to the ridge beam, had descended bodily 18 inches into the gutter; so that if plates of lead had not been inserted at the top, a strip of the roof of that length would have been left exposed to the weather. The sheet of lead which had so descended measured, from the ridge to the gutter, 19 feet 4 inches and along the ridge 60 feet. The descent had been continually going on, from the time the lead had been laid down. An attempt made to stop it by driving nails through it into the rafters had failed. The force by which the lead had been made to descend, whatever it was, had been found sufficient to draw the nails[*]. As the pitch of the roof was only 16½°, it was sufficiently evident that the weight of the lead alone could not have caused it to descend. Sheet lead, whose surface is in the state of that used in roofing, will stand firmly upon a surface of planed deal when inclined at an angle of 30°[†], if no other force than its weight tends to cause it to descend. The considerations which I have stated in the preceding articles led me to the conclusion that the daily variations in the temperature of the lead, exposed as it was to the action of the sun by its southern aspect, could not but cause it to descend considerably, and the only question which remained on my mind was, whether this descent could be so great as was observed. To determine this I took the following data:—

Mean daily variation of temperature at Bristol in the month of August, assumed to be the same as at Leith (Kaemtz, Meteorology, by Walker, p. 18), 8°·21 Cent.

Linear expansion of lead through 100° Cent. ·0028436.

Length of sheets of lead forming the roof from the ridge to the gutter 232 inches.

Inclination of roof 16° 32′.

Limiting angle of resistance between sheet lead and deal 30°.

Whence the mean daily descent of the lead, in inches, in the month of August, is determined by equation (2) to be

$$l = 232 \times \frac{8^\circ \cdot 21}{100} \times \cdot 0028436 \times \frac{\tan 16^\circ\ 32'}{\tan 30^\circ}$$

$$l = \cdot 027848 \text{ inches.}$$

The average daily descent gives for the whole month of August a descent of ·863288. If the average daily variation of temperature of the month of August had continued throughout the year, the lead would have descended 10·19148 inches every year. And in the two years from 1851 to 1853 it would have descended 20·38296 inches. But the daily variations of atmospheric temperature are less in the other months of the year than in the month of August. For this reason, therefore, the calculation is in excess. For the following reasons it is in defect:—1st, the daily variations in the temperature of the lead cannot but have been greater than those of the surrounding atmosphere. It must have been heated above the surrounding atmosphere by radiation from the sun in the day-time, and cooled below it by radiation into space at night. 2ndly. One variation of temperature only has been assumed to take place every twenty-four hours, viz. that from the extreme heat of the day to the extreme cold of the night; whereas such variations are notoriously of constant occurrence during the twenty-four hours. Each cannot but have caused a corresponding descent of the lead, and their aggregate result cannot but have been greater than if the temperature had passed uniformly (without oscillations backwards and forwards) from one extreme to the other.

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[*] The evil was remedied by placing a beam across the rafters near the ridge, and doubling the sheets round it, and fixing their ends with spike nails.

[†] This may easily be verified. I give it as the result of a rough experiment of my own. I am not acquainted with any experiments on the friction of lead made with sufficient care to be received as authority in this matter. The friction of copper on oak has, however, been determined by General Morin to be 0·62, and its limiting angle of resistance 31° 48′; so that if the roof of Bristol Cathedral had been inclined at 31° instead of 16°, and had been covered with sheets of copper resting on oak boards, instead of sheets of lead resting on deal, the sheeting would not have slipped by its weight only.

These considerations show, I think, that the causes I have assigned are sufficient to account for the fact observed. They suggest, moreover, the possibility that results of importance in meteorology may be obtained from observing with accuracy the descent of a metallic rod thus placed upon an inclined plane. That descent would be a measure of the aggregate of the changes of temperature to which the metal was subjected during the time of observation. As every such change of temperature is associated with a corresponding development of mechanical action under the form of work*, it would be a measure of the aggregate of such changes and of the work so developed during that period; and relations might be found between measurements so taken in different equal periods of times, successive years for instance, tending to the development of new meteorological laws.

The following are the results of recent experiments† on the expansion of ice:—

Linear expansion of ice for an interval of 100° of the Centigrade thermometer.

0·00524	Schumacher.
0·00513	Johrt.
0·00518	Moritz.

Ice, therefore, has nearly twice the expansibility of lead, so that a sheet of ice would, under similar circumstances, have descended a plane similarly inclined, twice the distance that the sheet of lead referred to in the preceding article descended. Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses by variations in the quantity and the temperature of the water, which flowing from the surface everywhere percolates them. That they must from this cause descend into the valleys is therefore certain. That portion of the Mer de Glace of Chamouni which extends from Montanvert to very near the origin of the Glacier de Léchaud, has been accurately observed by Professor James Forbes‡. Its length is 22,600 feet, and its inclination varies from 4° 19′ 22″ to 5° 5′ 53″. The Glacier du Géant, from the Tacul to the Col du

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* Mr. Joule has shown (Phil. Trans. 1850, Part I.) that the quantity of heat capable of raising a pound of water by 1° Fahr. requires for its evolution 772 units of work.

† Vide Archiv f. Wissenchaftl. Kunde v. Russland, Bd. vii. S. 333.

‡ Travels through the Alps of Savoy. Edinburgh, 1843.

Géant, Professor Forbes estimates (but not from his own observations, or with the same certainty) to be 24,700 feet in length, and to have a mean inclination of 8° 46′ 40″.

According to the observations of De Saussure, the mean daily range of Reaumur's thermometer in the month of July, at the Col du Géant, is 4°·257*, and at Chamouni 10°·092. The resistance opposed by the rugged channel of a glacier to its descent cannot but be different at different points and in respect to different glaciers. The following passage from Professor Forbes's work contains the most authentic information I am able to find on this subject. Speaking of the Glacier of la Brenva, he says, "The ice removed, a layer of fine mud covered the rock, not composed however alone of the clayey limestone mud, but of sharp sand derived from the granitic moraines of the glacier, and brought down with it from the opposite side of the valley. Upon examining the face of the ice removed from contact with the rock, we found it *set* all over with sharp angular fragments, from the size of grains of sand to that of a cherry, or larger, of the same species of rock, and which were so firmly fixed in the ice as to demonstrate the impossibility of such a surface being forcibly urged forwards without sawing and tearing any comparatively soft body which might be below it. Accordingly, it was not difficult to discover in the limestone the very grooves and scratches which were in the act of being made at the time by the pressure of the ice and its contained fragments of stone." (Alps of the Savoy, pp. 203–4.) It is not difficult from this description to account for the fact that small glaciers are sometimes seen to lie on a slope of 30° (p. 35). The most probable supposition would indeed fix the limiting angle of resistance between the rock and the under surface of the ice, set all over, as it is described to be, with particles of sand and small fragments of stone, at about 30°, that being nearly the slope at which calcareous stones will rest on one another. If we take then 30° to be the limiting angle of resistance between the under surface of the Mer de Glace and the rock on which it rests, and if we assume the same mean daily variation of temperature (4·257 Reaumur, or 5·321 Centigrade) to obtain throughout the length of the Glacier du Géant, which De Saussure observed in July at the Col du Géant; if, further, we take the linear expansion of ice at 100° Centigrade to be that (·00524) which was determined by the experiments of Schumacher; and, lastly, if we assume the Glacier du Géant to descend as it would if its descent were unopposed by its confluence with the Glacier de Léchaud, we shall obtain, by substitution in equation (2) for the mean daily descent of the Glacier du Géant at the Tacul, the formula—

$$l=24700\times 5{\cdot}321\times\frac{{\cdot}00524}{100}\times\frac{\tan 8^\circ 46'}{\tan 30^\circ}$$

$$l=1{\cdot}8395 \text{ feet.}$$

The actual descent of the glacier in the centre was 1·5 foot. If the Glacier de Léchaud descended at a mean slope of 5°, singly in a sheet of uniform breadth to Montanvert without receiving the tributary glacier of the Talèfre, or uniting with the Glacier du Géant, its diurnal descent would be given by the same formula, and would be found to be ·95487 foot. Reasoning similarly with reference to the Glacier du Géant, supposing it to have continued its course singly from the Col du Géant to Montanvert without confluence with the Glacier de Léchaud, its length being 40,420 feet, and its mean inclination 6° 53′, its mean diurnal motion l at Montanvert would, by formula (2) have been 2·3564 feet[*]. The actual mean daily motion of the united glaciers, between the 1st and the 28th July, was Montanvert (Forbes's 'Alps of the Savoy,' p. 140),—

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* Quoted by Professor Forbes, p. 231.

Near the side of the glacier	1·441 foot.
Between the side and the centre ..	1·750 foot.
Near the centre	2·141 feet.

The motion of the Glacier de Léchaut was therefore accelerated by their confluence, and that of the Glacier du Géant retarded. The former is dragged down by the latter.

I have had the less hesitation in offering this solution of the mechanical problem of the motion of glaciers, as those hitherto proposed are confessedly imperfect. That of De Saussure, which attributes the descent of the glacier simply to its weight, is contradicted by the fact that isolated fragments of the glacier stand firmly on the slope on which the whole nevertheless descends; it being obvious that if the parts would remain at rest separately on the bed of the glacier, they would also remain at rest when united.

That of Professor J. Forbes, which supposes a viscous or semi-fluid structure of the glacier, is not consistent with the fact that no viscosity is to be traced in its parts when separated. They appear as solid fragments, and they cannot acquire in their union properties in this respect which individually they have not.

Lastly, the theory of Charpentier, which attributes the descent of the glacier to the daily congelation of the water which percolates it, and the expansion of its mass consequent thereon, whilst it assigns a cause which, so far as it operates, cannot, as I have shown, but cause the glacier to descend, appears to assign one inadequate to the result; for the congelation of the water which percolates the glacier does not, according to the observations of Professor Forbes[†], take place at all in summer more than a few inches from the surface. Nevertheless it is in the summer that the daily motion of the glacier is the greatest.

The following remarkable experiment of Mr. Hopkins of Cambridge[‡], which is considered by him to be confirmatory of the sliding theory of De Saussure as opposed to De Charpentier's dilatation theory, receives a ready explanation on the principles which I have laid down in this note. It is indeed a necessary result of them.

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[*] On the 1st of July, the centre of the actual motion of the Mer de Glace at Montanvert was 2·25 feet.

[†] Travels in the Alps.

[‡] I have quoted the above account of it from Professor Forbes's book, p. 419.

"Mr. Hopkins placed a mass of rough ice, confined by a square frame or bottomless box, upon a roughly chiselled flag stone, which he then inclined at a small angle, and found that a slow but uniform motion was produced, when even it was placed at an inconsiderable slope." This motion, which Mr. Hopkins attributed to the dissolution of the ice in contact with the stone, would, I apprehend, have taken place if the mass had been of lead instead of ice; and it would have been but about half as fast, because the linear expansion of lead is only about half that of ice.

IX. *Intelligence and Miscellaneous Articles.*

ON THE ANÆSTHETIC PRINCIPLE OF THE *LYCOPERDON PROTEUS* AND CERTAIN OTHER FUNGI. BY THORNTON HERAPATH, ESQ.

THE smoke of the puff-ball, it is well known, has been long employed in some parts of the country, by apiarists, for stupefying bees. In a paper "On the Anæsthetic Properties of the *Lycoperdon proteus*, or common Puff-ball," which was read before the Medical Society of London in 1853, Mr. B. W. Richardson called particular attention to this fact, and stated that the fumes of the burning fungus produced the most perfect anæsthesia, not only in insects, but also in dogs, cats, rabbits, and probably in all the larger animals, and might consequently be applied as a substitute for the vapour of chloroform and æther in producing insensibility to pain in surgical practice. With the assistance of Dr. Willis, he said, he had removed a large tumour from the abdomen of a dog that had been placed under the influence of the narcotic, without any sign of pain being exhibited by the animal during the operation. From this gentleman's experiments it appeared, that when a moderate quantity of the fumes was inhaled slowly, the narcotism came on and passed off slowly, the animal exhibiting all the symptoms of intoxication, with convulsions and sometimes vomiting; but that when they were administered in larger quantity, life was invariably destroyed. The consideration of these and other facts induced Mr. Richardson to conclude, that the peculiar effects that were produced by the inhalation of the smoke of the puff-ball were caused by a volatile narcotic principle contained in the fungus, which was liberated by the action of heat, but was not absorbable by water, alcohol, or a strong alkaline solution. What the exact nature of this principle was, however, he confessed himself to be unable to determine. About eight or nine months ago I carefully repeated Mr. Richardson's experiments, and after making several futile attempts, at last, I believe, succeeded in isolating the narcotic constituent of the smoke.

The first step I considered it necessary to take in the investigation, was to determine in what part of the fungus the anæsthetic ingredient was contained; that is to say, whether in the sporules, the cellular tissue, or the matters soluble in water. I accordingly digested two or three ounces of the fungus, previously torn up into small pieces, in moderately warm water, and by means of pressure and washing, separated the sporules and soluble constituents from the cellular matter. Then, by allowing the water that had been used in this operation to remain undisturbed for several hours, the sporules were collected in the form of a dark brown-coloured, muddy deposit. This was well washed once or twice with water, and dried in an oven, as was also the cellular matter, and the watery solution was evaporated to dryness. On testing these three substances, it was found that only two of them, namely the sporules and the cellular tissue, were capable of producing anæsthesia; the aqueous extract evolved a thick irritating vapour, but this did not occasion insensibility on inhalation.

It was clear, therefore, that the narcotic principle should be looked for in the two former. Accordingly, small portions of each of them were digested for several hours in boiling alcohol, æther, bisulphide of carbon, wood-spirit, chloroform, diluted sulphuric acid, and fusel oil, but in every instance the residuary matter, when pressed and dried, was found to retain its original narcotic quality. Fresh quantities were then soaked for a considerable period in hot alkaline lye, and in a hot solution of moderately strong nitric acid, until nothing further was dissolved out by either of the reagents; the insoluble portion was well washed with water, and again dried in an oven. On this being tested as before, anæsthesia was found to be no longer produced.

In the next series of experiments I operated in a different way. I introduced the fumes of the burning fungus into bottles containing small quantities of liquor potassæ, dilute hydrochloric acid, alcohol, fusel oil, and diluted sulphuric acid. The bottles were then well shaken for several minutes, and the properties of the purified fumes were tested by introducing flies, bees, or wasps, secured by cement to the ends of long splinters of wood, into the bottles, and observing the effects. In every case, however, insensibility was still produced, thus showing that the narcotic quality of the fumes was not caused by any body soluble in these solutions. There being no substance with which I am acquainted, except carbonic oxide, nitrous oxide, and perhaps some compounds of cyanogen which possess all these properties; and having, moreover, in the mean time read a paper, by M. Adrien Chenol, "On Pure Oxide of Carbon, considered as a Poison*," it immediately occurred to me that it was the former of these substances that was the cause of the narcotism. I therefore specially examined the fumes for carbonic oxide, by agitating them with an acid solution of chloride of copper, and also by absorbing the carbonic acid, ammonia and oxygen, by means of lime-water, diluted muriatic acid, and a solution of the protosulphate of iron saturated with nitric oxide gas, when indications of the presence of carbonic oxide were readily obtained; the fumes, after agitation with the solution of chloride of copper, no longer induced narcotism; whilst those, on the contrary, which had been treated with the other solvents, were more than ordinarily powerful, and rendered an insect insensible much more quickly than before; they also burnt with a blue flame, and possessed all the well-known characters of the oxide of carbon. The correctness of this conclusion was, moreover, confirmed by experimenting with carbonic oxide prepared by acting on oxalic acid with oil of vitriol, and passing the gas evolved through caustic soda-ley. Even when largely diluted with air, it still continued to produce insensibility in insects, and acted in every way like the purified fumes of the *Lycoperdon**.

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* *Comptes Rendus*, No. 16, April 17, 1854.

It is not difficult to understand how carbonic oxide is formed by the ignition of the fungus, as this gas is invariably produced in larger or smaller quantity when certain organic substances are decomposed by heat, though some yield it in greater proportion than others; and consequently, as might have been anticipated, I find that the fumes of several other fungi act in the same manner towards animals as those of the *Lycoperdon proteus*. The principal of those to which I allude are the common *Lycoperdon* of the druggist, *L. giganteum*, and the mushroom, *Agaricus campestris*.

Old Park, Bristol,
May 25, 1855.

ON A STRONGLY FLUORESCENT FLUID. BY RUDOLPH BÖTTGER.

Dr. Böttger has informed Prof. Poggendorff, that a solution of platino-cyanide of potassium possesses the property of fluorescence in a still higher degree than sulphate of quinine. The solution of the metallic salt fluoresces with a yellowish light, resembling one of the colours exhibited by its dichroitic crystals.—Poggendorff's *Annalen*, vol. xcv. p. 176.

ON THE QUANTITATIVE DETERMINATION OF WATER.

BY DR. H. VOHL.

Water is almost always determined by the loss which a body undergoes when exposed to a high temperature. In those cases, however, in which the constituents of the body under investigation are liable to change when heated by taking up oxygen, the determination of water is attended with many difficulties, and unsatisfactory results are in consequence often obtained. The method usually employed consists in heating the body in an atmosphere free of oxygen, and passing the gas charged with watery vapour through a carefully weighed chloride of calcium tube. The loss of the substance should then be exactly equal to the increase in the weight of the chloride of calcium tube.

Another method which I have frequently employed with very satisfactory results, consists in mixing the readily oxidizable substances with an excess of bichromate of potash, pouring water over the mixture in a platinum or porcelain crucible, and then drying it, first in the water-bath and afterwards in the air-bath, at 392° Fahr. In many cases the mixture may be heated almost to redness over the flame of a lamp.

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* See also 'A Treatise on Poisons,' by Professor Christison, 4th edition, p. 827, for an account of the peculiar effects produced by the inhalation of the oxide of carbon.

The intermixture of bichromate of potash is a source of oxygen, which readily furnishes that substance to the oxidizable body, without, however, in any way altering the weight. Consequently if the weight of the substance employed, and of the bichromate of potash added to it be known, the loss, if no volatile body be present, gives only the quantity of the water contained in it, and volatilized from the substance under investigation. It is a matter of course that this substance should contain neither organic compounds nor ammoniacal salts. The addition of bichromate of potash is not always advisable, but in many cases the neutral salt must be employed, as in many salts the acid will have a decomposing action upon the chromic acid, giving rise to an evolution of oxygen or chlorine, and thus a higher amount of water will be obtained. This will occur, for instance, with all protosalts of iron containing sulphuric acid or chlorine; with these the neutral chromate of potash must always be employed, when, if the heat be not raised to redness, no decomposition of the chromic acid will take place. The chromate employed must be perfectly dry and free from all organic matter.

To obtain it in this state, the commercial salt is to be purified by crystallization, and the crystals obtained fused at a gentle heat (below redness) in a platinum or porcelain crucible. On cooling, the mass solidifies at first in a crystalline form, and afterwards falls into a coarse powder, which may be employed without further preparation, and should be weighed while still warm. The neutral chromate of potash is dried in a covered crucible and weighed whilst warm.—Liebig's *Annalen*, May 1855, vol. xciv. p. 216.

ON THE DETERMINATION OF LIME. BY H. VOHL.

Lime is usually determined either as carbonate or sulphate. A method which furnishes very exact results consists in precipitating the lime in the form of oxalate, washing the precipitate by decantation, and then treating it in the apparatus of Will and Fresenius with bichromate of potash and sulphuric acid. If a few particles float upon the surface during the decantations, they may be got rid of by the addition of a few drops of æther. The sulphuric acid sets free the oxalic acid of the lime-salt, which is then oxidized by the chromic acid, forming carbonic acid, which is evolved. From this loss the amount of lime is calculated in accordance with the following formula:—

$$\begin{aligned}&3(\mathrm{CaO}, \mathrm{C}^2\mathrm{O}^3) + \mathrm{KO}, 2(\mathrm{CrO}^3) + 7(\mathrm{SO}^3)\\ &\quad = 3(\mathrm{CaO}, \mathrm{SO}^3) + \mathrm{KO}, \mathrm{SO}^3 + \mathrm{Cr}^2\mathrm{O}^3 + 3(\mathrm{SO}^3) + 6(\mathrm{CO}^2),\end{aligned}$$

or for 84·396 of lime, 132·000 of carbonic acid will be evolved.

As the washing by decantation requires some practice, and is not always practicable, the oxalate of lime may be filtered and washed; then dissolved on the filter with very dilute boiling muriatic acid. The solution obtained, which may be reduced by evaporation if necessary, is then put into the apparatus for the evolution of the carbonic acid, and after it has been neutralized with ammonia, the chromate of potash is added. In order to prevent any evolution of chlorine, a small quantity of oxide of mercury must be added before the sulphuric acid is added to the mixture.—Liebig's *Annalen*, May 1855, vol. xciv. p. 217.

METEOROLOGICAL OBSERVATIONS FOR MAY 1855.

Chiswick.—May 1. Densely clouded: dry haze: clear. 2. Clear: sharp frost at night. 3. Fine: densely overcast. 4. Cloudy and cold: frosty. 5. Excessively dry air, with bright sun. 6. Fine. 7. Fine: overcast: boisterous at night. 8. Cloudy: clear, with sharp frost at night. 9. Fine: rain at night. 10. Cloudy. 11. Cloudy and fine: thunder-storm in afternoon: clear. 12. Cloudy and cold: very clear: frosty. 13. Overcast: boisterous, with heavy rain at night. 14. Cloudy. 15. Cloudy: rain. 16. Cloudy: clear and frosty at night. 17—19. Very fine. 20. Foggy: very fine. 21. Overcast. 22. Cloudy. 23—25. Fine. 26. Very fine: hot and dry. 27. Slight haze: cloudy: rain. 28. Rain. 29. Cloudy: showery. 30. Cold showers. 31. Rain.

Mean temperature of the month	48°·88
Mean temperature of May 1854	50 ·07
Mean temperature of May for the last twenty-nine years ...	53 ·72
Average amount of rain in May	1·85 inch.

Boston.—May 1. Cloudy. 2. Fine. 3. Cloudy: rain and snow P.M. 4—6. Cloudy. 7. Cloudy: rain A.M. and P.M. 8. Cloudy: rain, hail and snow P.M. 9. Cloudy. 10. Cloudy: rain A.M. and P.M. 11. Cloudy: rain P.M. 12. Fine. 13. Cloudy: rain P.M. 14. Cloudy. 15. Cloudy: rain A.M. 16, 17. Cloudy: rain P.M. 18—20. Cloudy. 21. Cloudy: rain A.M. and P.M. 22. Cloudy. 23—28. Fine. 29—31. Cloudy: rain A.M. and P.M.

Sandwick Manse, Orkney.—May 1. Cloudy A.M.: clear, fine P.M. 2. Drizzle A.M.: showers P.M. 3. Hail-showers A.M. and P.M. 4. Cloudy A.M.: clear P.M. 5. Cloudy A.M.: rain P.M. 6. Sleet-showers A.M. and P.M. 7, 8. Sleet-showers A.M.: cloudy P.M. 9. Hail, frost A.M.: clear P.M. 10. Bright A.M.: cloudy P.M. 11. Clear A.M. and P.M. 12, 13. Bright A.M.: cloudy P.M. 14. Cloudy A.M.: clear P.M. 15. Showers A.M.: cloudy P.M. 16. Cloudy A.M.: showers P.M. 17. Bright A.M.: drops P.M. 18. Bright A.M.: cloudy P.M. 19. Showers A.M.: cloudy P.M. 20. Bright A.M.: clear, fine P.M. 21. Cloudy A.M. and P.M. 22. Small rain A.M. and P.M. 23. Small rain A.M.: clear, fine P.M. 24, 25. Hazy A.M. and P.M. 26. Drops A.M.: cloudy P.M. 27. Hazy A.M. and P.M. 28. Cloudy A.M. and P.M. 29. Bright A.M.: clear P.M. 30. Cloudy A.M. and P.M. 31. Clear A.M. and P.M.

This month has been remarkably cold, the mean temperature being lower than that of any May during the twenty-eight years of my observations,—4°·18 below the average, and the first half of it was only about the average temperature of March.

Mean temperature of May for twenty-eight previous years .	47°·99
Mean temperature of this month	43 ·81
Mean temperature of May 1854	48 ·39
Average quantity of rain in May for fourteen previous years	1·68 inch.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London*; *by* Mr. Veall, *at* Boston; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. May.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·063	30·019	29·73	30·37	30·36	52	32	44	47	41	e.	nw.	calm	....	....	....
○ 2.	30·047	29·857	29·79	30·04	29·80	62	20	45	45½	37	ne.	ne.	nw.	....	....	·04
3.	29·702	29·638	29·37	29·99	29·86	64	35	46	33	33	n.	n.	nnw.	....	....	·10
4.	29·865	29·704	29·47	29·86	29·80	50	24	41·5	36½	37	ne.	nne.	sw.	....	·08	·09
5.	29·930	29·914	29·66	29·43	29·19	57	30	44	44½	40	ne.	nne.	sw.			
6.	29·951	29·938	29·50	29·30	29·49	63	41	52	41	41	w.	wsw.	w.	....	....	·31
7.	29·884	29·719	29·52	29·54	29·47	60	33	50	44	40½	sw.	ssw.	nw.	·06	·08	·21
8.	29·830	29·772	29·33	29·63	29·69	56	21	46	42	39	nw.	nw.	wsw.	·04	·09	·19
☾ 9.	30·004	29·616	29·60	29·71	29·73	62	43	46·5	38½	35	sw.	ssw.	ne.	·09	·20	·09
10.	29·508	29·409	29·05	29·57	29·56	63	44	49	42½	39	sw.	nw.	e.	·04	....	....
11.	29·624	29·366	29·00	29·66	29·80	59	34	51·5	42½	38	sw.	n.	nnw.	·36	·24	....
12.	29·831	29·780	29·43	29·82	29·82	53	28	43·5	44½	39½	n.	n.	n.	....	·03	....
13.	29·618	29·406	29·27	29·77	29·84	50	38	51	44	39½	se.	s.	n.	·49	....	....
14.	29·678	29·615	29·38	29·83	29·77	57	35	47	44½	39	ne.	n.	n.	....	·02	·05
15.	29·572	29·551	29·26	29·75	29·77	48	41	48	45	44½	ne.	n.	n.	·05	....	·08
● 16.	29·832	29·696	29·34	29·74	29·72	50	30	48	47	43½	n.	n.	nw.	....	·05	....
17.	29·990	29·901	29·48	29·75	29·84	61	30	51	45	41½	nw.	nw.	nw.	·01	·02	·04
18.	30·109	30·078	29·70	29·94	29·92	64	39	49	49	43½	sw.	wsw.	e.	....	·15	....
19.	30·025	29·860	29·60	29·85	29·92	69	48	59·5	50	49	s.	sw.	sse.	....	....	·02
20.	29·883	29·800	29·47	30·04	30·14	68	35	61	54½	47	nw.	ne.	e.	....	....	....
21.	29·862	29·830	29·47	30·10	29·99	55	46	47	46	45	w.	n.	nne.	....	....	....
22.	29·840	29·779	29·45	29·83	29·70	64	31	50	47	47	n.	nw.	ne.	·01	·16	....
23.	29·819	29·780	29·40	29·62	29·67	60	40	50	49	47	sw.	ssw.	e.	....	....	·16
☽ 24.	29·766	29·759	29·36	29·72	29·81	71	44	58	46½	46	s.	sw.	ese.	....	....	....
25.	29·802	29·789	29·40	29·92	30·00	78	42	61	52½	49½	se.	se.	ese.	....	....	....
26.	29·876	29·759	29·46	30·08	30·13	82	58	63	52	45½	e.	e.	e.	....	....	....
27.	29·749	29·723	29·40	30·14	30·12	75	52	57·5	47	45	se.	e.	ne.	·10	....	....
28.	29·856	29·789	29·42	30·10	30·14	56	35	51	45½	44½	ne.	ne.	ne.	·09	....	....
29.	29·893	29·872	29·50	30·22	30·28	59	37	47	49½	44	n.	nnw.	nne.	·02	....	....
30.	29·949	29·879	29·62	30·27	30·25	50	40	42·5	46	45	n.	ne.	ne.	·14	·14	....
○ 31.	29·683	29·450	29·43	30·25	30·25	54	47	45·5	47	45½	ne.	ne.	e.	·46	....	....

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X. *On the Formation of Indigo-blue.*—Part I.

By EDWARD SCHUNCK, *Ph.D., F.R.S.*[*]

INDIGO, one of the most important and extensively used dyes, owes its value entirely to a peculiar colouring matter contained in it, to which the name of *indigo-blue* or *indigotine* is applied by chemists. This substance has been repeatedly subjected to investigation, and several distinguished chemists have bestowed their attention and labour upon it. Its properties, composition, and products of decomposition, have been so carefully examined, that it may safely be asserted that there are few organic substances whose nature is more accurately known than that of indigo-blue. If, however, we inquire into the state of our knowledge regarding the origin and mode of formation of this body, it will be found that our information on this part of the subject is extremely defective. Indigo-blue may be obtained from a variety of plants, which, though belonging to the most different genera and orders, are rather limited in number. It has sometimes been observed to form in the milk of cows, especially such as have been fed exclusively on saintfoin[†]. Latterly it has been discovered by Hassall[‡] and others[§] in human urine, where its occurrence is attributed to a morbid state of the system. It is therefore a substance which is formed sparingly indeed, but in widely distant parts of the organic world. The properties of indigo-blue, which are so peculiar as almost to separate it from all other organic bodies, and to constitute it one *sui generis*, naturally suggest the inquiry, in what form it is contained in the plants and animals from which it is derived. If it exists ready formed in the indigo-bearing plants, how is it that though, when in a free state, insoluble in water, acids, alkalies, alcohol, and most simple menstrua, it should so easily be extracted from those plants by a mere infusion with cold water? If it does not pre-exist in the plant, in what state of combination is it contained therein, and what is the nature of the process by which it is eliminated? The usual method of preparing indigo from the *Indigoferæ* consists in steeping the plant, especially the leaves, in water, drawing off the infusion, allowing it to undergo fermentation, and then precipitating by means of agitation with air and the addition of lime-water. Now it may be asked, is this process of fermentation, which is often very tedious and difficult to manage, essential to the formation of indigo-blue, or is it merely an accidental phænomenon attending its preparation? If it is essential, at what stage of the process is the formation of the colouring matter to be considered as completed; and is it necessary, as some persons assert, to continue it until actual putrefaction has commenced, or not? These are points, which, though perhaps of little consequence to the dyer and consumer of indigo, are of great interest in a chemical point of view, and are of the greatest importance to the manufacturer of indigo. To the latter it must surely be extremely desirable to know the exact nature of the process on which his manufacture depends, and to ascertain whether this process yields into his hands the whole quantity of the product which the material employed is capable of yielding, and also whether the manner of conducting it is in perfect accordance with theoretical requirements. If, however, we consult the authors who have written on this subject, and the chemists who have endeavoured to elucidate it, we shall obtain very unsatisfactory replies to our inquiries. The views entertained on these different points are either mere surmises, or they are conclusions founded on a limited number of frequently imperfect experiments. The chief cause of our ignorance on these questions is, probably, that the process of manufacturing indigo is one carried on, not in the more highly civilized regions, but in remote parts of the world; and we are consequently obliged to rely for our knowledge concerning it chiefly on the accounts of travellers, who are usually possessed of merely general information, or of the manufacturers themselves, who are far from competent to give an opinion on a complex organo-chemical process.

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[*] From the Memoirs of the Literary and Philosophical Society of Manchester, vol. xii.; read April 3, 1855.

[†] *Annales de Chimie et de Physique*, vol. iii. p. 269.

[‡] Philosophical Transactions for 1854.

[§] *Ann. der Chem. und Pharm.* vol. xc. p. 120.

Fourcroy, according to Robiquet, considered the formation of indigo-blue to be a result of the process of fermentation employed in its preparation.

Roxburgh, who is the earliest authority I have had an opportunity of consulting on this subject, states that his predecessor, De Cossigny, whose work on the manufacture of indigo, published in the Mauritius, is very rare, was of opinion that volatile alkali was the agent by which the colouring matter was extracted from the plant and held in solution until volatilized by the agitation process. Roxburgh, who, like De Cossigny, was one of the few possessing special chemical information who have examined the process of manufacturing indigo from the *Indigoferæ* on the spot, concluded, from his experiments, "that the indigo plants contain only the base of the colour, which is naturally green; that much carbonic acid is disengaged during its extrication from the leaves; that the carbonic acid is the agent whereby it is probably extracted and kept dissolved; that ammonia is not formed during the process; that the use of alkalies is to destroy the attraction between the base and the carbonic acid; and that the vegetable base being thereby set at liberty, combines with some colouring principle from the atmosphere, forming therewith a coloured insoluble fecula, which falls to the bottom and constitutes indigo*." Roxburgh first directed attention to the fact, that it is possible to obtain indigo by merely treating the plant with hot water, and then agitating the infusion with air, from which it follows that fermentation is not an absolutely essential condition of the formation of indigo-blue.

Chevreul†, who was the first chemist of any eminence who examined the indigo-bearing plants and their constituents, inferred from his analyses of the *Isatis tinctoria* and *Indigofera anil*, that these plants contain indigo in the white or reduced state, in the same state in which it exists in the indigo vat; that in this state it is held in solution by the vegetable juices; and that when this solution is removed from the plant, it is converted by the action of the atmospheric oxygen into indigo-blue. The authority of so distinguished an investigator as Chevreul has had great weight with chemists, and most persons have adopted his view without question, though it is founded chiefly on the fact of the colouring matter being deposited from a watery extract of the plant, and of the only form in which it is known to be soluble in water being that of reduced indigo.

According to Michelotti‡, the extraction of indigo consists simply in dissolving a compound of malic acid and indigo, which is afterwards decomposed by the precipitants employed.

A few years after the appearance of Chevreul's memoirs, Giobert, Professor of Chemistry at Turin, published a work on

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* Transactions of the Society of Arts, vol. xxviii.

† *Ann. de Chim.* sér. 1. vol. lxvi. p. 5; vol. lxviii. p. 284.

‡ *Journal de Physique*, Avril 1812.

Woad*, which enunciates ideas on this subject far more nearly approaching the truth than those either of his predecessors or successors on the same field of investigation. The chief conclusions at which he arrived, partly by experiment and partly by reasoning, are contained in the following propositions:—1. Indigo-blue does not pre-exist in the plant, but is formed by the operations by means of which we believe it to be extracted. 2. There exists in a small number of plants a peculiar principle, different from all the known proximate constituents of plants, and which has a tendency to be converted into indigo; this principle may be called *indigogene*. 3. This principle differs from indigo in containing an excess of carbon, of which it loses a portion in passing into the state of indigo-blue, by means of a small quantity of oxygen which it takes up. 4. The loss of this portion of carbon is caused by the latter undergoing combustion, and being converted into carbonic acid. 5. It differs in its properties from common indigo in being colourless, in being soluble in water, and by its greater combustibility, which causes it to undergo spontaneous combustion at the ordinary temperature of the atmosphere. 6. Its combustibility is enhanced by heat and by combination with alkalies, especially lime; it is diminished by the action of all acids, even carbonic acid.

About the year 1839, the *Polygonum tinctorium*, an indigo-bearing plant indigenous to China, became the subject of a series of investigations by several French chemists, chiefly in order to ascertain whether this plant, if grown in France, could be advantageously employed for the preparation of a dye to substitute foreign indigo, so as to obviate the necessity of paying such large sums to foreign nations for this article, a necessity which seems at all times to have been a subject for extreme regret in France. Baudrimont and Pelletier, after an examination of this plant, concurred in the opinion of Chevreul, that the indigo is contained in it as reduced indigo; and the latter adduced in support of this view an experiment, which consisted in treating fresh leaves of the *Polygonum* with æther, taking care to exclude the air, until the green colour had changed to white, when, on exposure to the atmosphere, they speedily became blue. Robiquet†, Colin‡, Turpin§, and Joly||, on the other hand, expressed

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* *Traité sur le Pastel et l'Extraction de son Indigo.* Paris, 1813.

† *Journal de Pharmacie*, vol. xxv. p. 62.

‡ *Mémoire sur la Renouée des Teinturiers.* Paris, 1839.

§ *Etudes microscopiques sur le gisement de la matière bleue dans les feuilles du* Polygonum tinctorium, *Mémoire lu à l'Académie des Sciences le* 12 *Novembre*, 1838.

|| *Observations générales sur les plantes qui peuvent fournir des couleurs bleues à la teinture.* Montpellier, 1839.

a very decided conviction that indigo-blue pre-exists in the *Polygonum tinctorium,* but not in a free state; that it is combined with some organic substance or substances, which render it soluble in water, æther and alcohol; and that it requires the operation of potent agencies in order to destroy this combination and set the indigo at liberty. Osmin Hervey*, in a memoir on the *Polygonum tinctorium,* which in some parts is rather obscure, inferred from his experiments:—1. That indigotine exists in the leaves of this plant in a state of combination with a resin. 2. That this natural compound of indigo and resin contains both white indigo and blue indigo, and of the latter a larger proportion the older the leaves are. 3. That by the influence of certain organic substances, the indigo-blue is again reduced to the colourless state, if the solution be effected by means of water, without any destruction of the natural compound taking place. Girardin and Preisser† again returned to Chevreul's view, that the colouring matter is contained in the leaves of this plant in the form of reduced indigo. Since the publication of these treatises, no new ideas have, as far as I know, been promulgated by chemists in reference to this subject‡.

It will be seen that the opinions of the chemists which I have just shortly reviewed are of three kinds, and may be stated as follows:—1. Indigo-blue exists ready formed in the plants from which it is derived. 2. It is contained in these plants in the form of reduced indigo. 3. It does not pre-exist in the vegetable, but is formed subsequently to the extraction of the latter by means of a process of fermentation, a process which manifests itself by the evolution of gases of various kinds. To each of these views very strong objections may be raised. If the colouring matter is formed at once in the plant, it is difficult to conceive by what means it comes to be dissolved by water, for no combination of indigo-blue with any organic substance can be produced which is soluble in water. If to this it be objected, that a compound of this nature is produced by the plant, and cannot after decomposition be reproduced, then it is at once admitted that indigo-blue is not contained as such within the vegetable. That it cannot exist as reduced indigo is evident, since the latter requires the presence of some alkali for its solution in water, and the juice of most, if not all, indigo-bearing plants is acid. It is difficult, moreover, to conceive how deoxidized indigo, a body having so great an affinity for oxygen, can exist in the interior of plants which we know are constantly evolving that element. That the colouring matter is formed by the process of fermentation to which the extract of the plant is subjected, as it is the oldest, so it is the most probable view. Nevertheless, the fact that indigo may be procured from plants by mere infusion with hot water and precipitation with lime-water, without any of the usual signs of fermentation being manifested, appears to militate against this view. On one point all authorities seem to agree, viz. that the contact with oxygen is a necessary condition of the formation, or at least precipitation of the indigo from the watery extract*.

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* *Journal de Pharmacie,* vol. xxvi. p. 290. † Ibid. p. 344.

‡ I cannot refrain from expressing on the present occasion my regret, which is probably shared by many others, at the want of a general chemical bibliography, comprising references to all the known works, treatises, papers, &c. on chemical subjects written since the commencement of the modern æra in chemistry. In searching for the authorities referred to in this paper, I have felt this want very sensibly. It is with some difficulty that the mere names of all the works and memoirs relating to any special branch of the science, particularly such as have fallen into oblivion, are discovered. The only attempt to supply this deficiency, and that only in regard to one department of chemistry, is Wolff's *Quellenliteratur der Organischen Chemie.* Halle, 1845.

Such being the state of our knowledge on this rather obscure department of chemical science, I resolved, though without anticipating any very decided success, to endeavour to throw a little more light on it. I was induced to do so chiefly by the following consideration. The principal vegetable colouring matters have now been discovered to be not direct products of the vital energy of plants, but products of decomposition of substances contained in the vegetable, which are themselves mostly colourless. The formation of these colouring matters takes place equally well out of the plant as within it. Indeed, it is probable that it never happens within the plant until decay has commenced, or at least until the vital energy has begun to decline. The processes of decomposition by which colouring matters are formed from other substances are of two kinds. The first consists in the absorption of oxygen and the elimination of hydrogen in the form of water; it is a process of decay (*Verwesung*, Liebig), and requires the presence, not only of oxygen, but of some alkali or other base. The second process is one which consists in the splitting up of the original compound into two or more simpler bodies, of which one or more are colouring matters; it is a process of fermentation, and may in general be effected as well by the action of strong acids as by that of ferments. The first process gives rise to colouring matters of a very fugitive nature, such as the colouring matters of logwood and archil. Indeed, in this case the colouring matter, if this name be applied merely to substances endowed with a striking and positive colour, is only one of a long chain of bodies succeeding one another, and is generally not the last product of decomposition. The other process, of which the formation of alizarine is an example, yields colouring matters of a fixed and stable character, which are not further changed by a continuance of the process to which they owe their formation. Now if indigo-blue be a body which is formed from some colourless substance existing in the plant, we should infer *à priori* that the process by which it is formed is one of fermentation or putrefaction, not requiring the intervention of oxygen or of alkalies; a conclusion, however, so much at variance with the generally received ideas on the subject of the formation of indigo-blue, as to require the aid of very decisive experiments for its establishment.

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* Gehlen is the only chemist who, as far as I am aware, has asserted that the agitation with air in the manufacture of indigo may have for its object, not so much the oxidation as the aggregation or separation of the particles of indigo from the solution. *Vide* Schweigger's Journal, vol. vi. 1812.

I shall now proceed to give an account of the experiments which I have undertaken with a view to elucidate this subject.

The only plant cultivated in this country which is known to yield indigo in any quantity is woad, *Isatis tinctoria*; and as it was necessary to examine the indigo-bearing plant in a fresh state and in considerable quantities, I had recourse to this one for the purpose. Having procured 10 lbs. of good French woad seed, I sowed it at the commencement of the spring of last year on about half an acre of land. It was sown in drills about two feet apart, each drill being previously well supplied with farmyard manure. In a short time the young plants appeared, and grew vigorously during the summer months. Some of the plants bore flowers, and ripened their seeds in the course of the autumn. At no time during the whole progress of the growth were there any visible indications of the presence of blue colouring matter on the leaves or stems. Some of the ripe seeds only were tinged with a dark purple colour, forming a thin coating on the exterior. The greater proportion of the plants, however, bore leaves which did not exhibit the glaucous appearance nor the fleshy consistency, which, according to authors, are characteristic of the cultivated variety of woad. They were, on the contrary, of a bright grass-green, and possessed but little succulence—characters which belong rather to the wild variety.

As soon as I could collect a small quantity of leaves, I commenced my experiments. Having taken some leaves, I chopped them fine and then extracted them with boiling water. The filtered liquid was light brown and transparent; it had a bitter taste and an acid reaction. It deposited no indigo-blue, however long it might be left exposed to the atmosphere, and hence it might have been inferred that it contained no indigo-blue. Nevertheless, a very simple experiment sufficed to show that it was capable of yielding an appreciable quantity of that colouring matter. On adding to it sulphuric or muriatic acid and boiling, it became of a darker colour, and deposited a quantity of dark brown, almost black flocks. Now these flocks contained indigo-blue; for if after collecting them on a filter and washing out the acid, they were treated with boiling alcohol, they communicated to the latter a bright blue colour; and on being treated with a boiling alkaline solution of protoxide of tin, they gave a yellow solution, which on exposure to the air became covered with a thin blue film. A small quantity of finely-chopped woad leaves having been pounded in a mortar with water until converted into a uniform green pulp, yielded on being strained through calico a dark green opake liquor. On heating this liquor to near the boiling-point, the vegetable albumen contained in it coagulated, carrying down with it the green colouring matter. On now filtering through paper, a green coagulum was left on the filter, while a clear light yellow liquid ran through. On adding acetate of lead to the liquid, a yellow precipitate fell; and on again filtering, the liquid ran through almost colourless. The lead precipitate being decomposed with dilute sulphuric acid, the filtered acid liquid was boiled, when it gave a few black flocks, which, however, contained no indigo-blue*. If, however, sulphuric or muriatic acid in excess were added to the liquid filtered from the lead precipitate, the liquid soon became green, and began to deposit indigo-blue even before it could be filtered from the sulphate or chloride of lead, and after filtration and boiling it yielded flocks containing an abundance of the colouring matter. If acetate of lead was added to an extract of woad leaves made either with hot or cold water, if the precipitate thereby produced was separated by filtration and ammonia was added to the filtrate, a pale yellow precipitate fell. This precipitate having been decomposed with sulphuric acid, the filtered acid liquid gave on boiling flocks containing indigo-blue, while the flocks obtained in a similar manner by boiling with acid the liquid filtered from this precipitate gave no indigo-blue. If this lead precipitate was treated with a cold concentrated solution of carbonate of soda, a yellow solution was obtained, which on being tried, as before, with sulphuric acid, afforded no indigo-blue; whilst the precipitate, on being treated with acid, gave indigo-blue as before. But if the lead precipitate was suspended in water, and a current of carbonic acid gas was passed for some time through the liquid, it was completely decomposed: its colour changed from yellow to white, and it now consisted almost entirely of carbonate of lead, whilst the liquid had acquired a yellowish colour, and on being boiled with acid, deposited a quantity of blue flocks, which consisted of indigo-blue in a state of great purity, as they dissolved in boiling alcohol with a beautiful blue colour, the alcohol depositing on cooling crystalline scales, which were blue by transmitted, and copper-coloured by reflected light. If, instead of adding acid to any of the solutions yielding indigo-blue, caustic soda was first added in excess and the solution was left for a few moments, and then boiled with an excess of acid, it merely became brown without depositing any indigo-blue. Having taken some finely-chopped woad leaves, I pounded them in a mortar with cold alcohol. On filtering, I obtained a clear green solution, leaving on evaporation at a gentle heat a green syrup, from which, on the addition of water, a quantity of chlorophyll and fatty matter separated in drops. The watery solution, which after filtration had only a yellowish tinge, on being boiled with the addition of sulphuric acid, deposited a quantity of purple flocks, which were treated, after filtration and washing with water, with successive portions of boiling alcohol. The first portions of the alcohol with which they were treated acquired a beautiful purple colour, and the last portions a pure blue, each portion depositing, on standing, some flocks of a fine blue colour. The green mass insoluble in water contained no indigo-blue. Having carefully dried a few woad leaves, I reduced them to powder and then treated them in a bottle with cold æther. I obtained a dark green solution, which after being filtered and evaporated, spontaneously left a green syrupy residue, from which water extracted, as in the preceding case, a substance which, by the action of boiling sulphuric acid, yielded an abundance of very pure indigo-blue.

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* If the acetate of lead is in the least degree contaminated with basic acetate, it will be found to precipitate some of the indigo-producing body.

By these and similar simple and easily-performed experiments, I was enabled to infer, with positive certainty, that the *Isatis tinctoria* contains a substance easily soluble in hot and cold water, alcohol and æther, which, by the action of strong mineral acids, yields indigo-blue; that the formation of the colouring matter from it can be effected without the intervention of oxygen or of alkalies; and that the latter, indeed, if allowed to act on it before the application of acid, entirely prevent the formation of colouring matter, and it now only remained to separate it from the other constituents of the plant and ascertain its properties and composition. But, though I arrived at the conclusion just stated without any great difficulty, I found that the isolation and preparation, in a state of purity, of the substance whose existence had been indicated by these experiments, constituted a problem of no easy solution. I soon discovered that this body is extremely liable to decomposition; so much so, as completely to justify the assertion of an author, who, in speaking of the difficulties of the manufacture of indigo, says that "nothing is more fugitive, and more liable to be acted on by destructive agencies, than the colouring principle of the *Indigoferæ*[*]." The continued action of water, even at a moderate, but especially at a high temperature, as well as that of alkalies, I found to induce a complete change in the body which I was endeavouring to isolate. The fact of its being completely precipitated from the watery extract of woad by means of acetate of lead and ammonia, and of the lead compound being readily decomposed by means of a current of carbonic acid gas, seemed at first to lead to an easy method of preparation. But on extracting a large quantity of the plant with cold water, adding sugar of lead to precipitate the albumen and green colouring matter, and then adding ammonia to the filtered liquid, I found that the precipitate produced by ammonia, though the alkali was not used in excess, contained the substance in an altered state. This change, the nature of which I shall treat of presently, is readily indicated by boiling some solution of the substance in water with sulphuric or muriatic acid, when it will be found that black flocks are deposited, which, when treated with boiling alcohol, impart to the latter no trace of blue, but a pure brown colour, a great part of the flocks remaining undissolved by the alcohol in the shape of a black powder. This change is readily effected in the watery solution, either by the addition of alkali, or by the temperature of the solution rising to any great extent above that of the atmosphere. When dissolved in alcohol or æther, on the other hand, the substance exhibits a much greater resistance to change than when dissolved in water. I therefore soon abandoned all idea of extracting the plant with the latter menstruum, and had recourse to alcohol and æther. I shall refrain from giving an account of the numerous experiments I made, which led to no successful issue, but shall briefly describe the three methods of preparation, which were attended with more or less advantageous results.

All three methods consist in extracting with alcohol or æther. But, before commencing, it is necessary that the plant should be thoroughly dried. The leaves alone being gathered, which should only be done when not moistened with rain or dew, are spread out in a thin layer in some warm, dry place, as, for instance, near a stove or in a drying-room. All yellow or decayed leaves should be rejected. When they are perfectly dry they are carefully examined. All those leaves which during the drying have become brown—a result which takes place when the heat has been too great,—are cast aside, and only those are retained which have a pale green colour. These must now be reduced to a coarse powder, an operation easily effected if the leaves be taken while still warm, as they are then quite brittle; whereas if allowed to remain exposed to the atmosphere they attract moisture again, and become flaccid and difficult to pulverize. The powder must be kept warm until it is used.

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[*] Perrottet, *Art de l'Indigotier*, p. 110.

I. The leaves thus reduced to powder, being put in a displacement apparatus, such as is used for the preparation of tannic acid, are extracted with cold alcohol. The percolating liquid is dark green. The extraction is continued until the liquid runs through of a light green colour. The alcoholic extract is now distilled in a retort, until a great part of the alcohol has passed over; it is then evaporated in a basin at a moderate temperature. During evaporation, a large quantity of chlorophyll and fatty matter separates as a green glutinous mass, which is to be separated by filtration. The filtered liquid, which is brown, is to be further evaporated until it leaves a thick brown syrup. This syrup, being poured into a flask, is redissolved in warm alcohol, which it does with some difficulty, on account of the change which the substance has undergone in consequence of the heat employed in distilling the alcohol. To the warm solution there is now added several times its volume of æther. The æther renders the solution milky, and precipitates a large quantity of matter, which collects at the bottom of the flask into a dark brown syrup. After this syrup has completely settled, the æther, which is also dark brown, is distilled. When the greatest part has distilled over, water is added to the syrupy residue. A quantity of brown fatty matter is precipitated, which is filtered. The filtered liquid is light yellow, and contains the substance almost in a state of purity. Should a further purification, however, be necessary, this is best effected by agitating the liquid in the cold with hydrated oxide of copper. A green solution is obtained, which is filtered, and a current of sulphuretted hydrogen gas being passed through it, it is again filtered from the precipitated sulphuret of copper, and evaporated either spontaneously or *in vacuo* over sulphuric acid. This method is attended with considerable loss, as the whole of the brown syrup insoluble in æther, the quantity of which is not trifling, is formed at the expense of the indigo-producing body.

II. The second method consists in simply extracting the pounded woad leaves with æther in a displacement apparatus, distilling the greatest part of the æther, evaporating the remaining green liquid at a moderate temperature, adding a little cold water to the syrupy residue, separating the insoluble chlorophyll and other matters by filtration and evaporating the yellow liquid as before, either spontaneously in the air or *in vacuo*. If purification should be necessary, it is effected, as in the preceding case, by means of oxide of copper. I endeavoured to modify this method by agitating the æthereal extract with cold water, but without any advantage resulting, as the substance was not entirely removed from the æther by means of water, a considerable quantity still remaining dissolved in the æther.

III. The pounded woad leaves are extracted in a displacement apparatus, as before, with cold alcohol. To the green alcoholic extract there is added an alcoholic solution of acetate of lead, which produces a pale green precipitate, the precipitation being then completed by the addition of a little ammonia. The precipitate, which is bulky, is placed on a filter and washed with cold alcohol, until the percolating liquid instead of being dark green is only light green, and the excess of acetate of lead and ammonia have been removed. It is then suspended in water, and a current of carbonic acid gas is passed through the liquid. The precipitate gradually becomes paler in colour and at last almost white, and loses considerably in bulk, while the liquid acquires a yellow colour. The latter being filtered, sulphuretted hydrogen is passed through it to precipitate a little oxide of lead contained in it, and being again filtered is evaporated as before, either in the air or *in vacuo* over sulphuric acid.

Of these three methods I prefer the last, as being more expeditious than the first and less costly than the second. One precaution must not be forgotten, that of never in the last instance attempting to evaporate the solution at a higher temperature than the ordinary one. If the attempt be made, the substance will undergo an entire change, as I have repeatedly discovered to my cost. This change consists in the substance taking up the elements of water. It takes place as well *in vacuo* as in the air. I have no reason to believe that the oxygen of the atmosphere has any influence in producing decomposition, at least at the usual temperature; and though I have generally in the last instance evaporated *in vacuo*, I think the evaporation may just as well be conducted in the air. Notwithstanding all precautions, however, it is difficult to avoid some portion of the substance becoming changed during evaporation.

The body, the preparation of which I have just described, I propose to call *Indican**. By evaporation of its watery solution, it is obtained in the form of a yellow, transparent, glutinous residue, which can only be rendered dry by spreading it out in thin layers, and leaving it for some time *in vacuo* over sulphuric acid. On attempting to dry it in the water-bath, it immediately undergoes a complete alteration. Its taste is slightly bitter and nauseous. Its solutions have always an acid reaction, but whether this reaction is peculiar to it in an absolutely pure state, I am unable to say. When heated in a tube it swells up, and gives fumes which condense to a brown oily sublimate, in which, after some time, a white crystalline substance is formed. When boiled with caustic alkali it evolves ammonia. Its compounds have a yellow colour. With caustic alkalies, baryta, and lime-water, the watery solution turns of a bright yellow. The alcoholic solution gives with sugar of lead a bright sulphur-yellow precipitate, which is increased by the addition of ammonia. The watery solution gives no precipitate with acetate of lead until ammonia is also added. Its most remarkable and interesting property is that of yielding indigo-blue when treated with strong acids. If sulphuric or muriatic acid be added to its watery solution, no change whatever is perceptible for some time; but on heating to near the boiling-point, the solution immediately becomes sky-blue. On boiling for a short time, the solution becomes opalescent. On continuing to boil, it acquires a purple colour, and then, provided the solution is tolerably concentrated, a copious deposit consisting of dark purplish-blue flocks is formed. The liquid filtered from these flocks retains a yellow colour, and contains a peculiar species of sugar, to which I shall return presently. The flocks themselves do not consist of indigo-blue only. After being collected on a filter and washed with water, they appear of a dark purple colour, the filter also acquiring during washing a purple tinge. If they be now treated with alcohol, a part dissolves even in the cold, but to a greater extent on heating, the alcohol acquiring a beautiful purple colour. If the flocks remaining undissolved be treated after filtration with an additional quantity of boiling alcohol, the latter acquires a more bluish tinge. Each succeeding portion of alcohol with which the flocks are boiled acquires more and more of a blue colour, until at last the colour is a pure indigo-blue. There remains in general a large quantity of indigo-blue undissolved, and the alcoholic liquids, on standing, deposit the colouring matter contained in them in the shape of bright blue flocks. The purple alcoholic solution leaves, on evaporation, a reddish-brown residue, which bears the greatest resemblance to, if it is not identical with, the indigo-red of Berzelius. Like the latter substance, it is quite insoluble in caustic alkalies and gives, when heated in a tube, purple fumes and a small quantity of a white crystalline sublimate. I propose to call this substance *Indirubine.* I have found that it is invariably formed along with indigo-blue whenever indican is decomposed by acids. Nevertheless, the quantity of indigo-blue produced is always relatively larger when the indican is pure than when the latter has begun to change. Of the two colouring matters, the indigo-blue is always the first to be formed. If sulphuric or muriatic acid be added to a solution of indican in the cold, and the mixture be allowed to stand in the cold for some time, a slight precipitate is gradually deposited which consists almost entirely of indigo-blue. It is only after boiling for some time that the formation of indirubine commences, when the colour of the liquid changes from blue to purple. The constant occurrence of a red colouring matter both in indigo and in the indigo-bearing plants along with indigo-blue—a fact which has been repeatedly observed,—has led chemists to suspect that there must be some necessary connexion between the two. From the experiments just described, it follows that, in the case of woad at least, they are both products of decomposition of one substance. If nitric acid be added to a watery solution of indican, a slight deposit of indigo-blue is formed, which of course disappears immediately on heating the liquid.

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* As the termination *an* has not yet been applied by chemists to designate any peculiar class of bodies, I propose to restrict it to the names of such substances of a complex constitution, like rubian, as are direct products of the vital energy of plants or animals, and which by their decomposition give rise to one or more series of organic compounds of a simpler constitution. In applying it, it will of course be necessary carefully to ascertain that the substance to be named is really a proximate constituent of some organism, and not itself the result of any process of decomposition either within or out of the organism—that it is an educt and not a product.

There is another very remarkable property of indican which I have to describe, a property, the knowledge of which will probably throw great light on the process of manufacturing indigo. If indican in the form of syrup, as obtained by evaporation of the watery solution, be heated for some time in the water-bath, or if its watery solution be boiled, or even moderately heated, it undergoes a complete metamorphosis. If the solution be now evaporated, it leaves a yellow syrupy residue, not to be distinguished in appearance from indican itself. It will be found, however, to have become insoluble in æther, and not easily soluble in alcohol. If æther be added to its solution in alcohol, the solution becomes milky, and deposits oily drops which collect at the bottom of the vessel to a yellow or brown syrup, the unchanged indican, if there be any present, remaining dissolved in the æther. In its other outward properties it has not undergone any marked change. In the next stage of the process the indican acquires a brown colour, and becomes quite insoluble in cold alcohol; but it still dissolves, though with difficulty, in boiling alcohol. By continuing the process the substance acquires a dark brown colour, and its watery solution now gives a copious precipitate with acetate of lead. As soon as the indican has entered even on the first stage of this process of change, it ceases to give the least trace of indigo-blue with acids. A short period occurs at the commencement of the process, during which the watery solution, when boiled with sulphuric acid, deposits purple flocks consisting of indirubine only. Afterwards, however, it yields other products, which are always the same at every subsequent stage of the process. If the watery solution be then boiled with the addition of sulphuric or muriatic acid, it becomes in the first instance of a darker colour, and after considerable boiling, deposits slowly a quantity of dark brown, almost black flocks. The liquid filtered from these flocks contains sugar, just as in the case of indican itself. The flocks themselves generally consist of two bodies. If they be collected on a filter, washed with water, and then treated with boiling alcohol, a part dissolves with a brown colour, and after filtration and evaporation, is left as a dark brown, shining, resinous substance. This substance melts in boiling water into brown coherent masses. It is completely dissolved and decomposed by boiling nitric acid. It dissolves in ammonia with a brown colour, and the solution gives brown precipitates with the chlorides of barium and calcium. It is completely precipitated from its alcoholic solution by sugar of lead, the precipitate being brown. I propose to call this body *Indiretine.* That portion of the dark brown flocks which is insoluble in boiling alcohol, dissolves in caustic alkalies with a dark brown colour, and is precipitated by acids in black flocks. As it bears some resemblance in its outward properties to humus, I shall call it *Indihumine.* Its similarity to the indigo-brown of Berzelius is so great, as almost to lead me to suspect that it is the same body. Whether this is the case or not can only be ascertained by analysis. Sometimes the brown substance formed by the action of acids on modified indican is entirely soluble in boiling alcohol, and contains no indihumine, but under what conditions this takes place I am unable to say. The change which indican undergoes during this process consists merely in its absorbing the elements of water. It proceeds *in vacuo* as well as in the air, provided the temperature be raised to a certain degree, which proves that oxygen plays no part in the process. It is apparently effected instantly when indican comes into contact with alkalies in its watery solution, though the alcoholic solution may be made alkaline, with ammonia at least, without any alteration taking place. It is certainly a most remarkable circumstance, that by merely taking up the elements of water, indican should be converted into a substance, which, when exposed to the action of acids, yields no longer indigo-blue and its allied red colouring matter, but bodies of an entirely distinct nature which have none of the properties of colouring matters; and it goes far to prove that indigo-blue does not pre-exist in indican even as a copula, but is merely contained in it potentially. That the view I have taken of this metamorphosis is the correct one, is proved by a singular observation which I once accidentally made. Having on one occasion obtained a dilute solution of indican, I tried a small quantity of it by boiling with sulphuric acid, and ascertained that it gave indigo-blue and indirubine; but on heating the whole quantity to the boiling-point with acid, I obtained instead of these two bodies, a substance dissolving in alcohol with a brown colour. It was evident that the indican, in consequence probably of the solution being very dilute, had taken up the elements of water before the acid could act on it, and that the latter then gave rise to the products of decomposition peculiar to the hydrate. In general, however, the nature of the flocks which are deposited on boiling a watery solution of indican with sulphuric or muriatic acid, affords a very good test of the purity of the indican. A solution is boiled in a test-tube partly filled, and the flocks which are formed collected on a filter, washed with water, and then treated with successive portions of boiling alcohol until no more will dissolve. If to the first portions of alcohol a purple colour be imparted, and a fine purplish-blue to the succeeding ones, then the indican may be considered pure. If the colour of the alcohol is brown, and if black flocks are left undissolved, the substance has undergone a complete change.

The sugar which is formed when acids act either on indican or its hydrates, is obtained in a state of purity in the following manner. If sulphuric acid be employed, which is preferable to muriatic, the acid liquid is filtered from the flocks consisting of indigo-blue and other products of decomposition, and the acid is removed by means of an excess of acetate of lead. If to the liquid filtered from the sulphate of lead an excess of ammonia is added, the sugar is precipitated in combination with oxide of lead. The precipitate, which is usually yellow and bulky, is, after washing, decomposed with sulphuretted hydrogen, and from the liquid filtered from the sulphuret of lead the sugar is again precipitated with acetate of lead and ammonia. The second precipitate, which is usually almost white, is again decomposed with sulphuretted hydrogen, and the filtered liquid is evaporated over sulphuric acid, when it leaves a colourless or only slightly yellow syrup, which has the following properties. It has a faintly sweet taste. When heated it swells up, emitting the usual smell of burning sugar, and then burns leaving much charcoal. With concentrated sulphuric acid it strikes a dark red colour, which on heating becomes black. Boiling nitric acid decomposes it with an evolution of nitrous fumes. When its watery solution is boiled with caustic soda, it becomes yellow and deposits a few brown flocks. With sulphate of copper and caustic soda it gives a blue solution, which on boiling becomes yellow, and then deposits suboxide of copper. If nitrate of silver be added to its watery solution while boiling, a little metallic silver is precipitated; and when ammonia is added, a further reduction takes place, accompanied by the formation of a metallic mirror. On adding chloride of gold to the watery solution and boiling, a quantity of metallic gold is deposited in bright scales and spangles; and on adding caustic alkali to the filtered solution, an additional quantity of gold is precipitated as a purple powder. The watery solution gives no precipitates either with neutral or basic acetate of lead,—only on adding ammonia does any precipitation take place. It is soluble in alcohol, but not in æther. In its outward properties, therefore, this sugar does not differ in any marked degree from other kinds of sugar obtained by the decomposition of complex organic bodies, such as that derived from rubian. In its composition, however, it differs essentially from other species of sugar, as I shall presently show.

I have hitherto been unable, I regret to say, to ascertain the exact composition of indican by direct experiment. On account of its deliquescent nature, and its so readily undergoing change when heated, it was impossible to subject it to analysis in a free state, and I was therefore obliged to have recourse to the lead compound. But when this compound is precipitated from a watery solution by means of acetate of lead and ammonia, it no longer contains unchanged indican, but one of the bodies formed by the combination of the latter with water. It is necessary, however, to use water in some stage of the preparation; for if the lead compound be precipitated from an alcoholic extract of woad with acetate of lead and ammonia, if the precipitate be decomposed, after washing with alcohol, by suspending it in alcohol and passing a stream of carbonic acid through the liquid, and the substance be again precipitated from the filtered liquid by means of acetate of lead and ammonia, the lead compound thus formed will be found to contain, besides indican, a quantity of fatty matter, from which the indican can only be separated by means of water, and its analysis leads, as I have ascertained, to no satisfactory results. It is therefore necessary to evaporate a watery solution of indican spontaneously, to dissolve the residue in alcohol, and precipitate with acetate of lead and ammonia, taking care to leave a slight excess of indican in solution. The following analysis was made with a specimen of the lead compound prepared in this manner, the indican itself having been obtained by the third method described above.

I. 1·4340 grm. of the compound, dried *in vacuo* and burnt with oxide of copper and chlorate of potash, gave 0·9600 grm. carbonic acid and 0·2860 water.

1·2170 grm. gave 0·1700 grm. chloride of platinum and ammonium.

0·3790 grm. gave 0·3080 grm. sulphate of lead.

These numbers lead to the following composition:—

	Eqs.		Calculated.	Found.
Carbon	52	312	18·67	18·25
Hydrogen . .	35	35	2·09	2·21
Nitrogen . . .	1	14	0·83	0·87
Oxygen	38	304	18·23	18·88
Oxide of lead .	9	1005·3	60·18	59·79
		1670·3	100·00	100·00

Notwithstanding the care, however, which I took in the preparation of this specimen, I found that it did not contain unchanged indican, as a little of it when tested with sulphuric acid gave no indigo-blue. It is, nevertheless, the purest specimen of the lead compound which I have analysed; that is to say, the substance combined with the oxide of lead contained the least amount of hydrogen and oxygen.

The next analysis which I shall give, places in a striking light the effect which alkalies exert on indican. I took some of the same solution of indican which I had employed for the preceding analysis, and which I found to give, when a little of it was boiled with acid, very pure indigo-blue; but instead of evaporating it, I added a large quantity of alcohol to it, and then precipitated with acetate of lead and ammonia. The precipitate no longer contained unchanged indican, and the substance combined with the oxide of lead differed in composition from that of the preceding analysis by containing the elements of two equivalents more of water. The third analysis was performed with a lead compound made in the same way as that of the first analysis, but from a specimen of indican prepared by the first method. The composition I found to be exactly the same as that of the compound of the second analysis.

II. 1·0960 grm. dried *in vacuo* and burnt with oxide of copper and chlorate of potash, gave 0·7060 grm. carbonic acid and 0·2020 water.

1·5600 grm. gave 0·1880 grm. chloride of platinum and ammonium.

0·8930 grm. gave 0·7520 grm. sulphate of lead.

III. 1·1000 grm. gave 0·7135 grm. carbonic acid and 0·2050 water.

1·8350 grm. gave 0·2260 grm. chloride of platinum and ammonium.

0·5710 grm. gave 0·4770 grm. sulphate of lead.

From these numbers I deduced the following composition:—

	Eqs.		Calculated.	II.	III.
Carbon . .	52	312	17·33	17·56	17·69
Hydrogen .	37	37	2·05	2·04	2·07
Nitrogen .	1	14	0·77	0·75	0·77
Oxygen . .	40	320	17·80	17·69	18·01
Oxide of lead	10	1117	62·05	61·96	61·46
		1800	100·00	100·00	100·00

After deducting the oxide of lead, the amount of which is unusually large for a compound of definite constitution, the organic substance combined with it has the following composition :—

	Eqs.		Calculated.	II.	III.
Carbon . .	52	312	45·68	46·16	45·90
Hydrogen .	37	37	5·41	5·36	5·37
Nitrogen .	1	14	2·04	1·97	1·99
Oxygen . .	40	320	46·87	46·51	46·74
		683	100·00	100·00	100·00

An analysis which I made of a lead compound, prepared directly from a watery extract of woad by precipitating with acetate of lead, filtering, and then adding ammonia to the filtered liquid, gave a composition agreeing pretty well with the formula $C^{52} H^{43} NO^{46} + 13PbO$.

It appears, therefore, that the organic substances contained in these lead compounds differ from one another merely by the elements of water; and it may hence be inferred, with a great degree of probability, that indican itself in a state of purity differs in composition from them merely by containing the elements of several equivalents of water less. Assuming its composition to be represented by the formula $C^{52} H^{33} NO^{36}$, it must be shown how this formula explains the formation of indigo-blue. Before this can be done, however, it is necessary to know the composition of the sugar which is always formed simultaneously with the indigo-blue. The lead compound of the sugar, prepared as above described, and dried *in vacuo*, was analysed with the following results :—

I. 1·0580 grm., burnt with chromate of lead, gave 0·4550 grm. carbonic acid and 0·1670 water.

0·7620 grm. gave 0·7490 grm. sulphate of lead.

II. 1·0960 grm., after being dried *in vacuo* for some time longer, gave 0·4740 grm. carbonic acid and 0·1640 water.

0·6420 grm. gave 0·6370 grm. sulphate of lead.

These numbers lead to the following composition :—

	Eqs.		Calculated.	I.	II.
Carbon . .	12	72	11·69	11·72	11·79
Hydrogen .	9	9	1·46	1·75	1·66
Oxygen . .	11	88	14·30	14·21	13·55
Oxide of lead	4	446·8	72·55	72·32	73·00
		615·8	100·00	100·00	100·00

If the formula of the lead compound is $C^{12}H^{9}O^{11}+4PbO$, it is probable that the sugar in an uncombined state has the formula $C^{12}H^{10}O^{12}$. It differs, therefore, from other kinds of sugar by containing less hydrogen than is necessary to form water with the oxygen. Its formula is, however, perfectly in accordance with the one which I have adopted for indican, viz. $C^{52}H^{33}NO^{36}$. If we suppose the latter to take up two equivalents of water, it will then simply split up into 1 equivalent of indigo-blue and 3 equivalents of sugar, as will be seen by the following equation:—

$$\left.\begin{array}{ll}\text{1 eq. indican} & C^{52}H^{33}NO^{36}\\ \text{2 eqs. water} & \quad H^{2}\quad O^{2}\end{array}\right\}=\left\{\begin{array}{ll}C^{16}H^{5}NO^{2} & \text{1 eq. indigo-blue.}\\ C^{36}H^{30}\;O^{36} & \text{3 eqs. sugar.}\end{array}\right.$$

$$C^{52}H^{35}NO^{38} \qquad\qquad C^{52}H^{35}NO^{38}$$

I assume that the composition of the blue colouring matter derived from indican is the same as that of indigo-blue, since an examination of the properties of the former leaves little doubt concerning their identity. I regret not having as yet been able to ascertain by analysis whether this is the case or not, as I have devoted the whole of the material at my disposal to an investigation of the properties and composition of indican itself.

I also assume that indican, in undergoing decomposition with acids, splits up immediately into 1 equivalent of indigo-blue and 3 equivalents of sugar. It is, however, possible that these three equivalents of sugar may not be eliminated all at once; and from one analysis which I made, I should conclude that they separated successively. Having extracted some dried woad leaves with cold æther, I poured the ætherial extract into a large bottle and agitated it with about half its volume of cold water. The æther was poured off, and the watery liquid was employed again for agitation with several successive portions of ætherial extract of woad. It acquired at last a dark yellow colour. The æther contained in it was removed by spontaneous evaporation, and it was then evaporated under a bell over sulphuric acid. At first it yielded indigo-blue when boiled with sulphuric acid; but the evaporation having been conducted in too warm a place, the indican contained in it became changed, and it ceased to give blue flocks with acids. After the evaporation was completed there was left a brown syrupy residue, which was redissolved in alcohol.

Acetate of lead produced in the alcoholic solution a cream-coloured precipitate, which was separated by filtration; and on adding a small quantity of ammonia to the filtered liquid, a cream-coloured precipitate again fell, which was collected on a filter, washed with alcohol, and dried *in vacuo*. On analysing it I obtained the following results:—

0·3705 grm., burnt with oxide of copper and chlorate of potash, gave 0·2670 grm. carbonic acid and 0·0730 water.

0·3800 grm. gave 0·0680 grm. chloride of platinum and ammonium.

0·2680 grm. gave 0·2160 grm. sulphate of lead.

In 100 parts it contained, therefore,—

Carbon	19·65
Hydrogen	2·18
Nitrogen	1·12
Oxygen	17·75
Oxide of lead	59·30
	100·00

After deducting the oxide of lead, the amount of which stands in no simple relation to that of the other constituents, the organic substance combined with the oxide of lead will be found to have a composition expressed by the formula $C^{40} H^{26} NO^{27}$, as will be seen by the following calculation:—

	Eqs.		Calculated.	Found.
Carbon	40	240	48·38	48·28
Hydrogen	26	26	5·24	5·35
Nitrogen	1	14	2·82	2·75
Oxygen	27	216	43·56	43·62
		496	100·00	100·00

This body is therefore formed from indican by the latter taking up 3 equivalents of water and then losing 1 equivalent of sugar, as will be evident from the following equation:—

$$\left.\begin{array}{ll}\text{1 eq. indican} & C^{52} H^{33} NO^{36} \\ \text{3 eqs. water} & \underline{\quad H^{3} \quad O^{3}} \\ & C^{52} H^{36} NO^{39}\end{array}\right\} = \left\{\begin{array}{ll}C^{40} H^{26} NO^{27} & \\ \underline{C^{12} H^{10} \quad O^{12}} & \text{1 eq. sugar,} \\ C^{52} H^{36} NO^{39} & \end{array}\right.$$

It is perfectly conceivable that an additional equivalent of sugar may separate from the body $C^{40} H^{26} NO^{27}$ before the decomposition is finally completed; that is to say, that there exists another intermediate body containing 28 equivalents of carbon.

Of the red colouring matter, which I have called indirubine, I have not yet obtained a sufficient quantity for analysis.

I have several times submitted indihumine, obtained on different occasions, to analysis, but without being able to arrive at any positive conclusion regarding its composition. The difficulty of doing so arises from the circumstance of this substance forming compounds with alkalies and other bases which are not completely decomposed by acids. The analyses which I have made agree best with the formula $C^{16}H^{8}NO^{5}$. If this be the correct formula, it differs in composition from indigo-blue by the elements of three equivalents of water, just as the body or bodies from which it is formed differ from indican by containing the elements of several equivalents more water.

The analyses of indiretine led to more definite results. The substance was prepared from indican, which had undergone the alteration of which I have several times spoken, by treating it with boiling sulphuric acid, collecting the dark brown deposit which was formed on a filter, washing out the acid, treating with boiling alcohol, filtering from the indihumine which remained undissolved, and evaporating the alcoholic solution to dryness.

I. 0·4420 grm., dried in the water-bath and burnt with oxide of copper and chlorate of potash, gave 0·9930 grm. carbonic acid and 0·2100 water.

0·5470 grm. gave 0·3415 grm. chloride of platinum and ammonium.

II. 0·5370 grm. of another preparation gave 1·2130 grm. carbonic acid and 0·2675 water.

0·8780 grm. gave 0·4320 grm. chloride of platinum and ammonium.

III. 0·2760 grm. of a third preparation gave 0·6190 grm. carbonic acid and 0·1420 water.

These numbers correspond with the following composition :—

		Eqs.	Calculated.	I.	II.	III.
Carbon .	36	216	61·02	61·27	61·60	61·16
Hydrogen	20	20	5·64	5·27	5·53	5·71
Nitrogen	1	14	3·95	3·92	3·09	
Oxygen .	13	104	29·39	29·54	29·78	
		354	100·00	100·00	100·00	

Assuming $C^{36}H^{20}NO^{13}$ to be the correct formula for indiretine, then the formation of this substance from indican, or rather from its hydrates, can only be explained by supposing that carbonic acid is evolved during the process. If we take, for instance, the substance represented by the formula $C^{52}H^{37}NO^{40}$, which gives indiretine when treated with acids, we may suppose it to split up into 1 equivalent of indiretine, 1 equivalent of sugar, 4 equivalents of carbonic acid, and 7 equivalents of water, as will be seen by the following equation :—

$$C^{52}H^{37}NO^{40} = \left\{\begin{array}{llll} C^{36} & H^{20}N & O^{13} & \text{1 eq. indiretine.} \\ C^{12} & H^{10} & O^{12} & \text{1 eq. sugar.} \\ C^{4} & & O^{8} & \text{4 eqs. carbonic acid.} \\ & H^{7} & O^{7} & \text{7 eqs. water.} \end{array}\right.$$

$$\overline{C^{52}H^{37}NO^{40}}$$

I have certainly not observed the disengagement of carbonic acid during the formation of indiretine, but the evolution of gas might easily elude observation during the long-continued boiling which is necessary for the production of this substance.

The want of material has for the present interrupted the further prosecution of my experiments. As soon as I shall have obtained an additional quantity of the fresh plant, it is my intention to continue them. The action of acids on indican requires still further examination, and the effects produced by ferments and other agents remain to be investigated. I also propose to examine other indigo-bearing plants, in order to ascertain whether they contain indican or not.

The results of the present investigation may be summed up in the following propositions:—

1. The *Isatis tinctoria* does not contain indigo-blue ready formed, either in the blue or colourless state.
2. The formation of the blue colouring matter in watery extracts of the plant is neither caused nor promoted by the action of oxygen or of alkalies.
3. Indigo-blue cannot be said to exist in any state of combination in the juices of the plant: it is merely contained in them potentially.

XI. *On the alleged Fluorescence of a solution of Platino-cyanide of Potassium. By* Professor G. G. Stokes, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I was surprised, some time since, by reading in Poggendorff's *Annalen* the statement of Dr. Böttger, which has since been republished in the Philosophical Magazine, that a solution of platino-cyanide of potassium is highly fluorescent. My own observations had led me to the conclusion, that "it is only in the solid state that the platino-cyanides are sensitive,—their solutions look like mere water*." In consequence of Dr. Böttger's statement, I have just examined again a solution of platino-cyanide of potassium, with the advantage of bright sunshine and a complete quartz train; and so far from its being eminently fluorescent, I find it to be not fluorescent at all. It is useless to conjecture the cause of the discrepancy, which will probably be cleared up in time; my only object in writing is to prevent the spread of an erroneous statement.

I am, Gentlemen,
Your faithful Servant,
G. G. Stokes.

Pembroke College, Cambridge,
July 5, 1855.

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* Phil. Trans. for 1853, p. 396.

XII. *On the power of Icebergs to make rectilinear, uniformly-directed Grooves across a Submarine Undulatory Surface. By* C. Darwin, *Esq., Vice-Pres. R.S., F.G.S.*[*]

HAVING been induced to believe, with many geologists, that certain continuously scored and polished surfaces of rock were due to icebergs, and not to glaciers, I have nevertheless always felt much difficulty in understanding how long, rectilinear scratches, running in one given direction across an *undulatory* surface, could have been thus formed. Others have felt this same difficulty, and it has been advanced as an insuperable difficulty by the opponents of iceberg action. The following considerations, though possessing little or no novelty, have in my own case removed the difficulty. But first, to give one instance of such scratches, I may quote a passage from Agassiz[†], who, in describing the state of the surface near Lake Superior, says, "nothing is more striking in this respect than the valleys or depressions of the soil running E. and W., where we see the scratches crossing such undulations at right angles, descending along the southern gentle slope of a hill, traversing the flat bottom below, and rising up the next hill south in unbroken continuity." He proceeds to state that the scratches run up even steep northern slopes, though the southern faces of the hills are generally rugged. A glacier driven straight forwards over its unequal bed would perfectly account for these facts; but not so, at first appearance, floating ice, whether that of coast-ice or of icebergs. For such masses being borne along on the level ocean, would, when driven on shore or against a submarine hill, be deflected, as it might be thought, from their course, and mark the rocks horizontally or nearly so,--some allowance being made for the rise and fall of the tide. And although during either the submergence or emergence of the land, the whole surface of a mountain might become thus marked, yet the successive scores at each level would all be nearly horizontal. No doubt short inclined grooves might be formed by masses of ice being driven by gales up the beach; but as sea-shores run in every possible direction, it is obvious that such grooves could follow no uniform course, nor could they be of any considerable length; hence grooves thus made would not be comparable with those now under discussion.

The plasticity of glaciers, as shown by the manner in which they immediately expand after passing through gorges, and in which they mould themselves to every sinuosity and prominence in their beds, is now, thanks to the labours of a few eminent men, familiarly known to every geologist. It is asserted by some authors that glacier ice is most plastic when most charged with water, and the lower part of an iceberg must be water-logged. Again, a glacier, for instance of 1000 feet in thickness, must press on its bed with the whole immense weight of the superincumbent ice; but in an iceberg 1000 feet thick, as the whole floats, there will of course be no pressure on a surface exactly level with its bottom, and if driven over a prominence standing up at the bottom of the sea some 50 or 100 feet above the basal line of the berg, only the weight of as much ice as is forced up above the natural level of the floating mass, will press on the prominence. It may therefore, I think, be concluded that an iceberg could be driven over great inequalities of surface easier than could a glacier. That the weight of a comparatively thin sheet of ice is sufficient to groove rocks, we may infer from the case described by Sir C. Lyell of the scores made by the packed *shore-ice* on the coast of the United States. That icebergs do not break up when grounded, as *à priori* might have seemed probable, is obvious from the simple fact of their having been often observed in this condition in open turbulent seas. Let anyone who has witnessed the crash of even so small an object as a ship, when run into by another having only a barely perceptible movement, reflect on the terrific momentum of an iceberg, some mile or two square, and from 1000 to 2000 feet in thickness, when, borne onwards by a current of only half a mile per hour, it runs on a submarine bank: may we not feel almost certain, that, moulding itself like a glacier (of which it originally was a portion), but owing to its water-logged state and little downward pressure moulding itself more perfectly than a glacier, it would slide straight onwards over considerable inequalities, scratching and grooving the undulatory surface in long, straight lines? In short, if in our mind's eye we look at an iceberg, not as a rigid body (as has hitherto been always my case) which would be deflected or broken up when driven against any submarine obstacle, but as a huge semi-viscid, or at least flexible mass floating on the water, I believe much of the difficulty will be removed which some have experienced in understanding how rectilinear grooves could be formed continuously running, as if regardless of the outline of the surface, up and down moderately steep inequalities, now existing as hills on the land. It should be borne in mind that the course of deeply-floating icebergs is determined by the currents of the sea, and not, as remarked by Scoresby, by the shifting winds; and as the currents of the sea are well known to be definite in their course, so will be the grooves formed by current-borne icebergs. It is indeed difficult to imagine any difference between the effect on the underlying surface, of a glacier propelled by its gravity, and that of a mountainous island of ice driven onwards by an oceanic current, except that the iceberg would perhaps have the power, from the causes above specified, of even more closely moulding itself, and, as it were, of *flowing* straight over submarine obstacles, than has a glacier on the dry land.

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[*] Communicated by the Author.

[†] Lake Superior, its Physical Character, &c., by L. Agassiz, p. 406.

One other point is perhaps worth considering. I have elsewhere* endeavoured to show that the action of coast-ice and of icebergs must be considerably different in transporting boulders; the worn stones on the beach being imbedded in coast-ice, and fragments of rock which had originally fallen on the parent-glacier being carried by icebergs as on rafts. But when we reflect that icebergs are driven onwards year after year in certain definite directions by the currents of the sea,—that they float so deeply as to have been seen aground at the depth of 1500 feet,—that when stranded they must (as I conceive) mould themselves to the inequalities of the bottom and slide some distance over it,—it can hardly be doubted that they also must, like glaciers on the land, push in certain determinate directions moraines before them. Although a fragment of rock or an irregularly formed moraine may by any one iceberg be propelled for only a very short distance, yet in the course of years the transportal can hardly fail to become far extended, the boulders being rolled over large inequalities of surface, and even up heights by the action of successively smaller bergs: an abyss, however, deeper than the deepest-floating iceberg would, of course, absolutely stop this rolling or pushing action. Finally, in the case of every mass of erratic boulders, we have now to determine, and I believe hereafter it will be so determined, whether they were transported by glaciers or by floating ice, and in this latter case whether imbedded in coast-ice, strewed on the surface of icebergs, or pushed onwards as a subaqueous moraine.

XIII. *On Electric Conduction.*

By Professor Faraday, *D.C.L., F.R.S.*†

SINCE the time when the law of definite electrolytic action was first laid down (Exp. Res. 783–966), it has become a question whether those bodies which form the class of electrolytes conduct only whilst they are undergoing their proper change under the action of the electric current; or whether they can conduct also as metals, dry wood, spermaceti, &c., do in different degrees, *i. e.* without the accompaniment of any chemical change within them. The first kind of conduction is distinguished as the *electrolytic,* the transference of the electric force appearing to be essentially associated with the chemical changes which occur; the second kind may be called *conduction proper,* and there the act of conduction leaves the body ultimately as it found it. Electrolytic conduction is closely associated with the liquid state, and with the compound nature and chemical proportions of the bodies in which it occurs, and it is considered as varying in degree (*i. e.* in facility) with the affinities of the constituents belonging to these bodies; there are, however, other circumstances which evidently, and indeed very strongly, affect the readiness of transfer, such as temperature, the presence of extraneous matters, &c. Conduction proper differs as to facility by degrees so far apart, that that quantity of electricity which could pass through a hundred miles of one substance, as copper, in an inappreciably small portion of time, would require ages to be transmitted through the like length of another substance, as shell-lac; and yet the copper with its similars offers resistance to conduction, and the lac and its congeners conduct.

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* Transactions of the Geological Society, vol. vi. (2nd series) 1841. p. 430.

† From the Proceedings of the Royal Institution of Great Britain, for Friday, May 25, 1855.

The progress and necessities of science have rendered it important within the last three or four years, and especially at the present moment, that the question "whether an electrolyte has any degree of conduction proper" should be closely considered, and the experiments which are fitted to probe the question have been carried to a very high degree of refinement. Buff*, by employing the electric machine and Wollaston terminals, *i. e.* platinum wires sealed into glass tubes, and having the ends only exposed, has decomposed water by a quantity of electricity so small that it required four hours to collect gas enough to fill a little cylinder only one-tenth of an inch in diameter, and one-fifth of an inch in length; yet the decomposition was electrolytic and polar, and therefore the conduction was electrolytic also. When one pole only was in the water and the other in the air over it, still the decomposition, and therefore the conduction, was electrolytic; for one element appeared at the pole in the water, and the other in the air or gas over the water at the corresponding pole. Buff concludes that electrolytes have no conduction proper. Many other philosophers have supported, with more or less conviction, the same view, and believe that electrolytic conduction extends to, and includes cases which formerly were supposed to depend upon conduction proper. Soret advances certain experimental results†, but reserves his opinion from being absolute. Von Breda and Logeman adopt the more general view unreservedly[*]. De la Rive, I think, admits that a very little may perhaps pass by conduction proper, but that electrolytic conduction is the function of electrolytes[†]. Matteucci has at one time admitted a little conduction proper, but at present, I believe, denies that any degree exists. On the other hand, Despretz[‡], Leon Foucault[§], Masson[||], and myself, have always admitted the possibility that electrolytes possess a certain amount of conduction proper—small indeed, but not so small as to prevent its being evident in certain forms of experiments; and beautiful and close as the electrolytic proofs have been carried, they are not by us considered as sufficient to show that the function of conduction proper is altogether absent from electrolytes.

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* MS. letter.

† *Annales de Chimie*, xlii. 257.

[Some account was then given of the experiments and arguments on both sides; and of the striking electrolytic fact, that if a current of electricity, however small, is sent through a circuit containing a couple of platina plates in dilute sulphuric acid, the plates are found thereby electrically polarized.]

The inquiry as regards electrolytes takes on three forms. They may possess a degree of conduction proper at all times; or they may be absolutely destitute of conduction proper; or they may possess conduction proper up to a certain condition, governed either by requisite intensity for electrolyzation, or by other circumstances, but which, when that condition is acquired, changes into electrolytic conduction; and these three forms may be further varied by considerations dependent upon the physical state of the electrolyte, as whether it be solid or liquid, hot or cold, and whether it be pure or contain other substances mingled with it.

From the time when the question was raised by myself, twenty years ago, to the present day, I have found it necessary to suspend my conclusions; for close as the facts have in certain cases been urged by those who believe they have always obtained decomposition results when an electrolyte has performed the part of a conductor, and freely as I could have admitted the facts and the conclusions if there had been no opposing considerations, still, because there are such considerations, I am obliged to reserve my judgement. In the first place, all bodies not electrolytic, even up to gases (Becquerel), are admitted to possess conduction proper; *à priori*, therefore, we have reason to expect that electrolytes will possess it also. If from amongst different bodies we retain for consideration the class of electrolytes only, then, though the amount of electricity of a given intensity which these can transmit electrolytically when they are *fluid* is often almost infinitely greater than that which they can convey onwards by conduction proper when they are *solid,* still the conduction in the latter cases is very evident. A piece of perfectly dry solid nitre, and of many other electrolytes, discharges a gold-leaf electrometer very freely, and I believe by the power of conduction proper; and that being the case, I do not see that the assumption of the very highest condition of electrolytic conduction when the nitre is rendered fluid is any argument for the absolute disappearance of the conduction proper which belonged to the body in the solid state, though it may override the latter for the time, and make it insensible. These considerations are, however, such as arise rather from the absence of the final and strict proof on the opposite side, than from anything very positive in their own character; but it has occurred to me that the phænomena of static electricity will furnish us with many reasons of a positive nature in favour of the possession by liquid electrolytes of the power of conduction proper. Some of these I will endeavour briefly to state, illustrating the subject by a reference to water, which in its pure state has but a low degree of electrolytic conduction.

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[*] Phil. Mag. viii. 465. [†] *Bibl. de Genève,* xxvi. 134, 144; xxvii. 177.

[‡] *Comptes Rendus,* xxxviii. 897.

[§] *Comptes Rendus,* xxxvii. 580; or *Bibl. de Genève,* xxiv. 263; xxv. 180; xxvi. 126.

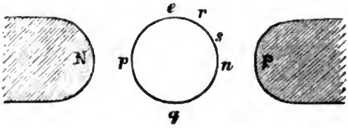
[||] Prize Essay, Haarlem Trans. xi. 78.

The ordinary phænomena of static charge and induction are well known. If an excited glass rod or other body be held near a light gilt sphere, suspended from the hand by a metal thread, the inductive action disturbs the disposition of the electricity in the sphere, and the latter is strongly attracted; if in place of the sphere a soap bubble be employed, the same results occur. If a dish filled with pure distilled water be connected with the earth by a piece of moist bibulous paper, and a ball of excited shell-lac be suspended 2 or 3 inches above the middle of the water, and if a plate of dry insulating gutta-percha, about 8 inches long and 2 inches wide, have its end interposed between the water and the shell-lac, it may then be withdrawn and examined, and will be found without charge, even though it may have touched the shell-lac; but if the end once touch the water under the lac (and it may be dipped in) so as to bring away a film of it, charged with the electricity the water has acquired by the induction, it will be found to possess, as might be expected, a state contrary to that of the inductric shell-lac.

In order to exclude any conducting body but water from what may be considered as a reference experiment, two calico globular bags with close seams were prepared, and being wetted thoroughly with distilled water, were then filled with air by means of a fine blowpipe point; they were then attached to two suspending bands of gutta-percha, by which they were well insulated, and being about 3 inches diameter, they formed, when placed in contact, a double system 6 inches in length. A metallic ball, about 4 inches in diameter, was connected with the electric machine to form an inductric body, an uninsulated brass plate was placed about 9 inches off to form an inducteous body; between these the associated water balls could be placed so as to take part in the induction, and when the electric charge was so low that the moist atmosphere caused no transmission of electricity, the balls could be introduced into position and brought away without having received any permanent charge. Under these circumstances, if the associated balls were brought into the place of induction, were then separated, withdrawn and examined, they were found, the one charged positively and the other negatively, by electricity derived from themselves, and without conductive or convective communication with any other substance than their own water.

It is well known indeed that by the use of water we may replace metal in all electro-static arrangements, and so form Leyden jars, condensers, and other induction apparatus, which are perfect in principle though with imperfect action. The principles are the same whether water or metals be used for conductors, and the function of conduction is essential to all the results; therefore conduction cannot be denied to the fluid water, which in all such cases is acting as the only conductor. In nature, indeed, the phænomena of induction, rising up to their most intense degree in the thunder-storm, are almost, if not altogether, dependent upon the water which in the earth, or the clouds, or the rain, is then acting by its conducting power; and if this conducting power be of the nature of *conduction proper*, it is probable that that function is as large and as important as any exercise of the electrolytic conduction of water in other natural phænomena.

But it may be said that all these cases, when accompanied by conduction, involve a corresponding and proportionate electrolytic effect, and are therefore cases of electrolytic conduction; and it is the following out of such a thought that makes me think the results prove a conduction proper to exist in the water. For suppose a water bubble to be placed midway between a positive and a negative surface, as in the figure, then the parts at



and about *p* will become charged positive, and those at and about *n* negative, solely by the disturbance of the electric force originally in the bubble, *i. e.* without any direct transmission of the electric force from N or P; the parts at *e* or *q* will have no electric charge, and from those parts to *p* and *n* the charge will rise gradually to a maximum. The electricity which appears at *p*, *n*, and elsewhere, will have been *conducted* to these parts from other parts of the bubble; and if the bubble be replaced by two hemispheres of metal, slightly separated at the equatorial parts *eq*, the electricity (before *conducted* in the continuous bubble) will then be seen to pass as a bright spark. Now the particles at any part of the water bubble may be considered under two points of view, either as having had a current passed through them, or as having received a charge; in either view the idea of conduction proper supplies sufficient and satisfactory reasons for the results; but the idea of electrolytic conduction seems to me at present beset with difficulties. For consider the particles about the equator *eq*,—they acquire no final charge, and they *have conducted*, as the action of the two half spheres above referred to show; and they are not in a state of mutual tension, as is fully proved by very simple experiments with the half hemispheres. Therefore oxygen must have passed from *e* towards *n*, hydrogen from *e* towards *p*, *i. e.* towards and to the parts to which the electricity has been conducted, for without such transmission of the anions and cations there would be no transmission of the electricity, and so no electrolytic conduction. But then the questions arise,—Where do these elements appear? is the water at *n* oxygenated, and that about *p* hydrogenated? and may the elements be at last dispersed into the air at these two points, as in the case of decompositions against air poles? (Exp. Res. 455, 461, &c.) In regard to such questions other considerations occur respecting the particles about *p* and *n*, and the condition of charge they have acquired. These have received the electricity which has passed as a current through the equatorial parts, but they have had no current or no proportional current through themselves—the conduction has extended *to* them but not *through* them; no electricity has passed for instance through the particle at *n* or at *p*, yet more electricity has gone by some kind of conduction to them than to any other of the particles in the sphere. It is not consistent with our understanding of electrolytic conduction to suppose that these particles have been charged by such conduction; for in the exercise of that function it is just as essential that the electricity should *leave* the decomposing particle on the one side, as that it should *go to it* on the other: the mere escape of oxygen and hydrogen into the air is not enough to account for the result, for such escape may be freely permitted in the case of electrodes plunged into water; and yet if the electricity cannot pass from the decomposing particles into the electrodes, and so away by the wires, in a condition enabling it to perform its full equivalent of electric work anywhere else in the circuit, there is no decomposition at the final particles of the electrolyte, nor any electrolytic conduction in its mass. Even in the air cases above referred to, there is a complete transmission of the electricity across the extreme particles concerned in the electrolysis.

If the above reasoning involve no error, but be considered sufficient to show that the particles at *p* and *n* are not electrolyzed, then it is also sufficient to prove that none of the particles between *p* and *n* have been electrolyzed; for though one at *e* or *q* may have had a current of electricity passed through it, it could not give up its elements unless the neighbouring particles were prepared to take them in a fully equivalent degree. To stop the electrolysis at *n* and *p*, or at those parts of the surface where the moving electricity stops, is to stop it at all the intervening parts according to our present views of electrolysis, and to stop the electrolysis is to shut out electrolytic conduction; and nothing at present remains but *conduction proper*, to account for the very manifest effects of conduction which occur in the case.

It may be imagined that a certain polarized state of tension occurs in these cases of static induction, which is intermediate between it and electrolytic conduction (Exp. Res. 1164); or that a certain preparatory and as it were incomplete condition may be assumed, distinguishing the case of static conduction with globes of water, which I have taken as the ground of consideration from the same case when presented by globes of metal. Our further and future knowledge may show some such state; but in respect of our present distinctive views of conduction proper and electrolytic conduction, it may be remarked that such discovery is just as likely to coincide with the former as with the latter view, though it most probably would alter and correct both.

Falling back upon the consideration of the particles between *e* and *n*, we find, that whether we consider them as respects the current which has passed through them, or the charge which they have taken, they form a continuous series; the particle at *e* has had most current, that at *n* none, that at *r* a moderate current; and there are particles which must have transmitted every intermediate degree. So with regard to charge; it is highest at *n*, nothing at *e*, and every intermediate degree occurs between the two. Then with respect to these superficial particles, they hold all the charge that exists, and therefore all the electricity which has been conducted is in them; consequently all the electrolytic results must be there; and that would be the case, even though for the shell we were to substitute a sphere of water. For, if those particles which have had more current through them than others be supposed to have more of the electrolytic results about them than the others, then that electricity which is found associated chiefly, if not altogether, with these others, could have reached them only by conduction proper, which for the moment is assumed to be non-existent. So, to favour the electrolytic argument, we will consider the conduction as ending at, and the electrolytic results as summed up in, these superficial particles, passing for the present the former objection that though the electricity has reached, it has not gone through, these particles. Taking, therefore, a particle at *r*, and considering its electrolytic condition as proportionate to the electricity which has arrived at that particle, and given it charge, we may then assume—for we have the power of diminishing the inductive action in any degree,—that the electricity, the conduction of which has ceased upon the particle that was there, has been just enough to decompose it, and has left what was the under but is now the surface particle, charged. In that case, some other particle, in a higher state of charge, and nearer to *n*, as at *s*, will have had enough electricity conducted towards its place to decompose two particles of water;—but it is manifest that this cannot be the next particle to that at *r*, but that a great number of other particles in intermediate states of charge must exist between *r* and *s*. Now the question is, how can these particles become intermediately charged by virtue of electrolytic conduction only? Electrolytic action is definite, and the very theory of electrolytic conduction assumes that the particles of oxygen and hydrogen as they travel convey not a variable but a perfectly definite amount of power onward in its course, which amount they cannot divide, but must take at once from a like particle, and give at once to another like particle. How then can any number of particles, or any action of such particles, carry a fraction of the force associated with each particle? It is no doubt true, that if two charged particles can throw their power either on to one, or to three or more other particles, then all the difficulty disappears. Conduction proper can do this: but, as we cannot conceive of a particle half decomposed, so I cannot see how this can be performed by electrolytic conduction, *i. e.* how the particle between *r* and *s* can be excited to the intermediate and indefinite degree, conduction without electrolysis being denied both to it and the particles around it.

If the particles between *e* and *n* be supposed to conduct electrolytically by the current which passes *through* them (dismissing for a time, amongst other serious objections, that already given, that the products would not be found at the places to which the electricity has been conveyed), still the present argument would have like force. At *r* enough electricity may have passed through to decompose two particles of water, at *s* only enough to decompose one,—how is a particle between *r* and *s* to change elements with the particles either towards *r* or towards *s*, if electrolytic change only is to be admitted? or how, as before inquired, can two particles throw their power on to, or receive their power from one? Many other considerations spring out of the thought of a water bubble, under static induction; but these just expressed, with those that relate to the *seat* of electrolytic action, whether at the place of current or of charge, create a sum of difficulty fully sufficient, without any others, to make me suspend for the present any conclusions on the matter in question.

The conduction power of water may be considered under another point of view; namely, that which has relation to the absolute charge that can be given to the fluid. A point from the electrical machine can charge neighbouring particles of air, and they issue off in streams. It can do the same to particles of camphine or oil of turpentine;—it can do the same to the particles of water; and if two fine metallic wires connected with Ruhmkorff's apparatus, be immersed in distilled water, about half an inch apart, the motes usually present will soon show how the water receives the charge, and how the charged water passes off in streams, which discharge to each other in the mass. Now such charge is not connected with electrolysis; the condition of electrolyzation is, that the electricity pass through the water and do not stop short in it. The mere charge of the water gives us no idea where any constituents set loose by electrolysis can be evolved, and yet conduction is largely concerned in the act of charging. A shower of rain falls across a space in the atmosphere subject to electric action, and each drop becomes charged; spray may be thrown forth from an electrified fountain very highly charged;—conduction has been eminently active in both cases, but I find it very difficult to conceive how that conduction can be electrolytic in its character.

When drops of water, oppositely electrified, are made to approach each other, they act by convection, *i. e.* as carriers of electricity; when they meet they discharge to each other, and the function of conduction is for the time set up. When the water bubble, described p. 102, is taken out of the sphere of induction, the opposite electricities about *p* and *n* neutralize each other, being conducted through the particles of the water. Are we to suppose in these cases that the conduction is electrolytic? if so, where are the constituents separated, and where are they to appear? It must be a strong conviction that would deny conduction proper to electrolytes in these cases; and if not denied here, what reason is there ever to deny it absolutely?

The result of all the thought I can give to the subject is a suspended judgement. I cannot say that I think conduction proper is as yet disproved in electrolytes; and yet I cannot say that I know of any case in which a current, however weak, being passed by platinum electrodes across acidulated water, does not bring them into a polarized condition. It may be that when metallic surfaces are present, they complete by their peculiarities the condition necessary to the evolution of elements, which, under the same degree of electrification, would not be evolved if the metals were away; and, on the other hand, it also may be that after the metals are polarized, and a consequent state of reactive tension so set up, a degree of conduction proper may occur between them and the electrolyte simultaneously with the electrolytic action. There is now no doubt that as regards electrolysis and its law, all is as if there were but electrolytic conduction; but, as regards static phænomena (which are equally important) and the steps of their passage into dynamic effects, it is probable that conduction proper rules with electrolytes as with other compound bodies; for it is not as yet disproved, is supported by strong presumptive evidence, and may be essential. Yet so distant are the extremes of electric intensity, and so infinitely different in an inverse direction are the quantities that may and do produce the essential phænomena of each kind, that this separation of conductive action may well seem perfect and entire to those whose minds are inclined rather to see conduction proper replaced by electrolytic conduction, than to consider it as reduced, but not destroyed; disappearing, as it were, for electricity of great quantity and small intensity, but still abundantly sufficient for all natural and artificial phænomena, such as those described, where intensity and time both unite in favouring the final results required.

But we must not dogmatise on natural principles, or decide upon their physical nature without proof; and, indeed, the two modes of electric action, the electrolytic and the static, are so different yet each so important, the one doing all by quantity at very low intensity, the other giving many of its chief results by intensity with scarcely any proportionate quantity, that it would be dangerous to deny too hastily the conduction proper to a few cases in static induction, where water is the conductor, whilst it is known to be essential to the many, only because, when water is the electrolyte employed, electrolytic conduction is essential to every case of electrolytic action.

XIV. *On the Molecular Constitution of Crystals.*
By Robert T. Forster, *A.B.*[*]

[With Two Plates.]

ANY theories which have been advanced to account for the phænomena of crystallization have been but short lived in their reputation; and, indeed, it may be safely doubted if any occupy such a position as to entitle them to much credit.

The subject is itself one of much interest; and although the consideration that so many able minds have failed in the attempt to investigate it may naturally deter an inquirer, still the prize is sufficient to persuade him to hazard the attempt.

I shall first take a short review of the many theories which have been advanced in explanation of the phænomena, and I trust I shall be able to show that some were unquestionably faulty, and all undoubtedly and confessedly deficient.

Passing by the many absurd theories advanced on this subject in ancient times, the first whom we find deserving of notice is Christian Huygens. In his celebrated work on double refraction, he considered the crystals of Iceland spar to be built up of spheroids, which by their unequal density separated the incident light into two rays. He did not, however, give any reason why the particles are so aggregated.

Hooke, in his *Micrographia,* advanced a similar hypothesis, except that he considered the atoms to be spherical; a supposition which would have accounted for forms in the first system, but which would have utterly failed in case of the third or "rhombohedral," to which he applied it; nor does he perceive that the molecules, if left to themselves, would not assume a definite arrangement. He does not seem to consider these spheres as the ultimate atoms; he says, "that having already shown how a fluid will naturally assume the spherical form, he will proceed to show how these spheres will unite to form a crystal." On the absurdity and complication of such ideas I need not dwell.

The next who commanded attention was M. Prechtl de Brun, whose ideas were to a certain extent those of Hooke; he considered a fluid to be made up of soft molecules, which, while the body was undergoing its change of state, suffered a change of form arising from their mutual pressure, and that they were thus formed into cubes, dodecahedrons, &c.; the cubes by their union forming larger cubes, &c. Not only was this hypothesis quite insufficient, inasmuch as it substituted one difficulty for

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[*] Communicated by the Author; having been read before the Royal Irish Academy, May 14, 1855.

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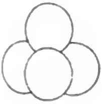

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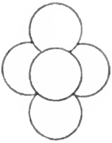

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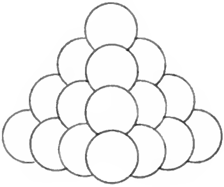

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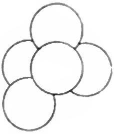

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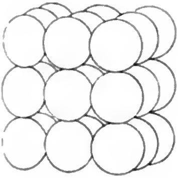

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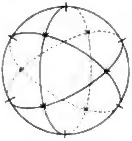

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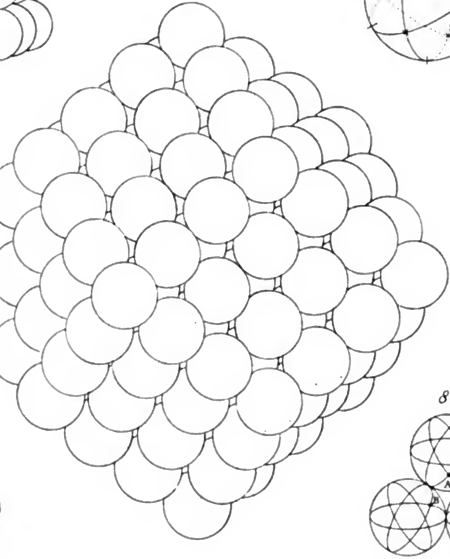

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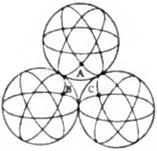

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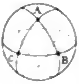

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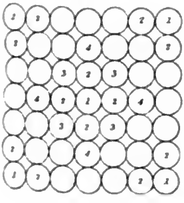

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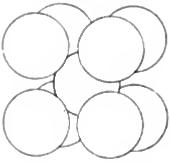

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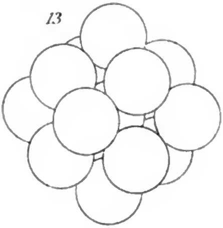

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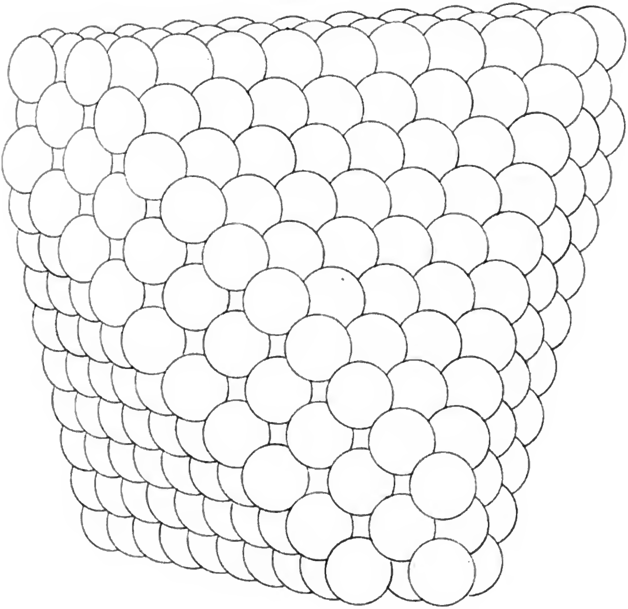

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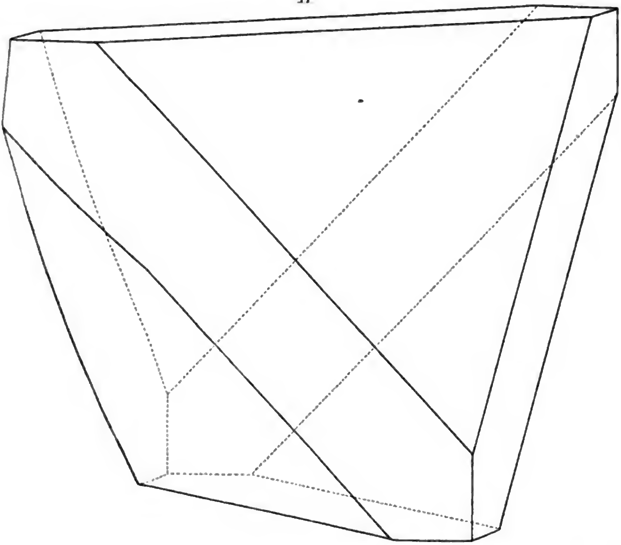

another, but it was totally erroneous in theory, as Dr. Wollaston has ably shown.

Immediately after him, Dr. Wollaston published in the Philosophical Transactions a theory of the manner in which the common octahedron and tetrahedron may be built up of spheres having a mutual attraction for each other. He evidently considered his ideas the same as those of Hooke, as he says he found himself anticipated by that writer. He does not seem to have prosecuted his inquiries very far, indeed he leaves totally unnoticed the innumerable other forms to be met with in nature. He does, however, make some observations with regard to the cube, but he does not appear to have satisfied himself; his words are to this effect:—"If a number of white and an equal number of black balls are so arranged that each white ball shall be equidistant from the four nearest black balls, and also from the adjacent white balls, the form which they would take would be a cube."

I shall now endeavour to show that this theory of the formation of the octahedron is faulty, inasmuch as the molecules, if left to themselves, would never assume such an arrangement.

If any four spherical atoms become attached in consequence of their mutual attraction, they will combine in the manner shown in Plate I. fig. 1, and any fifth atom will attach itself, as shown in fig. 2; but if we examine fig. 3, which represents the arrangement which he supposes them to have, we will find that any five adjacent spheres occupy a position such as shown in fig. 4, a position which they evidently never would naturally assume.

Wollaston himself seems to have some lurking doubts of the validity of this assumption, as he begins by showing how the spheres will become aggregated *if on a plane,* and then from this basis builds up the octahedron; this, however, is the very manner in which crystals do not form.

The first who really investigated this most interesting subject was the Abbé Haüy; his work forms an æra in the history of the science. He perceived that many crystals were liable to cleavage, and that thus in many cases new solids were obtained; he was led from this to consider, that, if this cleavage were continued long enough, he would ultimately arrive at the absolute atom; and he assumed, without sufficient foundation, that this atom should have the same form as the solid obtained by cleavage.

The forms which he considered the molecules to have were the tetrahedron, parallelopiped, and the three-sided prism. He clearly showed that many of the forms could be constructed with these elements, and he pointed out how some of the secondary forms would result from certain decrements or omissions in the regular succession of the particles. He however failed in accounting for the formation of the ordinary octahedron, as such a form could not result unless from a mixture of little tetrahedrons and octahedrons, or unless the tetrahedrons were united by their corners, leaving spaces between them. The necessity for such an arrangement so unaccountable and so totally at variance with the rest of his theory was evidently subversive of it. He moreover does not attempt to explain why the decrements that we have spoken of take place, nor does it seem possible to do so on his hypothesis. He seems to have begun at the wrong end, and instead of investigating how certain particles could unite to form certain crystals, he contented himself with showing that such crystals might be formed by piling up the molecules in a particular manner.

The last and most successful writer is Mr. Dana, the American mineralogist, who first published his views on the subject in the year 1836.

He considers the molecules to be spherical, and possessed of six poles, each 90° distant from the nearest four; three adjacent poles being of one denomination and three of another, those of a like sort repelling, and those of an unlike attracting each other. By means of this hypothesis he very satisfactorily explained the cubical formation, and pointed out, like Haüy, how the many secondaries can be obtained from the cube by certain decrements at the edges or angles. He also accounted for the formation of twin crystals, by showing how two molecules may accidentally unite at a point of equilibrium between two or three poles. He supposed the molecules to be in the first system spheres; in the second spheroids; in the third spheroids, the poles being at the extremities of equal conjugate diameters; in the fourth ellipsoids; in the fifth the same, the poles being at the extremities of axes, two of which are inclined to each other; and in the sixth the same, all the axes of the molecules being inclined. He, however, does not in the least account for the formation of secondary planes (although he wishes us to suppose so), nor of the occurrence of hemihedral forms. In this respect all writers on the subject have erred in not entering sufficiently fully on the investigation; either contenting themselves, like Haüy, with considering how certain solids could be built up to form other solids, or, like Dana, merely observing how molecules, if possessed of certain properties, will unite after certain laws.

The problem, as it appears to me, is of a much more general nature; we have to consider a number of molecules to which we have given certain properties, and we have to investigate what forms they will assume, and what modifying influences may come into play during the process. We cannot expect, that, after several molecules have united and a solid has thus been formed, no change will take place in the law of attraction consequent on the mutual action of all the accumulated particles; nor can we safely suppose, that properties which are themselves transient, and certainly not existing when the body is in all its states, may not be alterable after the body has assumed the solid form.

Dana seems to have given unnecessary complication to his hypothesis, and by this means to have rendered the explanation of some of the phænomena impossible. It appears to me that all the conditions would be satisfied by the molecules having six poles, all exercising a mutual attraction for each other; the form which would result from the union of such molecules would evidently be the cube (fig. 5). But it is natural to suppose, in accordance with similar phænomena in nature, that these poles may be liable to removal or displacement on account of their mutual attraction for each other, and that the attracting fluid is only held in its place by a certain coercive force of the molecule itself, as is the case in the common magnet. As the crystal increases in size, those molecules which are at its extremities will be more and more acted on by this influence, and some may altogether lose their polarities (at the same time it is by no means a consequence that these molecules will drop off, since the attracting fluid need not be removed from the molecule, but only displaced); the consequence will be, that the free particles will not be attached to those which have lost their polarities, and modifying planes will appear.

Thus in case the molecule at each corner of a cube lose its three unattached poles, a plane will appear at each corner, and the octahedron will be thus formed.

If a row of molecules along each edge lose their unattached poles, planes will appear on each edge, which will of course belong to the dodecahedron; but if two rows of molecules lose their poles, the resultant form will be a tetrakishexahedron; if three, another tetrakishexahedron.

Let us now consider under what circumstances these different results will take place. If we consider the manner in which the additional particles are laid on those layers already formed, it is evident that those particles nearest the centre will be the first to which new molecules will become attached, and that in general the particles will attach themselves as nearly as possible to the centre of the crystal, inasmuch as the attraction is strongest there. On the contrary, those molecules which are furthest from the centre will be the first to lose their attractive force, as numbered in fig. 6.

If all the molecules in a face already formed be covered except the external row, and if, while the internal molecules were having new molecules attached, this external row lost its polarities, then dodecahedral faces will appear. In like manner, if only the extreme corner molecule lost its polarities, then all would be covered but that one, and the plane formed would belong to the octahedron. In fact, we have the molecules becoming consecutively attached in one direction, and the polarities of those molecules to which they are to be attached being consecutively removed in exactly an opposite direction; of course, then, after a certain number of molecules have become attached, no more molecules will be attached on that layer, and a new layer will be commenced in exactly the same manner, and exactly the same result will ensue, the same number being dropped as in the last layer, and the consequence being that a plane will be formed being a tangent plane to those molecules to which no others have become attached. It might appear at first sight that this is not true, since the number of molecules in each face are as the squares of the number of molecules along an edge; but we are to remember that all those molecules equidistant from the centre have new molecules attached to them at the same time; therefore the time which elapses while the whole face is being covered is only that which elapses while a single row is being covered.

There is one description of crystals which have been left nearly altogether unnoticed by all writers on this subject, namely hemihedral forms. The only person who mentioned them at all was Wollaston, and he contented himself with proposing a theory for the formation of the tetrahedron, leaving quite unexplained the union of the cube with the tetrahedron (although his own thoughts were elicited by the octahedral cleavage of cubical fluor spar). His theory of the formation of the tetrahedron I have already shown to be faulty. I will now endeavour to show under what circumstances hemihedral forms without parallel faces will be produced. The theory I am about to put forward is not sufficient to explain the formation of crystals with parallel faces; but this does not invalidate it, inasmuch as, from the well-known fact that forms of these two denominations are never found united, we can safely infer that there is something essentially different in their nature; indeed we might say, that if any theory was capable of accounting for both, the fact of its doing so would be an argument for its rejection. If the molecules be spheres, each having twelve poles, or centres of attractive force, on its surface, and these poles were arranged as shown in fig. 7, the form which such molecules would assume is the tetrahedron. (The intersection of the great circles in the figure show the position of the poles; each great circle is divided into six equal parts by the others, and they cut each other at angles of 70°31′44″.)

For if any two of these spheres become united by two of their poles, they will evidently, from the mutual action of all the poles, assume such a position as shown in fig. 8; and a third molecule would be attracted in a similar position, while a fourth would attach itself to the three poles, A, B, C, by its three poles *a, b, c* (fig. 9). We have now a tetrahedron formed, and by precisely analogous reasoning we can continue the process of formation. It will be observed that a fifth molecule, if attached, will be in the same plane with three others, and will only touch two; a result to the necessity of which I have already adverted when speaking of Dr. Wollaston's hypothesis.

If a tetrahedron be thus formed, and if a row of molecules along each edge lose their polarities in each consecutive layer, each additional layer of particles will be deficient by a row, and faces will appear replacing the edge of the crystal tangentially; such faces, it is well known, belong to the cube. We have now a compound form consisting of a cube and tetrahedron. (*Vide* Plate II. figs. 10 and 11.)

If each layer be deficient by one molecule at each corner of the faces, planes will appear truncating the corners of the tetrahedron, which will of course belong to the octahedron. In like manner, if two or more rows be omitted, the hemiikositetrahedron will be formed. And if the same occurrences take place after an octahedron has been formed, the holohedral forms of these solids will be obtained.

It is well known that crystals in the first system are liable to three distinct cleavages, which would evidently indicate three distinct formations; two we have already investigated, namely the cubical and tetrahedral; we will now endeavour to show under what circumstances spherical molecules will assume the form of a dodecahedron, and will then proceed to prove that these three formations will give rise to three distinct cleavages, the directions of cleavage being in every case parallel to the faces of the solid itself, an instance of agreement which is strongly suggestive of truth.

If the spherical molecules have eight poles situated in the same relative positions as the corners of a cube, they will, if under no disturbing influence, assume the form of a dodecahedron; for if any sphere attract eight others, they will be arranged as shown in Pl. I. fig. 12, all the poles of each sphere having the same position as regards the eye; for it is evident, that, if after union they have any other position, the mutual actions of *all* the poles will cause them to rotate on each other till they have that definite position; these eight will be attached simultaneously, and immediately six others will be attached to them, as shown in fig. 13. We have now a dodecahedron formed; and it is evident that as the crystal grows, the same form will be retained (fig. 14). Such a crystal will be liable to modification in the same manner as those already discussed, and thus all other forms may be built up in this manner.

We have thus seen that on these three hypotheses it is possible to explain all forms; the cubical gives rise primarily to the cube, and by means of decrements to all others except the tetrahedron. And here we may mention a fact strikingly confirmatory of these views, namely, that *there is not a single instance in nature of a crystal exhibiting the tetrahedral form, or that of any of its hemihedral derivatives, and possessed of cubical cleavage.* The tetrahedral gives rise to that solid and all others by its modifications, and the dodecahedral in like manner gives rise to all forms; and thus any crystal may have any cleavage.

As regards cleavage, it takes place in every case in whatever direction the least resistance is met with; thus where each molecule has six poles the cleavage is cubical, because such a division separates each atom from one other only, whereas the dodecahedral cleavage would separate each atom from two others, and the octahedral from three, as is at once evident on inspection of fig. 5.

In like manner, in the tetrahedral arrangement the cleavage is parallel to the faces; for it is evident (fig. 3) that such a division would separate each molecule from three others, whereas the only other cleavage which it could possibly undergo, namely that parallel to planes on the edges, would separate each molecule from four others. Lastly, where the poles are eight in number, and the formation dodecahedral, the cleavage planes are parallel to the faces; for by division in such directions we overcome the resistance of two poles in each molecule, while if we divided the crystal by planes parallel to the faces of the cube, we should separate each atom from four others; if by planes parallel to the faces of the octahedron, from three others.

These three formations occur in the other systems also, and give rise to corresponding cleavages; but as some of the poles are stronger than others, in those systems particular cleavages are eminent.

We have now reviewed all the different forms of the first system: it only remains to speak of hemitrope and twin crystals.

Dana has satisfactorily shown that these will arise from the accidental union of two molecules at the middle points between two or more poles; an account of his theory first appeared in the American Journal of Science for 1836. He was certainly the first person who fully investigated this part of the subject; but justice compels me to add, that his ideas were directly borrowed from Sir David Brewster, who some years before, in an article on polarization produced by compression, thus expressed himself:—"The phænomena of cleavage and of hemitrope forms would clearly indicate that the integrant molecules are spheres, each having six poles on its surface." This article was quoted by Dana in the American Journal of Science, and formed the basis of his investigations, without however being acknowledged. Sir David Brewster goes on to say, that it is owing to the different degrees of force possessed by the different poles that the different cleavages are due; and in this he is clearly in error. For if a cube be divided either by the cubical or octahedral cleavage, every molecule will eventually become separated from every other. In fact, to divide a crystal in which the arrangement is cubical by the octahedral cleavage, we must at *each section* separate every molecule from three adjacent molecules, or in other words, we must overcome the cohesion of three of its adjacent poles; but it was to the unequal cohesion of these very three poles that he ascribed the liability to cleavage.

Dana also, as I have already stated, pointed out that in the second and remaining systems the molecules are no longer spheres, but spheroids or ellipsoids; and with this modification, all I have said applies word for word to the other systems.

XV. *On the Felspars occurring in the Granites of the Dublin and Wicklow Mountains. By the* Rev. Joseph A. Galbraith, *M.A., Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

A COMMUNICATION from Mr. Jennings which appeared in your last Number, on the Felspars of the Dublin Mountains, makes it necessary for me to request that you will insert in your Journal a short account of a discussion which arose some time since in the Royal Irish Academy on this subject.

In the month of January 1853, Sir Robert Kane brought under the notice of the Academy an analysis of the waters which descend from the sides of the Dublin Mountains. The following is his analysis of the residue after evaporation, of what he considered to be a typical specimen:—

Organic matter	23·30
Carbonic acid	7·40
Muriatic acid	17·99
Sulphuric acid	6·34
Silica	3·81
Lime	3·03
Magnesia	0·85
Potash	2·86
Soda	30·48
Loss	3·94
	100·00

In the discussion of this analysis, he considered that the muriatic acid present existed in the form of common salt, derived from the vapours of the adjoining sea, which leaves of the soda 15·20 to be disposed of; he then allocated all the potash to the sulphuric acid, of which it saturates 2·43, leaving 3·91, which he supposed to be combined with soda, leaving finally of this alkali 12·16 to exist in combination with silica and the organic acids. This large amount of silicate of soda in the waters he attributed to the decomposition of the felspathic materials of the granite; and in conclusion, drew the inference that the felspar of the Dublin granites is, upon the whole, a soda or albitic felspar, although in particular localities orthose or potash felspar may be found. This inference he also stated was verified by a great number of analyses of specimens of granite taken from various parts of the range which extends from Dublin into the county Wicklow; in all these specimens both alkalies were found, but the soda in such excess, as to indicate that the potash should be referred to the mica which the granite contained, and the soda allocated to the felspar.

By this interesting communication to the Irish Academy two important questions were raised; viz. 1, whether the felspar which enters into the constitution of the granite is an albite; 2, whether orthose or potash felspar is a rare mineral, to be found only in special localities, such as Killiney.

In a communication made at a subsequent meeting by Dr. Apjohn, the first of these questions was fully disposed of, as from the analyses which he furnished of specimens of felspars taken from different localities, potash was invariably found in notable quantity, constituting at least one-half of the alkalies present, showing that the felspar could not in any sense be considered as albite or soda felspar.

To the second of these questions I directed my attention, and having procured specimens of felspar, taken from seven localities at about equal distances in a line stretching forty miles from Dublin, I subjected them to careful analysis, and furnished the results to the Academy, and also to your valuable Journal. These results leave nothing to be wished for as far as the second question is concerned, as in every case the specimen appeared to be true orthose, resembling in every particular those which are to be found in Killiney and Dalkey.

In the last Number of your Journal I perceive that a communication has been made by Professor Haughton, in which he gives a number of analyses of *rock specimens* taken from different localities in this range; his results appear to be altogether inconsistent with the opinion expressed by Sir Robert Kane as to the character of the felspars of these mountains, and therefore, of course, with the extended series of analytical investigations of rock specimens on which Sir Robert Kane stated that he founded his opinion, and which, although now nearly three years have elapsed, he has never produced.

Professor Haughton has, in the course of his research, discovered a most interesting fact as to the character of these granites, namely that in every specimen taken from the range itself the potash predominates over the soda, while in the outlying granitic protrusions, the soda prevails, thus indicating a curious constitutional difference, which may, and probably does, accompany a difference of age in these rocks hitherto undiscovered.

As to Mr. Jenning's interference in this discussion, he has not, in my opinion, contributed much to either side; nor do I think he understands the question in all its bearings. In the course of this article it has been stated, that, at the very outset of the discussion, analyses of felspars were produced and relied on by Dr. Apjohn, in which the proportion of the alkalies was nearly equal; it will therefore appear that the production of a single additional, and that but a partial analysis, confirming, so far as it goes, Dr. Apjohn's result, cannot throw much new light on the question at issue.

Immediately after the meeting of the Geological Society, at which I obtained a part of the specimen furnished by Mr. Jennings, I made the following analysis:—

Silica	70·32
Alumina	16·12
Peroxide of iron . .	3·20
Lime	1·34
Potash	4·65
Soda	3·39
Loss by ignition . .	0·96
	99·98

I used the greatest care in obtaining the alkalies *separately*, and have no doubt that the numbers just given accurately represent their relative proportions. It is to be regretted that Mr. Jennings did not obtain from Mr. Campbell a complete analysis, as perfect confidence cannot be placed in quantitative results if all the constituents be not weighed; but if we agree to accept Mr. Campbell's determination of the alkalies as perfectly correct, it is as wholly irreconcileable with Sir Robert Kane's statement as any that has been produced during the controversy.

I regret particularly that Sir Robert Kane has left this question, so far as he is concerned, without any means of solution. And as those who are competent to decide such questions naturally withhold their judgement till all sides are heard, I hope I may be excused if in these pages I express the hope that ere long Sir Robert Kane will favour the public with the investigations on which he has based his opinion. I may, no doubt, be in error in some of my views of this question; and if so, would be glad to have my mistakes corrected by so competent an authority.

I remain, Gentlemen,
Your obedient Servant,
JOSEPH A. GALBRAITH.

Trinity College, Dublin,
July 16, 1855.

XVI. *On two doubtful British Species,* Glottalite *and* Zeuxite.

By R. P. GREG, *Esq.*[*]

I HAVE lately had an opportunity of seeing the original specimens from which, many years ago, the late Dr. Thomson described the rather doubtful species *Glottalite* and *Zeuxite.*

1. The *Glottalite* occurs both crystallized and massive. The massive much resembles, as indeed Dr. Heddle lately suggested it might be, edingtonite. The crystals, which Dr. Thomson rather loosely described as "having *nearly* equilateral faces, either four-sided pyramids or regular octahedrons," are without doubt chabasie. The crystals, though not easy to distinguish, occur in double six-sided pyramids, a form of chabasie sometimes also called phacolite. Another proof of its being in all probability chabasie is the fact, that *glottalite* is described by Thomson as containing $21\frac{1}{4}$ per cent. of water, which is exactly the proper proportion for chabasie.

2. *Zeuxite.*—This mineral I have long considered as merely a tourmaline or schorl, and my inspection of Dr. Thomson's specimen only confirms me in that opinion. At the time the specimen was found, viz. in 1814, at Huel Unity, little was then known comparatively of the precise composition of tourmaline,

[*] Communicated by the Author.

or of the laws of isomorphism; and many minerals might then have been considered true and distinct species which would be now united. The following Table of *old* analyses of tourmaline, given in Dr. Thomson's original book, will at once show how highly probable it is that *zeuxite* is in reality nothing but tourmaline, and that at that time no better analysis could have been expected.

	Zeuxite.	Tourmaline.			
	Thomson.	Gmelin. Eibenstock.	Leplay. Mount Rosa.	Gmelin. Greenland.	Gmelin. Devonshire.
$\dddot{\mathrm{Si}}$	33·48	33·05	44·10	38·79	35·20
$\dddot{\mathrm{Al}}$	31·85	38·23	26·36	37·19	35·50
$\dot{\mathrm{Fe}}$	26·01	23·86	11·96	5·81	17·86
$\dot{\mathrm{Ca}}$	2·45	0·85	0·50	....	0·55
$\dot{\mathrm{Mg}}$	....	....	6·97	5·86	1·13
$\dot{\mathrm{Na}}$	....	3·17	....	3·13	2·09
$\dot{\mathrm{K}}$	....	....	2·32	0·22	....
$\dddot{\mathrm{B}}$	....	1·89	5·72	3·63	....
$\dot{\mathrm{H}}$	5·28	....	0·60	1·86	....
	99·07	101·05	98·46	96·48	92·33
Spec. grav.	3·05	3·03	3·14	3·06	3·24

Not only does the specific gravity of *zeuxite* agree with that of tourmaline, but its behaviour before the blowpipe, as described by Dr. Thomson, is also similar; and it is worthy of remark, he mentions that the water in *zeuxite* contained traces of muriatic acid. Had he tried for boracic acid, he would more probably have found it was boracic acid.

XVII. *On the Induction Apparatus and its Effects.*

By J. C. POGGENDORFF.

[Continued from p. 12.]

2. *On the Phænomena observable during the Action of the Apparatus.*

HITHERTO it has not at all, or has not sufficiently been kept in view, that the phænomena manifested by the apparatus are essentially different according as the ends or poles of the induction wire are either (1) connected by means of a good conductor, or (2) separated by air or gas, or (3) separated by means of a liquid or solid insulator. In this peculiarity Volta's induction apparatus differs essentially from the magneto-electric machine.

First case.—When the poles of the induction coil are connected by a metallic wire or by a good conducting liquid, the induction current consists of two parts, or two alternate currents, one in the opposite, and the other in the same direction as the inducing current, corresponding respectively to the moments when the circuit of the latter current is closed and broken.

This can be detected by the galvanometer. When the induction wire is very thin and long, and a galvanometer is introduced, no deviation is observed; but when the said wire is thicker and shorter, or the inducing current stronger, the phænomenon of a twofold deviation, which I described in 1838, is observable*. This is caused, of course, by quick, successive magnetizations of the needle in opposite directions.

If water or dilute sulphuric acid is introduced into the current between platinum plates, oxygen and hydrogen are liberated at each plate, and after the cessation of the current no trace of a polarization of the plates can be detected.

In a similar manner, moistened iodide of potassium paper, when put in contact with the poles, shows a separation of iodide of equal strength at each pole.

An electric thermometer rises, the direction of the current being indifferent to it, but a thermo-electric current receives no charge.

Lastly, the condenser has no influence upon these phænomena.

Second case.—When the poles are separated by a stratum of air or gas, even although the stratum be very thin, only one of the two induction currents acts, *i. e.* the one produced by breaking the galvanic current; the other, produced by closing the current, remains without external action in the coil, which on that account obtains fixed poles.

This is evident in the phænomena of tension, which are exhibited when the induction coil is unconnected, that is to say, when its poles are separated by a great thickness of air. If during its activity an electrometer be brought for a moment into contact with one pole of the apparatus, it will be quite accidental whether it will become charged positively or negatively, as the two electricities change places continually on each pole. If the electrometer be placed near enough to allow sparks to spring over, only one kind of electricity is obtained from each pole, *i. e.* that which belongs to the pole when the current is interrupted.

This is still better shown when one of the poles is connected with the earth by means of a wire. The electrometer, when held at a considerable distance from the other pole (*e. g.* an inch and a half), becomes charged with the electricity which corresponds to that pole and the above-mentioned current; but it is charged by radiation merely, and not by sparks. In accordance with these results, it was found that when a wire connected with the earth was held at a short distance from one of the poles, and sparks taken from it for some time, then, after the cessation of the current, the whole induction coil was charged with that kind of electricity which belonged to the other pole.

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* Poggendorff's *Annalen*, vol. xlv. p. 353.

The sparks which can be drawn from one of the poles of the induction coil in its unconnected condition are of course stronger when the other pole is connected with the earth, and apparently always stronger at the negative than at the positive pole.

When the two poles of the induction coil are brought so near to one another that sparks pass between the two, the circuit may be considered closed; but this closing of the circuit differs from that produced by a good conductor; in the latter, both currents circulate; in the former, only the one which is produced by breaking the inducing current. The stratum of air between the poles, which may be compared to a filter, insomuch as it keeps one current back, requires to be only very thin. A piece of dry blotting-paper, when placed between two metallic plates connected by wires with the poles of the induction coil, perfectly fulfils the purpose of separating one-half of the electricity, and occasions a current of constant direction, which, its discontinuity excepted, possesses all the properties of a galvanic current of high intensity,—is similar, in fact, to that of a very great number of very small plates.

The interruption by means of points serves, of course, the same purpose; and in many respects this purpose may be still better effected by means of the electric egg, because in rarefied air the discharge between the ends of the metallic conductors takes place much more quietly than by the sudden springing of sparks in air of the common density. The thinner and more rarefied the stratum of air, the less the current will be weakened by this interruption, although, perhaps, a part of the same may always remain behind in the wire.

The *effects* of the current when interrupted by a stratum of air, and its difference from a perfectly closed current, are manifested by the galvanometer. A definite deviation is now obtained which depends upon the direction of the current. It is *greater* with a *thick* than with a *thin* induction wire, and it is also more *continuous* and *regular* when the current is interrupted by discharges in a partial vacuum, than when it is broken by sparks in common air. When we consider the difficulty with which the deviation of a magnetic needle is effected by frictional electricity, the facility with which it is here produced, even without any insulation of the galvanometer, will appear somewhat remarkable.

According to Riess, a thermic action is produced in the wire of the interrupted current, but it is an exceedingly weak one. With my less sensitive air-thermometer I could not detect this action, although the same was sufficiently sensible to show the increase of heat in the perfectly closed wire, especially when the latter was thick. It is worthy of notice, however, that notwithstanding this, a thermo-electric battery was charged in exactly the same manner as with a galvanic current, that is to say, opposite to the direction of the same. I observed it in a combination of two V-formed bismuth and antimony pairs. The common thermo-electric batteries are not here applicable, in consequence of their imperfect insulation.

The sparks at the point where the current is broken have also a thermic action, in fact, a much greater one than that which takes place in the metallic circuit of the current. When very thin platinum wires are used as poles, and when they are brought near one another, it has already been observed by several physicists that the point of the negative wire, which is then surrounded by the positive sparks, becomes red-hot.

Chemical decompositions, decompositions of water, iodide of potassium, &c., are effected in exactly the same manner as with a galvanic current, *i. e.* only *one* electrolyte is separated at each pole. In water the electrodes become polarized, and when they are composed of very *thin* platinum wires, the negative one becomes slightly illuminated at the extremity.

Many of these decompositions are interesting when they are so made that the point where the current is broken is at the liquid itself. For this purpose the wires of the discharger are placed vertical and near one another, and the liquid is made to approach them from below, so that the sparks may be made to strike the liquid from both wires at once, or only from one, the other being kept immersed. In general it is found that the stream of sparks from the positive pole possesses a yellow or reddish-yellow colour at the top, close to the point of the wire, and forms a blue disc below on the surface of the water; whereas the stream of sparks from the negative pole is blue at the top, and surrounds with this light a part of the wire from the point, whilst a yellowish glimmer rests upon the liquid. When the liquid is placed nearer to the wires, about a quarter of a line from them, the phænomenon reduces itself to blue sparks at the negative, and yellow ones at the positive pole.

Further, the phænomenon is different according to the nature of the liquid. Non-conducting liquids, such as turpentine, do not, of course, show the phænomenon at all. With distilled water it is weak, because the sparks are small and have little intensity of light. It is more perfect with weak sulphuric acid, hydrochloric acid, nitric acid, with solutions of hydrate of potash, carbonate of soda, &c.; with these alkaline liquids the middle part of the stream of sparks has a beautiful yellow colour, at the positive pole it is even gold-yellow. The phænomenon, however, is most perfect with concentrated sulphuric acid: not one of the liquids which I examined showed the blue disc on its surface underneath the positive pole so great and clear, and beautifully coloured. All the above-mentioned phænomena are more fully developed when the experiments are made in air rarefied to about 8 inches of mercury pressure.

No chemical decomposition can be seen in any of the above cases when both poles send sparks to the liquid. If, however, sparks are allowed to issue from one wire only, and the other is held immersed, a decomposition is immediately visible at the latter, and it is quite indifferent which pole is immersed, and from which sparks are emitted. This remarkable decomposition at a single actual electrode is certainly weak, but I have distinctly observed it with dilute acids, and still more distinctly with concentrated sulphuric acid.

With a solution of iodide of potassium the action is different, for it is decomposed even when sparks fall upon it from both poles; consequently when there are no real electrodes. Whether nitric acid or ozone is thereby formed, may remain at present undiscussed.

If a piece of blotting-paper be moistened with this solution, put in contact with the negative pole, and the positive pole be brought sufficiently near, a brown spot will immediately make its appearance underneath the latter, and the positive stream of sparks will have a brownish colour unlike the violet colour of the vapour of iodine. Underneath the negative pole no colour is visible. But if, on the contrary, the paper be touched by the positive pole and receive sparks from the negative, a brown colour will be perceptible underneath both, though much weaker underneath the latter. Something similar is observed when the negative pole is connected with the earth, and a piece of moist iodide of potassium paper is held opposite the positive pole only.

Paper and sparks have then a brown colour. If sparks are allowed to strike upon the paper from both poles, a brownish colour will generally be perceptible underneath both, but always weaker under the negative than under the positive.

If the two moist places upon the paper which are struck by the sparks be separated by a dry part of small breadth, sparks will also spring over the latter.

A piece of paper saturated with nitrate of silver, perchloride of platinum or perchloride of gold, is coloured brown underneath the positive stream of sparks. With perchloride of gold and perchloride of platinum the sparks themselves have a brown colour.

Underneath them the paper begins to smoke, it soon dries, and now a hole is burned through it, which, glimmering like a fusee at the edges, rapidly increases in size. Brown filaments rise from the paper, attach themselves to the wire, and form a kind of beard around the same. Underneath the negative stream of sparks these phænomena do not manifest themselves, but a dark spot is there formed, which evidently consists of reduced metal.

The effects of the induction current are now described. The reason why that current is only active which is produced by breaking the inducing current, and why the other, generated by closing the same, has no action and does not possess the requisite tension to break through so thin a stratum of air, is simply to be sought in the fact, that by closing the current a complete conducting circuit is formed, which, like all continuous metallic circuits surrounding the induction coil internally or externally, is unfavourable to the development and the duration of the current induced therein.

Good conducting liquids, like those of a galvanic circuit, are in this respect not different from metals. Of this it is easy to arrive at a conviction if the primary coil possesses two wires. When the galvanic current is sent through one of these wires only, and the other is connected with a pair of metallic plates, the development of sparks by the induction coil is immediately suspended when these plates are immersed in dilute sulphuric acid*.

Under ordinary circumstances, when the current is broken, the destruction of the closed circuit itself removes this impediment, and nothing impedes the full development of the current in the induction coil, consequent upon this act, except what may be termed an accumulation of electricity at the ends of the interrupted inducing current, as well as a similar accumulation at the ends of the induction wire: both are produced by the resistance which the air opposes to the passage of electricity.

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* For the same reason a former experiment of mine failed. I there attempted to generate a strong induction current by continually reversing the magnetization of the soft iron core of the apparatus. It is easy to produce this reversion *without interruption*. For this purpose it is only necessary to keep one wire of the primary coil *continually* connected with *one* galvanic element, and to connect the other, in opposite direction, with a battery consisting of two such elements, and continually interrupted by a Neef's hammer. This combination, however, did not produce the intended result, because within the induction coil a circuit closed by a good conducting liquid was always present.

The first-mentioned accumulation is now prevented by Fizeau's happy idea of employing a condenser, for by it the accumulated electricities can flow quickly out of the inducing wire, without a closed circuit being formed.

The quantity of electricity in the induction wire is not increased by the condenser. This is easily shown by closing the induction circuit after introducing a magnetometer into the same. The deviations which are obtained in the latter instrument at each time of opening and closing the inducing current are not only equal to one another, but are as great without the condenser as with it*.

The generation of electricity in the induction wire at the moment of breaking the inducing current is, however, accelerated by means of the condenser, and consequently the tension of the generated electricity is thereby increased. Hence a pole of the unconnected induction current acts upon the electrometer at a much greater distance when the condenser is employed than when it is not. From this it follows, that when the ends or poles of the induction wire are separated by a certain thickness of air, the sparks which break through it are increased in number when a condenser is employed; indeed, many sparks appear when this distance between the poles is so great, that, without a condenser, not a single one would be visible. The condenser, therefore, actually increases that part of the electricities which equalize themselves at the poles, or are set in circulation, when the induction wire is interrupted by air.

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* From the following considerations it is well known that the equality of the quantities of electricity which are generated may be concluded from the equality here mentioned. If, at one time, through a section of the wire, a quantity of electricity equal to *unity* passes in the *unit* of time, so that an intensity of current and a deviation, each equal to *unity*, are obtained; and if, at another time, the *unit* of electricity passes through the same section in *ten* units of time, the intensity of current will be equal to *one-tenth*, and consequently the deviation in the unit of time will be *one-tenth*; therefore in the whole time it will be $=10\cdot\frac{1}{10}$, that is, it will be again $=1$; provided, of course (which has been assumed throughout), that the whole time of passage is so small in comparison to the time of oscillation of a magnetic needle that the single impulses will strike the needle in about the same situation, that is to say, not far distant from its position of rest.

It is otherwise with the quantities of heat which are developed at the opening and closing of the circuit; inasmuch as these are proportional to the squares of the intensities of the current, and not (like the deviations) directly proportional to the same, we cannot conclude the equality of the quantities of heat from the equality of the quantities of electricity which are generated, unless the times in which they are generated are also equal. For example, if at one time through a certain section the *unit* of electricity passes in the *unit* of time, so that an intensity of current and a quantity of heat, each equal to *unity*, are obtained; and if at another time through the same section the *unit* of electricity passes in the time *ten*, then the intensity of current will be equal to *one-tenth*, consequently the quantity of heat generated in the unit of time will be $=\frac{1}{100}$, and in the whole time of passage will be $=10\cdot\frac{1}{100}=\frac{1}{10}$, therefore it will be *ten* times smaller; that is, inversely proportional to the time of passage.

Hence, if the equality of the quantities of electricity which are generated be proved by means of the magnetometer, the relation between the times of generation may be determined by means of the electric thermometer. Instead of the latter instrument, the electric dynamometer, which W. Weber has already employed for such determinations (Pogg. *Ann.* vol. lxxiii. p. 215), could be used. Even a galvanometer with a soft iron needle would determine, approximately, the duration of the times of generation.

This is also proved by the galvanometric deviation, which, under these circumstances, is increased; whilst, as has before been mentioned, it is not changed by the condenser when the metallic continuity of the wire is unbroken. It is also proved by the decomposition of water which is effected by the induction current when broken by air; for this action is also increased by the condenser.

What has here been said of tensions, sparks, deviations, and decompositions, is also true of the physiological actions,—of the shocks which are received when part of the body is introduced into the circuit. These are also increased by the condenser. Not only is the action of the *external* or actual induction current more intense, but that of the *inner* or extra current is also increased. I have convinced myself of this in several ways, the least painful of which was the following. Two platinum plates were placed in spring-water, and connected either with the poles of the induction coil or with the hammer on each side of the point of interruption. If the finger be immersed in the water between the plates, the effect of introducing the condenser can be observed without pain. The action is increased with the quantity of the current, as Faraday terms it. With the thin induction wire the action is exceedingly weak, stronger with the thick induction wire, and most sensible with the still thicker wire of the extra current.

These experiments induce me to believe that for most of, if not for all, the medical applications which are now so frequently made of the apparatus, the induction coil is quite superfluous, for the extra current can easily be made so strong that no patient would be able to bear its effects.

Certain it is that the action of the condenser is strengthening only when, between the poles, there is a resistance to overcome. The more this resistance is diminished the less the action of the condenser. On this account its action is much stronger with sparks in free air than with the luminous phænomena in the electric egg. The more the latter is exhausted the less the effect of the condenser, which effect disappears altogether (as in metallic contact) when the exhaustion is carried to a line, or less, of mercury pressure; provided only that the induction current itself is sufficiently intense. For a similar reason an induction coil which possesses a short wire produces much weaker sparks in free air than one with a long wire; whereas with the former the luminous phænomena in a well-exhausted electric egg are very little less developed than with the latter. I observed this with the induction wire only 1200 feet long.

Fizeau does not appear to have thoroughly understood the action of the condenser. Amongst other things, in order to explain the action of the instrument, he only considers a single interruption of the inducing current, consequently only a single charge of the condenser, and never mentions what further occurs. But it is evident, in order that the condenser may continue to serve its purpose, it must be discharged before every interruption of the current; and this actually takes place every time the circuit is closed after it has been broken. At both these times sparks are visible; in fact, when a condenser is employed, Neef's hammer occasions a double series of sparks, which, although they cannot be separated by the eye, owing to their quick succession, are nevertheless *distinguishable* in character. The spark, on breaking contact, is always weakened by the condenser, and the degree in which it is so weakened may be considered as a measure of the condenser's action*; but the spark caused by its discharge is proportionally strengthened.

Thus it appears, that, by the addition of a condenser, the *luminous phænomena* at the hammer are, according to circumstances, apparently *unchanged*, *increased*, or *diminished*. For example, when a double wire around the primary coil was employed, together with a soft iron core, and a battery of two of Grove's elements, the introduction of the 8-feet condenser caused the luminous phænomenon at the hammer to decrease; with the 2-feet condenser it was, on the contrary, increased, without any particular change in the sparks of the induction coil being perceptible in the two cases. By employing *one* of Grove's elements, with which the sparks at the hammer are always weak, and are still further weakened by the condenser, the discharge sparks are scarcely perceptible, although they can be *heard*, and by their irregular and somewhat sonorous stroke they can be easily distinguished from the regular beat of the hammer. On the contrary, by employing a strong galvanic current, these sparks are quite visible; they are more steady than the interruption sparks, emit a very clear light, throw out smaller sparks, and, with condensers of small dimensions, assume the appearance of a small flame breaking forth from under the hammer. This is a consequence of the great intensity of the current, which has also the effect of fusing the platinum extremity of the hammer, even when the same is 1 millim. thick, and soldering it to the anvil, so that the action of the instrument is arrested unless the small electromagnet has sufficient power to overcome the solder. The latter always occurred when a current from one of Grove's elements was conducted through the wire 400 feet long and two-thirds of a millim. thick.

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* When the metallic continuity of the induction circuit is unbroken, the sparks at the hammer are always considerably diminished. Even when the induction circuit is interrupted by a stratum of air so thin that the induction sparks form a continuous line, the sparks at the hammer are always diminished.

Riess found that this fusion could be avoided by using a long, thin, German silver wire to connect the condenser with the hammer, and thus throwing a body of great resistance into the circuit. The discharge sparks, however, are thereby merely weakened; they can still be observed by employing the current from a battery of two elements, even when 120 or 130 inches of German silver wire, 0·45 millim. in thickness, are introduced between the condenser and the hammer by means of the rheochord, and it is at the same time indifferent whether the current traverses the wires of the primary coil side by side or *one after the other*.

Further, the action of the condenser upon the induction current is weakened by so great a length of German silver wire, as may be best seen by the luminous phænomena in the electric egg. This is most perceptible when the wires of the primary coil are traversed *side by side*; when they are arranged *one after the other*, no diminution in the action can be detected, even when 150 inches of German silver wire are used. In the latter case, however, where the interruption at the hammer is scarcely perceptible, the luminous phænomenon in the egg is, *without a condenser*, somewhat different to what it is in the first case; it is more compact, and more like that which is caused by the addition of a condenser.

The interruption sparks as well as the discharge sparks attack the hammer strongly, and pulverize the parts which beat against one another to a black powder. It is a remarkable fact, which has never yet been sufficiently reconciled with the action of a pure voltaic current, that here, where an induction current (an extra current) also acts, the negative pole is most heated, and that a transfer of platinum from this to the positive pole is the consequence. If the *platinum end* of the hammer be connected with the *negative* or *zinc* pole of the battery, and the tongue with the *positive* or *platinum* pole, the first only will wear away; and on the plate of the tongue an eminence of fused platinum will be formed, so that in time the *platinum end* of the hammer and the *tongue* touch one another. I always adopted this kind of connexion, because it is much easier to replace the platinum end and file the eminence down, than to fill up the holes which, in the opposite case, would be burnt into the plate.

Fizeau has already remarked, that the condenser may, to a certain extent, be replaced by a wire of sufficient intensity connecting the vibrating parts of the hammer. I have tested this substitute and found it to be but an imperfect one, probably because the circuit always remains closed. On this account I was led to the construction of the hammer before described, by means of which the interruptions of the current can be effected within a liquid. In order to study the action of this instrument, several liquids of very different conducting powers were employed,—dilute *sulphuric acid*, *spring water*, *alcohol of* 0·863 *spec. grav.*, and *oil of turpentine*. With *sulphuric acid* the instrument had no strengthening influence whatever upon the induction current, and that clearly on account of its great conductibility, which had also the effect of producing a copious decomposition of water even when only a single Grove's element was employed, and thereby causing hydrogen, in accordance with the direction of the current, to be liberated at the pin in connexion with the zinc. With *oil of turpentine* it had also no effect, and evidently for an opposite reason, viz. the complete absence of conductibility. On the contrary, with *alcohol*, with *spring water*, and particularly with *distilled water*, the instrument had a strong action, though still inferior to that of the condenser. It was interesting to see how a single drop of water, placed between the vibrating parts of the current-breaker, immediately caused a vivid stream of sparks between the points of the discharger, even when the distance between these points was so great, that, without the drop, not a single spark would have appeared. Water, alcohol, and oil of turpentine, however, all suffer decomposition between the parts of the current-breaker. With water this decomposition is electrolytic, with oil of turpentine it is electro-thermic (so termed by me in a memoir published in 1847*), that is, it is produced by the heat of the interruption spark; and lastly, with alcohol the decomposition is a mixture of both the former.

The superiority of the condenser, when compared with any of the above liquids, is best proved by combining it with the current-breaker when it is playing in water, alcohol, or oil of turpentine; a strong stream of sparks is immediately produced between the poles of the induction wire, apparently just as strong as when the current-breaker works in air. This combination is further interesting, because it furnishes an opportunity of more minutely observing the discharge sparks. They are here particularly strong and brilliant; they spurt and hiss, particularly in alcohol and oil of turpentine, and they pulverize the platinum to a black powder, so that in a few moments the liquid has just the appearance of ink. The last also takes place without a condenser, but much more slowly and less intensely.

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* Poggendorff's *Annalen*, vol. xciv.

Third case.—Interruption of the Induction Current by Insulators.

When a glass plate is placed across the stream of sparks passing between the poles of the apparatus, it almost entirely causes their cessation, provided that the poles consist of wires more or less pointed at their extremities. It is otherwise when the poles consist of plates. For example, if a square copper plate connected with one of the poles be placed upon the table and covered with a glass plate; and if upon the latter a second, smaller, round copper plate, connected with the other pole, be placed, a durable, loud, crackling noise will be heard as soon as the apparatus is set in activity; and if the experiment be made in the dark, the small round disc will appear surrounded by a halo of electric light, consisting of an innumerable number of small sparks in unceasing motion. The smaller the round plate, the broader the halo. On the lower great plate, which in this case was 3 inches square, no illumination was visible, although the glass plate projected more than half an inch over it on each side. It is of no importance with which pole the one or the other plate may be connected; it is always the smaller of the two around whose edges the halo is visible. If the latter be moved about over the surface of the glass plate, so that only a part of it remains vertically above the lower metal plate, it is only this part which is surrounded by sparks. If both metal plates are of the same size, and the upper one is placed exactly above the lower one, no halo is visible. When viewed from one side, however, it is evident, from the illuminated parts where the metal is accidentally not in perfect contact with the glass, that from both plates innumerable small sparks pass vertically to the glass.

Instead of *one* glass plate, a number of such can be placed between the metals. I took five, whose total thickness amounted to half an inch, and a rim of sparks was always visible around the upper circular plate. This rim was certainly not so brilliant as with a single glass plate; but, on the other hand, with five plates the limit was not reached at which the halo completely disappeared. To increase the number of glass plates, however, seemed the more unnecessary, as the distance of half an inch was already considerably greater than that at which sparks would pass between the metal plates when they were alone in air without the intervention of glass.

The influence of the glass in transmitting the action of induced electricity was consequently plainly proved. The following experiments will furnish additional evidence of this. The upper round copper plate was kept insulated, and held at a distance of 7 millims. from the lower plate on the table. Now and then a single, broad, short spark darted across where the distance between them was least, for without great care the upper plate could not be placed exactly parallel to the lower one.

A glass plate, 4 millims. thick, was now placed upon the lower metal. Immediately a perfect shower of small sparks fell from the upper plate upon the glass. This change in the nature of the sparks did not, as one might imagine, arise from the fact that the distance was now diminished to 3 millims.; for when the lower system was inverted, *i. e.* when the glass plate was laid upon the table and covered with the copper plate, the sparks between the latter and the upper plate were again of the same kind as before, although, of course, smaller and more frequent than at the original distance of 7 millims.

The above-mentioned change, therefore, is an effect of the glass; and this is also evident when the latter does not at all touch the metals, but is held in free air between them. Small sparks are seen to spring from both metal plates towards the glass, at distances where, in absence of the latter, no sparks would appear.

The same effects are produced when other insulating substances are used instead of glass, *e. g.* plates of marble, gutta-percha, broad columns of liquids, such as distilled water, alcohol, or oil of turpentine, enclosed between glass plates. The electrometer proves that in all these cases the insulating substance receives no, or only a very weak and indefinite, permanent charge.

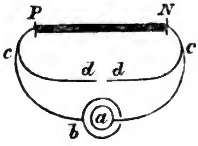
A pointed copper wire was now substituted for the small, round, copper plate, and, like it, connected with one pole of the induction coil. When held at a distance above the greater plate, the point of this wire appeared luminous in the dark; the weaker, of course, the greater the distance. With the power employed by me in all these experiments, viz. that of two of Grove's elements in combination with the shorter of my induction wires, the luminosity at the distance of 2 inches was so weak that it was scarcely perceptible. If now, when the wire is held at this limiting distance, the above-mentioned system of glass plates, half an inch in thickness, be laid upon the lower plate, the luminosity of the point will immediately be again distinctly visible; it increases the nearer the point approaches the glass, until, at a distance of a few lines, a stream of unbroken weak sparks descends. If the point is made to approach still nearer, until it is about a quarter of a line distant, the sparks will not only be clearer, but will disappear in all directions upon the glass, thereby forming a vein-like figure, similar to that of Lichtenberg's, with positive electricity. No difference could be observed in the form of this figure when the point was either positive or negative, except, perhaps, that with a positive point the figure appeared a little larger. Of course it is more perfect and beautiful when the copper plate is covered with *one* thin glass plate instead of with *five*. It may also be remarked, that even when both poles of the induction coil are pointed, the action is not altogether interrupted by the intervention of a glass plate. On the contrary, the points appear luminous in the dark, and emit phosphorescent sparks. In air rarefied to about 6 or 8 inches of mercury pressure, the vein-like figure appears well developed on both sides of the plate, the rays being more than an inch long. By further rarefaction the usual phænomenon occurs, the positive flame curves itself around the glass and forms a brilliant luminous arch.

It is interesting and instructive to examine these phænomena in their reaction on the induction current.

When two glass plates, about a square foot in magnitude, are each coated on one side with tinfoil and placed with their uncoated sides in contact, and the coatings afterwards put in connexion with the poles of the induction coil, a continuous crackling noise is heard so long as the apparatus is in activity, and in the dark the whole intermediate space between the plates, as far as the coatings extend, is seen to be filled with innumerable microscopic sparks. A piece of iodide of potassium paper placed between the plates is coloured brown, on each side equally. If the system is withdrawn from the apparatus during its activity, and then tested, by means of the wire connecting one coating, for a charge, a very weak one will sometimes be detected, though oftener none whatever. This is equally the case whether the poles have actually touched the coatings, or whether they have been separated from them by a small stratum of air, so that sparks might pass from them. It is exactly the same with a Leyden jar. In both cases it will either receive no charge at all or only a very small one, however long it may have been exposed to the current. Even when sparks are taken from one pole upon the knob of the jar, whilst the outer coating and the other pole are connected with the earth, it scarcely receives any charge at all.

If, however, whilst the apparatus is in activity, and its poles are either connected with the coatings of the jar or send sparks to them, a wire be connected with one coating and held sufficiently near to the other, broad, loud sparks will be obtained, which far surpass the thin induction sparks in splendour and strength, although they are inferior to them in length. This difference in the sparks is most perceptible when the poles are within the spark-distance from the coatings; three series of sparks are then obtained, of which two charge the jar and one discharges it. It is evident that charges and discharges are here only apparently simultaneous; actually, they follow one another at intervals too small to be recognized by the eye*.

A curious modification can be made in the experiment, by connecting the discharging wire not immediately with the coatings of the jar, but at any points of the wires which connect the jar with the poles. Let P and N in the adjoining figure be the poles of the induction coil PN, *a* and *b* be the coatings of the jar, and P*b* and N*a* the wires connecting the same with the poles; the discharging wires can be connected at any points *cc* of these latter wires; indeed they may proceed from the poles P and N themselves, and the intense discharge spark will be obtained at the point of interruption *dd*. But, of course, these sparks will be weaker the longer the paths *acd* and *bcd*, or, more correctly, the more resistance these paths oppose. If wires are here inserted which are equal in length or resistance to the induction wire itself, then all difference between the discharge sparks and the induction sparks vanishes. *Cæteris paribus*, the discharge sparks are stronger the more powerful the battery and the smaller the charged surfaces, as might be predicted from the former experiments of Riess with jars laden by the electrical machine.



With respect to the reason why the induction current fails permanently to charge a Leyden jar, even when the former acts only by means of sparks, I was at first inclined to believe that, in consequence of a transmitting action of the glass, a discharge took place; the phænomena with the glass plates coated on one side seemed favourable to this opinion. I have since convinced myself, however, that the cause is rather to be sought in the fact, that, under all circumstances, even when no second connexion is present, the jar is always discharged again by the induction wire itself. The proof of this lies in the fact, that the current of the induction wire always passes to and fro, even when its poles do not touch the coatings, but send sparks to them across a stratum of air; in the latter case, indeed, it is the current consequent upon breaking the inducing current which alone oscillates. If behind the Leyden jar or the coated glass plates, a galvanometer be introduced into the circuit, no deviation is obtained; whereas this never fails when the induction current is simply interrupted by a thickness of air across which sparks can pass*.

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* This observation was communicated by me to the Berlin Academy, on the 7th of December, after I had communicated it some weeks previous to a few friends. Since then the phænomenon has been described by Grove in the January Number of the Philosophical Magazine. During the publication of the present memoir, I have also learned that it was known to a few persons in Berlin as early as last summer.

This to-and-fro motion of the current is still more perceptibly and beautifully shown by means of the electric egg. So long as it is alone in the circuit, the knob connected with the negative pole is the only one which is surrounded by the well-known beautiful blue light; but as soon as the Leyden jar or the coated glass plates are also introduced into the circuit, both knobs appear blue, and when the vacuum is tolerably good, the positive flame is also visible above the blue light; not, of course, because two opposite currents simultaneously traverse the partial vacuum, but because they follow one another at such short intervals of time that the eye is no longer sensible of the changes†.

Insulating liquids, such as oil of turpentine, when placed between platinum plates and introduced into the circuit, produce the same effects as the Leyden jar.

The experiments with the egg are very instructive, especially when a second circuit also exists. When it is introduced into the primary circuit P*c* or N*c*, or into the branch *cc* at the point *dd*, only one knob is blue. When it is introduced in *ca* or *cb*, both knobs are covered with the blue light.

It is worthy of mention, that the jar *ab* has no influence upon the luminous phænomenon in the branch *cc*, provided the egg be well exhausted, and the induction current itself be sufficiently intense. If air is allowed to enter, however, or if the intensity of the current is diminished, by substituting a long for a short induction wire or by dispensing with the condenser, then the introduction of the jar at *ab* augments the luminous phænomenon at *dd*, though in a much less degree than with sparks in free air.

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* If, instead of the coated glass plates, an oiled silk condenser is introduced into the induction circuit, a continuous crackling is heard, but at the same time a strong deviation is obtained when a galvanometer is also introduced. The crackling in this case, therefore, is caused by an actual passage of electricity, by the breaking through of sparks. At first, when I employed a condenser whose coated surfaces were separated from one another by a double layer of paper, covered on each side with amber varnish, I obtained the phænomena of the glass plates.

† As Riess has already shown (Pogg. *Ann.* vol. xci. p. 290), the same optical illusion is obtained when, by means of the pin already described (Phil. Mag. vol. x. p. 6), the galvanic current is made to traverse the inducing wire alternately in opposite directions.

Mixed Cases.

By mixed cases I understand those wherein a branched communication exists between the poles of the induction wire, and where each branch presents one of the before-mentioned cases. We have just considered a combination of the second and third cases, wherein one branch was interrupted by air, and the other contained an insulating body. A combination of the first and third does not appear to promise much interest; consequently with two branches only, we have but to consider the combination of the first and second cases.

When *cc* is an unbroken metallic circuit, and instead of the jar a well-exhausted egg is introduced at *ab*, no light appears in the latter as long as the wire presents a less resistance. If the resistance be increased, however, either by using a longer or a thinner wire, the current divides itself between both circuits, and the egg becomes luminous. In general this division evidently takes place according to the same laws which hold for the galvanic current, and doubtless if the resistances in the primary and branch circuits, as well as the electromotive force of the induction were known, the intensity of the induction current in both branches could be expressed by the same or similar formulæ to those used in galvanism. The phænomena are here more complicated, merely because the induction apparatus furnishes two alternating currents of opposite directions, of which one has not the power of passing through the egg. Through the primary circuit P*c* or N*c* both currents pass alternately and with equal intensity; through *ca*, *bc* the current at breaking contact alone passes; and again, through *cc* both currents pass, but with unequal intensities, for the current at closing is greater than the current at breaking contact, because a part of the latter has passed along *ca*, *bc*. Hence, when the galvanometer is introduced into P*c* or N*c*, no deviation (or an ambiguous one when the current is sufficiently intense) is obtained; in *cb* or *ca* a deviation corresponding to the current at breaking contact is found; and in *cc* the deviation *opposite* to this, corresponding to the current at making contact. These phænomena were very distinct when one of the 10,000-feet wires was employed to generate the induction current, and the other to make the branch *cc*. They were also evident, though accompanied with far less light in the egg, when *cc* consisted of the wire 1200 feet long and 0·25 millim. thick.

Miscellaneous Phænomena.

Many very instructive experiments can be made with two or more instruments, of which the following may be mentioned.

The induction current can be excited in one apparatus and conducted through the coils of the other, in order to study the influence of increased resistance upon the sparks and luminous phænomena in general. For this purpose I conducted the current of the shorter of my induction wires through the 20,000 feet of wire in the other two coils. The sparks were smaller and less brilliant, but there was no perceptible diminution in the distance at which sparks could be obtained. The additional introduction of twelve beakers full of water, and connected by platinum arches, did not alter this result.

This corresponds perfectly with similar phænomena in frictional electricity, formerly studied by Riess. Nevertheless, as far as my own experiments are concerned, I am inclined to doubt whether the tension is the same at all points of the long and well-insulated wire circuit; for when I loosened one end of this circuit from the active induction coil, and by means of another wire, led it alongside the other end of the circuit near to the pole with which it was still connected, sparks were visible between them when the other pole was connected with the earth.

The instruments can be connected alongside one another, or one before the other, in order to study the influence of the thickness and length of the wire.

I had here an opportunity of testing an assertion of Masson's. This physicist states that when two induction instruments are placed alongside one another, but in opposite directions, and are thus connected with the electric egg, both electric lights, the blue glimmering light and the positive flame, are simultaneously seen upon each knob[*]. From this he arrives at the strange conclusion, that rarefied air possesses the property of simultaneously conducting two electric currents of opposite directions. I obtained a different and far more natural result, viz. that under the circumstances no light whatever is visible. Masson was probably led to his conclusion by allowing two complete instruments to work against one another, and it is more than probable that the strokes of the hammers of both instruments were not exactly isochronous and synchronous. The phænomenon observed by him would then be a natural consequence. Such could not be the case in my experiment, for the inducing wires of both coils formed, one after the other, a single circuit, which was broken by a single hammer.

In conclusion I will notice a combination, the experiments with which are calculated to furnish indications relative to the construction of such apparatus.

Two instruments, A and B, were so combined that the current traversed the two primary coils *one after the other*, the induction coils, however, were separate, and A was allowed to give sparks, whilst B remained unclosed, and without a soft iron core; the introduction of the latter into B *weakened* the sparks of A, and the subsequent metallic closing of B *reproduced* their former *strength*. From this it follows that two complete instruments, combined one after the other, mutually disturb one another, and the total effect is not equal to the sum of the effects which each would produce with the same intensity of galvanic current.

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[*] *Comptes Rendus*, vol. xxxvi. p. 256, and vol. xxxvii. p. 849.

Further, if the above experiment be modified so that the current traverses the primary coils of A and B placed *alongside* one another, then the insertion of the soft iron core into B *increases* the sparks of A, and the subsequent metallic closing of B again *destroys* this increase.

Hence, when many induction coils are connected *one after the other*, so as to form a single coil, it is advisable to allow the current to traverse the primary coils of these instruments *side by side*.

With respect to the cause of these phænomena, it is no doubt to be sought in the extra or inner induction current. The outer induction current, with which we are at present solely concerned and which is produced by breaking the inducing current, has the same direction as the latter; whereas the extra current, which is at the same time excited in the inducing circuit, developes a contrary current in the induction wire, whose tendency is to weaken the first induction current, and the more so the stronger the extra current, which latter is especially strengthened by the soft iron core.

Consequently, when the primary coils of A and B are connected one after the other, it is clear that the insertion of the soft iron core into that of B will strengthen the extra current in that of A, and therefore weaken the induction current of the latter instrument.

By connecting the instruments *side by side*, a closed circle is at once formed by them, and the extra current produced by the insertion of the soft iron core into B acts against that in A, and may therefore, when both are equal, completely neutralize it, when the outer induction current of A will be necessarily strengthened. Without the circle formed by this combination, *i. e.* in the wire leading back to the voltaic battery, the two extra currents act, of course, in the same direction and strengthen one another, in consequence of which the sparks at the current-breaker are still very active.

By the metallic closing of B, the action of its soft iron core only will be neutralized. Metallic coverings act similarly, though under certain circumstances they may also act in an opposite manner.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 67.]

April 26, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

THE following papers were read:—

"Some Observations on the Ova of the Salmon, in relation to the distribution of Species; in a letter addressed to Charles Darwin, Esq., M.A., V.P.R.S. &c." By John Davy, M.D., F.R.SS. Lond. & Edinb.

In this paper the author describes a series of experiments on the ova of the Salmon, made with the intent of ascertaining their power of endurance under a variety of circumstances without loss of life, with the expectation suggested by Mr. Darwin, that the results might possibly throw some light on the geographical distribution of fishes.

The details of the experiments are given in five sections. The results obtained were the following:—

1. That the ova of the Salmon in their advanced stage can be exposed only for a short time to the air if dry, at ordinary temperatures, without loss of life; but for a considerable time, if the temperature be low, and if the air be moist; the limit in the former case not having exceeded an hour, whilst in the latter it has exceeded many hours.

2. That the vitality of the ova was as well preserved in air saturated with moisture, as it would have been had they been in water.

3. That the ova may be included in ice without loss of vitality, provided the temperature is not so low as to freeze them.

4. That the ova, and also the fry recently produced, can bear for some time a temperature of about 80° or 82° in water, without materially suffering; but not without loss of life, if raised above 84° or 85°.

5. That the ova and young fry are speedily killed by a solution of common salt nearly of the specific gravity of sea-water, viz. 1026; and also by a weaker solution of specific gravity 1016.

Finally, in reference to the inquiry regarding the distribution of the species of fishes, he expresses his belief that some of the results may be of useful application, especially those given in the second and third sections; inferring, that as in moist air, the vitality of the ova is capable of being long sustained, they may during rain or fog be conveyed from one river or lake to another adhering to some part of an animal, such as a Heron or Otter, and also during a time of snow or frost; and, further, that other of the results may be useful towards determining the fittest age of ova for transport for the purpose of stocking rivers, and likewise as a help to explain the habitats, and some of the habits of the migratory species.

"Observations on the Anatomy and Affinities of the *Phyllirrhoë bucephala* (Peron)." By John Denis Macdonald, Esq., R.N., Assistant-Surgeon of H.M.S.V. 'Torch.'

As the true position of Peron's genus *Phyllirrhoë*, and even the very existence of the animals composing it, have been matters of doubt to zoologists, during a late cruise to the Fiji Islands I determined to ply the towing-net with a little more diligence than usual, hoping to obtain a few of these almost hypothetical beings, and was rewarded by the capture of many specimens.

Some were taken in the neighbourhood of Lord Howe's Island, S. lat. 31° 31″, E. long. 159° 5″, some near Norfolk Island, S. lat. 29° 2″, E. long. 168° 2″, and others, although in smaller numbers, in different parts of our track. They generally made their appearance after dusk in the evening, and presented a great diversity in size, form and other external characters, which is due to changes in the muscular system, a variable amount of pigment-spots, &c. Indeed at first I fully believed that several distinct species had been brought up together, but this idea was abandoned when I observed the most dissimilar forms gradually assume so close a resemblance to each other, as ultimately to render it difficult to distinguish them.

From these facts I am much inclined to think that the three species described by Quoy and Gaimard, viz. *P. amboinensis*, *P. punctulata* and *P. rubra*, *P. Lichtensteinii* (*Eurydice Lichtensteinii* of Eschscholtz) and *P. rosea* of D'Orbigny, are all referable to Peron's original species *P. bucephala*.

The body of *Phyllirrhoë* is elongated in form and compressed laterally, presenting for description an anterior and posterior extremity, a right and left surface, and a dorsal and ventral border. The head is surmounted by two lengthy, somewhat flattened and acuminate tentacula; the eyes lie beneath the skin, not being visible externally, and the mouth is in the form of a short truncated proboscis, with a vertical opening. The oval-shaped body is on an average about one inch and a half in length, which is something over twice the measurement from the dorsal to the ventral border taken at the middle or broadest part. The tail is quadrilateral in figure, gradually widening towards its posterior border, which is exceedingly thin. The outer integument is perfectly transparent and lined by muscular bundles, disposed longitudinally, and somewhat more than their own breadth apart. These communicate with one another by oblique branching slips, which thus form a kind of network enclosing long lozenge-shaped spaces. Here and there nerve-trunks of considerable size accompany the longitudinal bundles, dividing off into smaller twigs, which distribute themselves at pretty equal distances in a direction more or less perpendicular to that of the muscular fibres. Scattered about at irregular intervals amongst these structures are numerous reddish-brown pigment-spots, in the centre of each of which a clear vesicle is generally distinguishable. As above alluded to, the actual tint of this pigment, and the relative number of spots deposited within a certain space, determine both the general quality and the depth of colour which are found to vary so much in different specimens of *Phyllirrhoë*.

The alimentary canal of this creature consists of a muscular tube lined with mucous membrane, extending without flexure from the mouth to the vent. It commences anteriorly in an oral dilatation, in connexion with which we notice a pair of lateral horny jaws articulated with each other superiorly, and beset with very minute and sharp-pointed teeth along the cutting edge, altogether much resembling those of *Glaucus*, and a lingual ribbon gradually increasing in diameter from before backwards, and supporting a pavement of long, conical, flattened and gracefully curved teeth with fine denticulations at the base. The central series of plates being symmetrical, the large tooth in each takes up a middle position, but in the lateral plates it inclines to the inner side. In some examples I have observed certain lobulated bodies lying in contact with the buccal mass, and which I am disposed to regard as salivary glands. The œsophagus is short, and suddenly expands into a moderately large stomach; and the latter, having received the biliary ducts near its posterior extremity, is continued into the rectum, which passes directly backwards some little distance, and ends in the anus, on the right side of the body, at the union of its posterior and middle thirds. The liver in *Phyllirrhoë* consists of four elongated, tubular, and sacculated portions or lobes, disposed along the borders of the body, two lying above and two below the alimentary canal. Each of the superior hepatic glands opens by a distinct duct into the superoposterior part of the stomach, while the ducts of the inferior ones unite to form a common tube joining it at its infero-posterior part. The opposite or cæcal extremities of the two anterior hepatic lobes end in the neighbourhood of the head, while those of the others extend to within a short distance of the tail. The secreting cells of these organs are of a rounded or polyhedral form, containing, besides the nucleus, a reddish-brown pigment and fatty globules.

Phyllirrhoë possesses a simple systemic heart, consisting of a single auricle and ventricle. This organ lies upon the stomach, between the ducts of the two superior biliary glands; and a large vessel or sinus, with many circular constrictions in its walls, may be traced towards the auricle, bringing back the aërated blood from the hinder extremity of the body. There are no visible respiratory organs, but it is probable that the cutaneous surface permits of the necessary exposure of the blood to the air contained in the surrounding medium.

The nervous system is well developed. The supra- and subœsophageal ganglia, with their commissural chords, form a close ring round the gullet immediately behind the buccal mass. The auditory sacs, which are filled with vibratory otokonia, appear to lie between both sets of ganglia, and the rudimentary visual organs, consisting each of a simple cell containing a refracting globule imbedded in black pigment, are also in contact with the nervous matter. Besides the actual distribution of the nerves given off from the cephalic ganglia, I noticed nodules of neurine lying at the base of the tentacula, communicating by commissural threads, and sending off each a principal nerve to the corresponding tentacle. The ganglion-globules were lined with a reddish-coloured pigment, deposited round the vesicular nuclei, and when twigs are given off from the smaller nerves, both the homogeneous neurilemma and the contained nervous matter break up like a dividing vessel, without preserving the individuality of distinct nerve-tubes.

The sexes are combined in *Phyllirrhoë*, the male and female generative openings lying close together on the right side of the body in the inferior gastero-hepatic space, and before the anal aperture. The ovaries lie in the inferior recto-hepatic space, varying in number from two to five, in general. They are dark-coloured, subrotund, and finely lobulated bodies, from the fore part of each of which a very delicate duct arises, and all the ducts unite to form a single tube, with a trifling increase in its diameter. This common oviduct, lined by a pavement of transparent epithelial cells, passes forwards beneath the stomach in a flexuous manner; and in the inferior gastero-hepatic space, it first unites with the duct of the testis and again continues its devious course until it ends in the fundus of a much larger tube, whose lining membrane is armed with numerous conical and tooth-like processes, and to this is appended a long cæcal process much resembling the spermatheca of *Helix* for example. The external orifice of the male generative apparatus lies immediately posterior to that of the female organs. The testis is rather small, subglobular in form, and closely connected with a short twisted tube*, much dilated at the middle part, and coated over with a layer of dark pigment-cells. It is with this tube, as above noticed, the small oviduct communicates, in order, as it would seem, to permit of self-impregnation, or to answer some other purpose, with the nature of which we are unacquainted; but there is also an intromittent organ, which, however, I have never seen properly exserted.

As to the affinities of *Phyllirrhoë* with Gasteropods, it may be observed that the animal is bisexual, that the eyes, like those of *Glaucus* and *Ianthina*, are very small and rudimentary, being closely applied to the ganglia of the brain, after the manner of the acoustic sacs, and that both *Phyllirrhoë* and *Glaucus* agree in possessing two lateral horny jaws, articulated with each other superiorly, and bordered with minute conical teeth.

In the *Glaucidæ*, the branchiæ, which consist of simple papillary projections of the skin, are distributed in an equable manner over the dorsal region of the body; and any deviation from this arrangement would naturally tend, either to a more definite localization, or still further dispersion. It is the latter modification which appears to have taken place in *Phyllirrhoë*; so that its respiratory vessels ramify minutely through the common integument, just as the vascular trunks analogous to those which break up in the pectinate gill, adapted for aquatic breathing, are subdivided, and spread themselves over the smooth walls of the lung-chamber in Pulmonifera.

As respects its affinity to the Pteropods, here too the lateral jaws of *Phyllirrhoë* must be borne in mind, together with the almost complete suppression of the organs of vision. It is worthy of note also, that its acoustic capsules contain otokonia, as in Pteropoda, instead of single globular otolithes like those of *Glaucus*, and there is some reason to believe that the long tentacula, so called, are the homologues of the cephalic fins of Pteropods.

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* I have distinctly traced the homologue of this tube in *Pteropoda*, *Heteropoda*, and the *Gasteropoda* proper.

The particular features of *Phyllirrhoë*, expressed in the last paragraph, also serve to distinguish it from the Heteropoda, but it somewhat approximates this order in the general conformation of its body, which is elongated, laterally compressed, and presents a kind of proboscis at the anterior, and a rudder-fin at the posterior extremity. There is also, as it would appear to be, a small remnant of the foot on the inferior thin margin of the body, and the lateral undulatory motion of the animal in the water exactly resembles that of *Cerophora*, or *Carinaria*.

The heart of *Phyllirrhoë*, in common with that of Heteropods in general, holds a dorsal position. The auricle lies posterior to the ventricle, as in *Cerophora* and *Firola*, but the reverse is the case in *Atlanta* and *Carinaria*, the difference being due to the relation which the respiratory surface bears to the heart itself, lying in every case on the auricular side. Moreover it is remarkable that the rectum is directed backwards in the former instances, but turns forwards in the latter, taking an opposite course to that of the circulation through the heart.

It may be observed in conclusion, that in Heteropoda the viscera are closely packed together so as to occupy the smallest possible space, while they are widely distributed through the abdomen in *Phyllirrhoë*; thus, again, calling to mind its relationship to the Pteropoda.

This paper is illustrated with drawings representing the animal described and some of the details of its internal structure.

"Brief sketch of the Anatomy of a new genus of pelagic Gasteropoda, named *Jasonilla*." By John Denis Macdonald, Esq., R.N.

This communication refers to a remarkable genus of pelagic Gasteropoda, characterized, like *Macgillivraya* and *Cheletropis*, by the presence of ciliated cephalic appendages, but having, as in the present instance, a beautifully transparent, cartilaginous and perfectly symmetrical shell. The author has seen but one species, which was frequently taken between Port Jackson and the Isle of Pines.

The shell resembles that of *Argonauta* in shape, is less than one-eighth of an inch in diameter, and the little animal, when fully retracted, occupies but a small portion of its cavity. The margin of the mantle is of considerable thickness, containing loosely-packed cells, similar to those of the middle or operculigerous lobe of many Pteropods. About eight ciliated arms, identical in character with those of *Macgillivraya*, &c., encircle the head, including the mouth, which is furnished with two massive lateral jaws bearing sharp prominent dental processes on the anterior border, and with a pair of simple tentacula having a dark ocellus at the outer side of the base of each. A well-formed foot arises by a narrow pedicle from the under surface of the body, immediately behind the ciliated collar. The creeping disc is elongated in form, subquadrate in front, and tapers off gradually towards the posterior extremity. The latter part, corresponding to the operculigerous lobe of other species, is speckled with little clusters of dark pigment-cells, disposed so much after the manner of those of the ciliated arms as to lead to the impression that it is one of the same series, or whorl of organs, to use botanical phraseology. A pectinate gill extends beneath the mantle, along the anterior third of the dorsal region, lying, as in most cases, in advance of the heart. The visceral mass of the body, though elongated, is but slightly curved upon itself, not exceeding half a turn. The lobules of the liver, distended with large amber-coloured oil-globules, may be distinctly seen through the transparent outer envelope and shell. Single spherical otolithes are contained in the acoustic sacs, and the lingual ribbon is lengthy and flexuous, presenting a row of uncini on each side, with a series of minute denticulations, pointing backwards on their anterior and posterior borders. The uncini of opposite sides interlock with one another so closely as to conceal the rudimentary segments of the rachis almost completely. The shell is cartilaginous, transparent, planorbicular, and perfectly symmetrical, presenting four rows of minute conical tuberculations on its convex or dorsal surface. The gyri of the involute nucleus are so curved as to leave a central perforation; the mouth of the tube encroaches considerably on the last whorl, and the outer lip is deeply notched between the two lateral rows of tubercles. The author has named the species *Jasonilla M'Leayiana.* The paper is accompanied with illustrative figures.

Note "On the position of Aluminum in the Voltaic series." By Charles Wheatstone, Esq., F.R.S.

Having, through the kindness of Dr. Hofmann, been permitted to examine a specimen of aluminum prepared by M. Claire-Deville, I availed myself of the opportunity to ascertain one of the physical properties of this extraordinary metal, which it does not appear has been yet determined, viz. its order in the voltaic series. The following are the results of my experiments.

Solution of potass acts more energetically and with a greater evolution of hydrogen gas upon aluminum than it does on zinc, cadmium or tin. In this liquid aluminum is negative to zinc, and positive to cadmium, tin, lead, iron, copper and platina. Employed as the positive metal, the most steady and energetic current is obtained when it is opposed to copper as the negative metal; all the other metals negative to it which were tried became rapidly polarized, whether above or below copper in the series.

In a solution of hydrochloric acid aluminum is negative to zinc and cadmium, and positive to all the other metals above named. With this liquid also copper opposed to it as the negative metal gave the strongest and most constant current.

Nitric and sulphuric acids are known not to act chemically in any sensible manner on aluminum. With the former acid diluted as the exciting liquid aluminum is negative to zinc, cadmium, tin, lead and iron. The current with zinc is strong; with the other metals very weak, and it is probable that their apparent negative condition is the result of polarization. When aluminum is immersed in dilute sulphuric acid, this metal appears negative to zinc, cadmium, tin and iron, but with lead, on which sulphuric acid has no action, the current is insensible. In both these liquids copper and platina are negative to aluminum, and notwithstanding the apparent absence of chemical action on the latter metal, weak currents are produced.

It is rather remarkable, that a metal, the atomic number of which is so small, and the specific gravity of which is so low, should occupy such a position in the electromotive scale as to be more negative than zinc in the series.

May 3.—Charles Wheatstone, Esq., V.P., in the Chair.

"An Experimental Inquiry into the nature of the metamorphosis of Saccharine Matter, as a normal process of the animal œconomy." By Dr. Pavy.

The author begins by observing, that the saccharine matter met with in the animal œconomy is derived from two sources—from the vegetable kingdom, and from the liver of the animal itself; in each case being poured into the general circulation through the hepatic veins. The liver not only enjoys the power of forming sugar, but it likewise exerts (as shown by the experiments of Bernard) some modifying influence over that which is traversing its capillaries and which has been absorbed from the food, by which it is transformed from *vegetable* into *animal* sugar, and thus rendered more apt for serving in the processes of animal life.

The sugar poured into the general circulation through the hepatic veins is conveyed to the capillaries of the lungs, where it in great part disappears, but never entirely so, according to very numerous analyses which the author has made on this subject. If the blood be traced onwards from the arteries through the systemic capillaries into the veins, the small amount of sugar which impregnates arterial blood will be found to be still undergoing a process of destruction; and what appears exceedingly interesting, this process of destruction is not carried on with equal activity in the different parts of the system at large. In the capillaries of the chylo-poietic viscera, the destruction is so complete, that the blood in the portal vein may be entirely free from saccharine principle, when the blood returning from other parts, as that contained in the femoral or jugular veins, remains slightly impregnated. This curious fact has a bearing that will be presently adverted to, with reference to the views to be advanced concerning the nature of the metamorphosis of sugar in the animal œconomy.

The *principal* seat of destruction of saccharine matter in the animal system being located in the respiratory organs, seems at first sight to support the theory of Liebig—that sugar is one of those substances which undergoes a process of combustion, by its direct combination with oxygen and its resolution into water and carbonic acid. Some experiments on the temporary obstruction of the respiration and the examination of arterial blood before and after the operation, led the author to call in question this view, as he observed that notwithstanding the supply of oxygen was cut off to such an extent as almost to occasion death, yet a considerable destruction of sugar took place in the lungs. This, coupled with the fact that a disappearance of sugar takes place in the systemic capillaries, and unequally so in different portions of them, induced him to push his investigations, and see if there might not be some other cause in operation in the living animal to effect the normal destruction of sugar, besides the direct chemical action of the oxygen absorbed in respiration. The results of these investigations, which were first directed towards the changes produced in blood normally containing sugar, injected through the capillaries of lungs removed from the animal, and artificially inflated with atmospheric air or oxygen gas, have induced the author to refer the metamorphosis of sugar in the animal œconomy, to a process which is perfectly consistent and analogous with the well-known chemical bearings of this substance apart from the animal system.

In experiments which the author has now several times repeated, he injected blood removed from the right side of the heart of an animal—and therefore normally containing sugar—through the capillaries of the artificially inflated lungs of another; and found that as long as the blood retains its fibrine, there is as much destruction of its sugar as would take place in the living animal; but that where the fibrine has been separated from the serum and corpuscles, the sugar ceases to be influenced by the presence of oxygen, or ceases to disappear during this process of artificial respiration. It would hence appear, that something besides mere contact with oxygen is requisite for the destruction of sugar. But in other experiments, he has found that oxygen is nevertheless a necessary agent concerned in the process of transformation observed during the arterialization of the blood that has not undergone spontaneous coagulation. It would therefore seem, in fact, that oxygen acts secondarily on the sugar through the medium of the fibrinous constituent of the blood:—that it exerts some changes upon this azotized principle, which are capable of inducing the metamorphosis of sugar.

If we look to the ordinary chemical bearings of saccharine matter apart from the animal system, we find that an azotized substance undergoing the molecular changes of decomposition, placed in contact with sugar readily excites a process of fermentation, and converts it by a mere alteration of the grouping of its elements into another substance, one atom of sugar ($C^{12} H^{12} O^{12}$) being resolved into two atoms of lactic acid ($C^6 H^6 O^6$). We also find that sugar is not susceptible of oxidation except under the influence of strong chemical reagents. Chemical analogy, therefore, would lead us to look upon the secondary action of oxygen as the more probable process of physiological destruction; especially when we take into consideration, that nowhere do we meet with such a constant series of molecular changes taking place as amongst the azotized constituents of a living animal. In the above-mentioned experiment of injecting fibrinated and defibrinated blood through an artificially inflated lung, when the blood is capable of undergoing the molecular changes of assimilation on contact with oxygen as in the living animal, the sugar in great part disappears, but so soon as the fibrine is separated by spontaneous coagulation, and the blood has thus lost its vital characteristics, oxygen is no longer capable of exerting any metamorphosing influence on its saccharine ingredients.

If the molecular changes occurring during the decomposition of an azotized substance be capable of converting sugar into lactic acid, why should not the molecular changes occurring during the building-up or elaboration of this same nitrogenized compound effect the same? Indeed, we have seen that the process of destruction is carried on to a certain extent in the systemic capillaries, and more especially in those of the chylo-poietic viscera, where the molecular changes of nutrition are also correspondingly carried on with greater activity than elsewhere. So that analogy and experiment would tend to show that the physiological destruction of sugar is owing to a process similar to fermentation induced by the molecular changes occurring in the nitrogenized constituents of the animal during life. And, in accordance with this, we find lactic acid present in the system, and largely separated from arterial blood by the muscular tissue, and the secerning follicles of the stomach.

As regards the lactic acid fermentation, it is well known that the presence of an alkali favours, whilst that of an acid retards the process. In two experiments on animals, the author injected carbonate of soda and phosphoric acid into the circulating current, and observed in the case of the latter that sugar immediately accumulated in the blood.

The preceding observations refer more especially to the changes that take place in the saccharine ingredient of the blood during life; and the author next proceeds to notice some interesting phænomena observable during the decomposition, and even the spontaneous coagulation of blood containing sugar.

If the blood of an animal normally impregnated with sugar be placed aside, and allowed to undergo spontaneous coagulation, on examining separately the serum and clot on the following day, it will be found, that although the serum may be largely saturated with sugar, the clot is entirely, or almost entirely destitute of it. Now, as the clot is moist and remains to a certain extent infiltrated with the serum from which it has partially separated, it would appear that even the molecular changes arising from the spontaneous coagulation of the blood are sufficient to effect the destruction of normal animal sugar. And this conclusion is strengthened by the fact, that in diabetic blood (the sugar of which, as would appear from other considerations also, is not so susceptible of metamorphosis as the healthy variety) the sugar does not disappear to a similar extent in the clot.

Under the changes of the decomposition of blood, normal animal glucose is very readily metamorphosed. The rapidity of the metamorphosis depends on the activity of the decomposition of the animal substances present, and when the destruction of the sugar is complete the blood has assumed an *acid reaction*.

This acid reaction of decomposing blood is only observable in that which was previously pretty largely impregnated with sugar. It appears to be owing to the formation of lactic acid. Certainly, it cannot be due to carbonic acid, for the reaction remains after exposure to a boiling temperature.

The disappearance of sugar in the manner just pointed out does not depend on the oxygen of the air, except in so far as this agent is concerned in exciting the decomposition of the azotized constituents of the blood; for the sugar disappears as rapidly when there is a small, as when there is a large amount of surface exposed to the air. But if the air be carefully and completely excluded, no signs of decomposition of the animal parts of the blood are to be observed, and under these circumstances the sugar also remains. The disappearance of sugar is more rapid where the fibrine and corpuscles are present, than when the serum is exposed alone; and in accordance with this, the blood in the one case undergoes decomposition much sooner than in the other—a fact easily intelligible from the greater amount of azotized ingredients present.

If blood normally impregnated with saccharine matter be placed aside until signs of incipient decomposition are observed, and the sugar is beginning to disappear, exposure to a current of oxygen rapidly completes the total disappearance of the saccharine constituent. In this observation we have a further illustration of the analogy that appears to exist, in the nature of the metamorphosis of sugar as a physiological process, and that which takes place chemically under the influence of an azotized compound, whose elementary particles are in a state of molecular transition. During life, the higher organic constituents of the blood are capable of undergoing the changes of assimilation on exposure to contact with oxygen, and there is a considerable destruction of sugar effected; for a short period after death these azotized constituents remain stationary and uninfluenced by oxygen, and with this, there is a corresponding suspension of the transformation of sugar; but finally, the animal matter of the blood on contact with oxygen, especially during a warm temperature, assumes a state of decomposition, the molecular changes of which again excite the destruction or metamorphosis of saccharine matter.

The sugar *disappears far less rapidly* from diabetic blood under the influence of exposure to the atmosphere, than from healthy right-ventricular blood. From these, and a few other observations which he has as yet been able to make on the blood in Diabetes Mellitus, the author, were he to hazard an opinion on the nature of that obscure disease, would be disposed to say that there appears to be a modification of sugar produced by the liver, which is not susceptible of undergoing the normal process of destruction in the animal system, and which, therefore, accumulating in the blood, is eliminated by the kidneys. The experiments of Bernard have shown that vegetable glucose (grape-sugar) is not susceptible of destruction in the processes of animal life, unless converted into animal glucose by the agency of the liver. Diabetic sugar would therefore seem to bear a resemblance in its physiological relations to vegetable, rather than to animal glucose.

The following communications were in part read:—

"Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S.

The author discusses the following problem:—"To find in how many ways a number q can be made up of the elements a, b, c.., each element being repeatable an indefinite number of times." The solution depends upon a peculiar decomposition of an algebraical fraction $\frac{\phi x}{fx}$, where the denominator fx is the product of any number of factors, the same or different of the form $1-x^m$, and upon the expansion by means thereof of the fraction in ascending powers of x. The coefficient of the general term is expressed in terms of circulating functions, such that the sums of certain groups of the coefficients are severally equal to zero; these functions the author calls prime circulators. The investigations show the general form of the analytical expression for the number of partitions, and they also indicate how the values of the coefficients of the prime circulators entering into such expression are to be determined.

"Further Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S.

The memoir contains a discussion of the problem "to find in how many ways a number q can be made up as a sum of m terms with the elements $0, 1, 2, \ldots . k$, each element being repeatable an indefinite number of times." The number q may without loss of generality be taken to be equal to $\frac{1}{2}(km-\alpha)$, and the expression for the number of partitions of this number $\frac{1}{2}(km-\alpha)$ is by a peculiar method reduced to the form coeff. x^m in $\frac{\phi x}{fx}$, where $\frac{\phi x}{fx}$ is an algebraical fraction, the form of which depends on the value of k, *but which does in anywise involve the number m*; the denominator fx is the product of factors of the form $1-x^\theta$, and up to certain limiting values of α the fraction is a proper fraction. The author remarks in conclusion that the researches were made for the sake of their application to the theory developed in his "Second Memoir upon Quantics."

May 10.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"An Experimental Inquiry undertaken with the view of ascertaining whether any *force* is evolved during Muscular Contraction analogous to the *force* evolved in the Fish, Gymnotus, and Torpedo." By Henry Foster Baxter, Esq.*

"On a simple Geometrical construction, giving a very approximate Quadrature of the Circle." By C. M. Willich, Esq.

Let AB be a quadrant of a circle A, B, C. In the arc BC place a chord BD equal to the radius, so that the arc BD is one of 60°.

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* This communication will appear in our next Number.

Bisect BD in E; join AE, and produce the joining line to meet the circumference in F. Then AF differs from the side of a square equal in area to the circle by somewhat less than the one four-thousandth part of that side.

May 24.—The Lord Wrottesley, President, in the Chair.

The following communication was read :—

"A Second Memoir upon Quantics." By Arthur Cayley, Esq. F.R.S.

The memoir is intended as a continuation of the author's introductory memoir upon Quantics (vide Phil. Mag. vol. viii. p. 69, and Phil. Trans. 1854, p. 245); the special subject of the memoir is the theorem referred to in the postscript of the introductory memoir, and the numerous developments arising thereout in relation to the number and form of the covariants of a binary quantic. The author, after some remarks as to the asyzygetic integrals and the irreducible integrals of a system of partial differential equations, and after noticing that the number of irreducible integrals is in general infinite, proceeds to establish the above-mentioned theorem, viz. that a function of any order and degree satisfying the necessary condition as to weight, and such that it is reduced to zero by one of the operations $\{x dy\}-x dy$ and $\{y dx\}-y dx$, is reduced to zero by the other of the two operations, *i. e.* that it is a covariant; and he shows how by means of the theorem the actual calculation of the covariants is to be effected. The theorem gives at once (in terms of symbols P, P′, which denote a number of partitions) expressions for the number of the asyzygetic covariants of a given degree and order, or of a given degree only, of a quantic of any order; this enables the discussion of particular cases, but to obtain more general results it is necessary to transform the expressions for the numbers of partitions by the method explained in the author's "Further Researches on the Partition of Numbers." It appears by the resulting formulæ that the number of the irreducible invariants or covariants does in fact become infinite for quantics of an order sufficiently high; the number of the irreducible invariants first becomes infinite in the case of a quantic of the order 7; the number of irreducible covariants first becomes infinite in the case of a quantic of the order 5. In particular, the formulæ show that in the case of a quantic of the order 5, or quintic, there are 4 irreducible invariants of the degrees 4, 8, 12 and 18, respectively connected by an equation of the degree 36; and that in the case of a quantic of the order 6, or sextic, there are 5 irreducible invariants of the degrees 2, 4, 6, 10 and 15 respectively connected by an equation of the degree 30; so that the system of the irreducible invariants of a sextic is analogous to that of the irreducible invariants of a quintic. The memoir concludes with a table of the covariants of a quadric, a cubic, and a quartic, and of certain of the covariants of a quintic.

XIX. *Intelligence and Miscellaneous Articles.*

SOUNDING RAPID CURRENTS.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

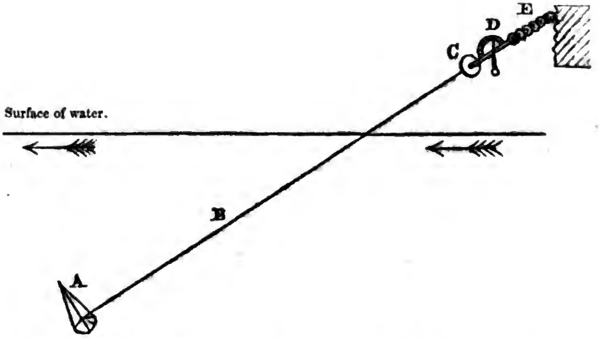
Seeing an account, which is making the tour of the newspapers, of an attempt to sound the depths of Niagara, which failed in consequence of the great velocity of the stream not allowing the plumb to sink, it struck me that that which could not be accomplished by main force might be done by a very simple contrivance.

The principle on which a schoolboy's common kite mounts in the air, overcoming the law of gravity, is well known; and I should think that an apparatus constructed on the same principle, and perhaps of a like form but much smaller, and strongly made of iron, would by the action of the current of water be forced to the bottom of the river. All boys know that the stronger the breeze, the higher the kite flies. The depth might be easily determined by the angle of the detaining chain or wire with the surface of the water. I merely throw out the hint, hoping it may perhaps lead to the invention of some apparatus that will overcome the difficulty.

I enclose a rough diagram, and remain, Sirs,

Your obedient Servant,

W. Sowerby.



A. Kite, strongly made of thick sheet iron.
B. Detaining chain or wire.
C. Reel fixed at the end of bar carrying the scale D, with a pointing needle weighted to keep perpendicular.
E. Chain and hook to attach the apparatus to the shore or bridge.

ON A FALL OF METEORIC STONES AT BREMEWORDE IN HANOVER.

BY M. WÖHLER.

On the 13th of May last, at 5 o'clock in the afternoon, a very remarkable fall of meteoric stones was observed near Bremeworde, not far from Hamburg. This phænomenon was accompanied by thunder and a great hissing noise. The sky was cloudy, so that the meteor was not seen; but three stones were seen falling, and these were found. It is very probable that a greater number fell. The largest of these stones weighs 3 kilogrammes, the second 1½ kilogramme, and the third 325 grammes.

Like most of the aërolites observed, these stones are covered with a black fused crust. Their fracture shows a mixture of several minerals of a gray colour, amongst which a considerable quantity of metallic iron and sulphuret of iron may be distinguished. They present a close resemblance with those which fell in Transylvania on the 4th of September 1852, and which form part of the collection in the Imperial Museum of Vienna.—*Comptes Rendus*, June 25, 1855, p. 1362.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1855.

Chiswick.—June 1. Overcast. 2. Cloudy and fine: very clear. 3. Fine: cloudy: overcast. 4. Heavy clouds: very fine. 5. Cloudy: fine. 6. Hot and dry: rain. 7. Very fine. 8. Cloudy: very fine. 9. Showery. 10, 11. Very fine. 12. Very fine: clear at night. 13. Slight haze. 14. Overcast: rain. 15. Cloudy: fine: rain. 16. Fine: showery: overcast: heavy rain. 17. Cloudy: slight showers. 18. Clear: cloudy: rain. 19. Overcast and fine. 20. Light clouds: fine: clear: *frosty at night*. 21. Clear and fine. 22. Fine: very dry air. 23. Overcast. 24. Very fine. 25. Cloudy and fine. 26. Overcast: very fine. 27. Dry haze: very fine. 28—30. Very fine.

Mean temperature of the month	57°·98
Mean temperature of June 1854	56 ·93
Mean temperature of June for the last twenty-nine years	60 ·39
Average amount of rain in June	1·89 inch.

Boston.—June 1. Cloudy: rain A.M. 2. Cloudy. 3, 4. Fine. 5. Cloudy. 6. Fine. 7. Cloudy: rain A.M. 8. Cloudy. 9. Fine: rain P.M. 10. Fine: rain with thunder P.M. 11. Fine. 12, 13. Cloudy. 14. Rain A.M. and P.M. 15. Cloudy: rain A.M. and P.M. 16. Fine. 17, 18. Cloudy: rain P.M. 19, 20. Cloudy. 21, 22. Fine. 23—27. Cloudy. 28—30. Fine.

Sandwick Manse, Orkney.—June 1. Bright A.M.: rain P.M. 2. Damp A.M.: foggy P.M. 3. Hazy A.M.: cloudy P.M. 4. Hazy A.M.: clear P.M. 5. Bright A.M.: clear P.M. 6. Rain, thunder A.M.: drops P.M. 7. Foggy A.M.: cloudy P.M. 8. Bright, fine A.M.: foggy P.M. 9. Showers A.M.: showers, foggy P.M. 10. Cloudy A.M.: clear P.M. 11. Bright A.M.: cloudy P.M. 12. Rain A.M. and P.M. 13. Foggy A.M. and P.M. 14. Clear A.M. and P.M. 15. Cloudy A.M.: clear P.M. 16. Showers A.M.: drops P.M. 17. Drizzle, showers A.M. and P.M. 18. Bright A.M.: drizzle, showers P.M. 19. Clear A.M. and P.M. 20. Cloudy A.M.: small rain P.M. 21. Foggy A.M. and P.M. 22. Foggy A.M.: bright P.M. 23. Bright A.M. and P.M. 24. Bright A.M.: bright, rain P.M. 25. Rain A.M. and P.M. 26. Drops A.M.: rain P.M. 27. Bright A.M.: clear P.M. 28. Clear, fine A.M.: bright, fine P.M. 29, 30. Bright, fine A.M. and P.M.

Mean temperature of June for twenty-eight previous years	52°·78
Mean temperature of this month	52 ·23
Mean temperature of June 1854	52 ·86
Average quantity of rain in June for fifteen previous years	2·21 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. June.	Max.	Min.		9¾ a.m.	8½ p.m.	Max.	Min.		9¾ a.m.	8½ p.m.						
1.	29·924	29·718	29·36	30·12	30·03	55	36	52	51½	46	sw.	s.	ne.	....	·75	....
2.	30·002	29·918	29·55	29·93	29·87	61	35	59	49	46	sw.	ssw.	se.	....	....	·08
3.	29·917	29·874	29·48	29·75	29·68	67	51	58	51	49	sw.	ssw.	sse.	·02	....	....
4.	29·870	29·859	29·38	29·62	29·55	69	50	62	52	49½	sw.	ssw.	se.	....	....	....
5.	29·893	29·876	29·38	29·55	29·64	76	54	60	58½	50½	sw.	s.	sse.	....	....	....
6.	29·809	29·693	29·36	29·72	29·81	86	53	66	53	52	s.	sse.	ese.	·16	....	·09
☾ 7.	29·881	29·826	29·34	29·70	29·79	71	47	60	55	53½	sw.	se.	......	....	·19	·09
8.	29·941	29·929	29·45	29·85	29·83	72	46	63	57	48½	sw.	ssw.	e.	....	....	....
9.	30·019	29·991	29·50	29·79	29·83	66	42	63	51½	49½	sw.	ssw.	nw.	·08	....	·05
10.	30·227	30·191	29·70	30·01	30·12	73	41	58	55½	51	ne.	sw.	wnw.	....	·04	·16
11.	30·195	30·189	29·78	30·16	30·09	72	46	60	55	52	e.	e.	wsw.	....	·20	....
12.	30·107	29·856	29·70	29·95	29·82	70	54	60	51½	50	ne.	e.	sw.	....	....	·30
13.	29·686	29·435	29·30	29·70	29·57	74	51	63	56	50	e.	e.	nw.	·27	....	·47
● 14.	29·517	29·420	28·98	29·40	29·32	65	51	51	53½	50½	sw.	nw.	ne.	·18	·46	·10
15.	29·346	29·280	28·80	29·26	29·39	65	42	61	51	47	sw.	w.	nne.	·11	·30	....
16.	29·434	29·328	28·40	29·52	29·69	65	46	60·5	50½	46½	s.	sw.	n.	·46	·30	....
17.	29·906	29·698	28·30	29·82	29·88	59	36	47	48½	44	nw.	n.	nw.	·02	·02	·02
18.	30·095	30·017	29·60	29·85	29·98	58	50	53	47½	42½	w.	w.	nw.	·18	·02	·06
19.	30·223	30·061	29·70	30·07	30·24	61	36	52	50	46	n.	nne.	se.	....	·06	....
20.	30·366	30·227	29·98	30·27	30·24	56	30	53	55½	55	n.	nne.	wsw.	....	....	....
21.	30·335	30·181	29·94	30·28	30·31	69	38	52	55	54	ne.	nne.	s.	....	....	·07
☽ 22.	30·252	30·104	29·83	30·20	30·17	74	58	58	59½	50	nw.	nw.	w.	....	....	....
23.	30·170	30·110	29·67	30·13	30·11	68	41	63	53	50½	nw.	n.	w.	....	....	....
24.	30·178	30·148	29·73	29·99	29·88	71	43	56	55	50½	nw.	nw.	ssw.	....	....	·07
25.	30·164	30·074	29·61	29·65	29·77	68	52	62	52	49	sw.	w.	e.	....	....	·59
26.	30·224	30·167	29·63	29·94	29·98	77	56	68	52	53½	nw.	w.	wsw.	....	....	·11
27.	30·357	30·297	29·84	30·10	30·29	81	45	70	57½	52	sw.	wnw.	w.	....	....	·38
28.	30·274	30·104	29·80	30·22	30·12	79	53	73	65	57	e.	sse.	e.	....	....	....
○ 29.	30·072	29·874	29·58	30·04	29·96	79	61	65	67½	56	se.	e.	sse.	....	....	....
30.	30·017	29·928	29·34	29·92	29·85	76	52	75	58½	55	sw.	ssw.	se.	....	....	....

Phil. Mag. Ser. 4. Vol. 10. Pl. I.
Phil. Trans. MDCCCLV *Plate* I

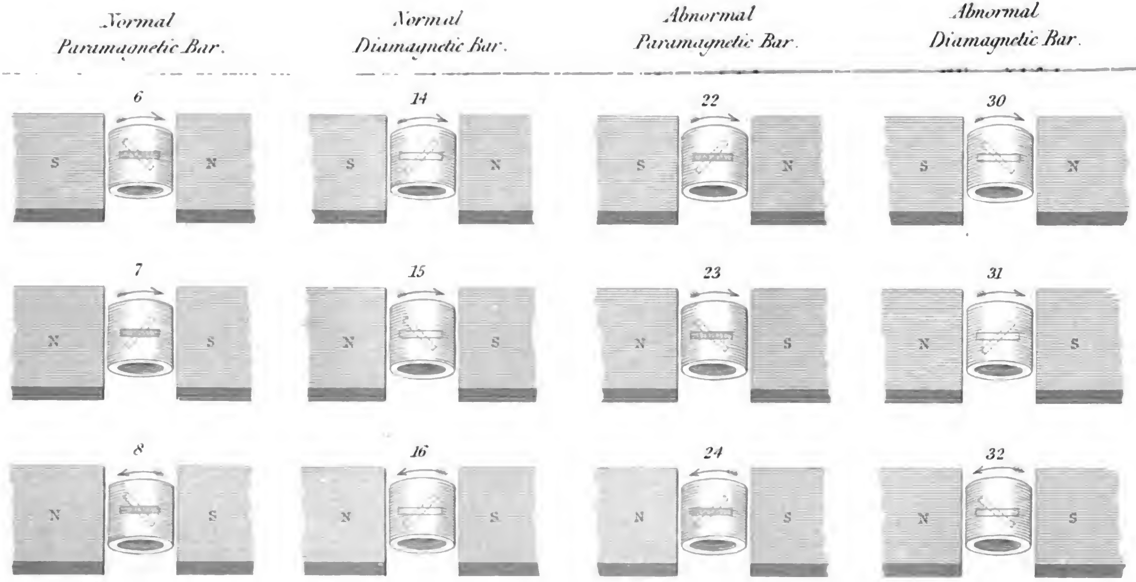


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Fig. 5.ª

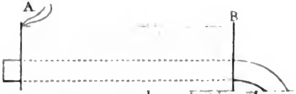
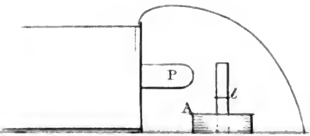


Fig. 39.



39.ª

Fig. 42.

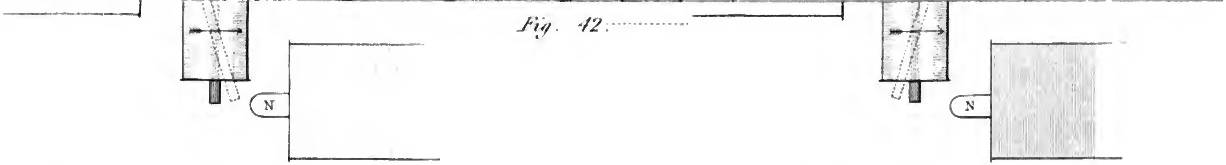
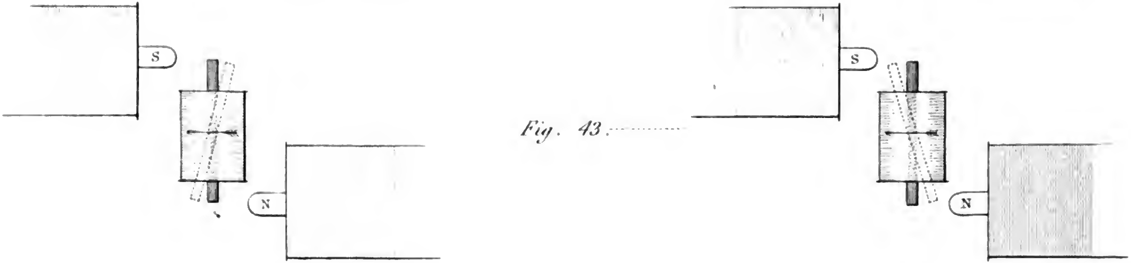


Fig. 43.



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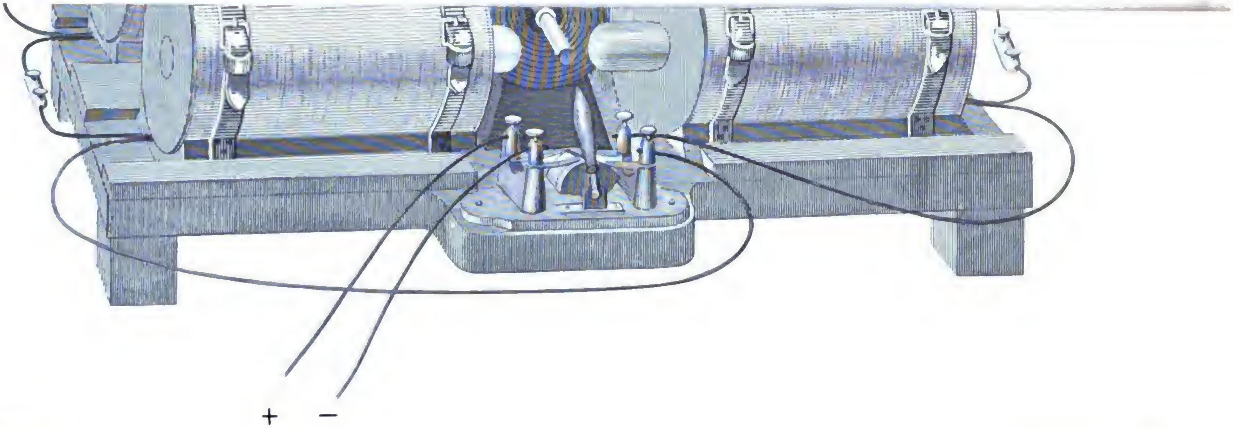
Phil. Trans. MDCCCLV. *Plate* III.



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THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1855.

XX. *On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; to which is prefixed, an Account of some Experiments on Molecular Influences. By* JOHN TYNDALL, *Ph.D., F.R.S. &c.*[*]

[With Three Plates.]

INTRODUCTION.

FROM the published account of his researches it is to be inferred, that the same heavy glass, by means of which he produced the rotation of the plane of polarization of a luminous ray, also led Mr. Faraday to the discovery of the diamagnetic force. A square prism of the glass, 2 inches long and 0·5 of an inch thick, was suspended with its length horizontal between the two poles of a powerful electro-magnet: on developing the magnetism the prism moved round its axis of suspension, and finally set its length at right angles to a straight line drawn from the centre of one pole to that of the other. A prism of ordinary magnetic matter, similarly suspended, would, as is well known, set its longest dimension from pole to pole. To distinguish the two positions here referred to, Mr. Faraday introduced two new terms, which have since come into general use: he called the direction parallel to the line joining the poles, the *axial* direction, and that perpendicular to the said line, the *equatorial* direction.

The difference between this new action and the ordinary magnetic action, was further manifested when a fragment of the heavy glass was suspended before a single magnetic pole: the fragment was repelled when the magnetism was excited;

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[*] From the Philosophical Transactions for 1855, part i.; having been received by the Royal Society October 31, 1854, and read January 25, 1855. The Bakerian Lecture.

and to the force which produced this repulsion Mr. Faraday gave the name of *diamagnetism*.

Numerous other substances were soon added to the heavy glass, and, among the metals, it was found that bismuth possessed the new property in a comparatively exalted degree. A fragment of this substance was forcibly repelled by either of the poles of a magnet; while a thin bar of the substance, or a glass tube containing the bismuth in fragments, or in powder, suspended between the two poles of a horseshoe magnet, behaved exactly like the heavy glass, and set its longest dimension equatorial.

These exhaustive researches, which rendered manifest to the scientific world the existence of a pervading natural force, glimpses of which merely had been previously obtained by Brugmann and others, were made public in 1846; and in the following year M. Plücker announced his beautiful discovery of the action of a magnet upon crystallized bodies. His first result was, that when any crystal whatever was suspended between the poles of a magnet, with its optic axis horizontal, a repulsive force was exerted on the said axis, in consequence of which it receded from the poles and finally set itself at right angles to the line joining them. Subsequent experiments, however, led to the conclusion, that the axes of optically negative crystals only experienced this repulsion, while the axes of positive crystals were attracted; or, in other words, set themselves from pole to pole.

The attraction and repulsion, here referred to, were ascribed by M. Plücker to the action of a new force, entirely independent of the magnetism or diamagnetism of the mass of the crystal*.

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* "*The force which produces this repulsion is independent of the magnetic or diamagnetic condition of the mass of the crystal; it diminishes less, as the distance from the poles of the magnet increases, than the magnetic and diamagnetic forces emanating from these poles and acting upon the crystal.*" —Prof. Plücker in Poggendorff's *Annalen*, vol. lvii. No. 10; Taylor's Scientific Memoirs, vol. v. p. 353.

The forces emanating from the poles of a magnet are thus summed up by M. Plücker:—

1st. The magnetic force in a strict sense.

2nd. The diamagnetic action discovered by Faraday.

3rd. The action exerted on the optic axes of crystals (and that producing the rotation of the plane of polarization which probably corresponds to it). *The second diminishes more with the distance than the first, and the first more than the third.*—Taylor's Scientific Memoirs, vol. v. p. 380.

The crystal (cyanite) does not point according to the magnetism of its substance, *but only in obedience to the magnetic action upon its optic axes.*—Letter to Mr. Faraday, Phil. Mag. vol. xxxiv. p. 451. The italics in all cases are M. Plücker's own.

M. De la Rive states the view of M. Plücker to be:—that the axis in its quality as axis, and independently of the very nature of the substance of the crystal, enjoys peculiar properties, more frequently in opposition to

Shortly after the publication of M. Plücker's first memoir, Mr. Faraday observed the remarkable magnetic properties of crystallized bismuth; and his researches upon this, and other kindred points, formed the subject of the Bakerian Lecture before the Royal Society for the year 1849.

Through the admirable lectures of Professor Bunsen on Electrochemistry in 1848, I was first made acquainted with the existence of the diamagnetic force; and in the month of November 1849 my friend Professor Knoblauch, then of Marburg, now of the University of Halle, suggested to me the idea of repeating the experiments of M. Plücker and Mr. Faraday. He had procured the necessary apparatus with the view of prosecuting the subject himself, but the pressure of other duties prevented him from carrying out his intention. I adopted the suggestion and entered upon the inquiry in M. Knoblauch's cabinet. Our frequent conversations upon the subject led to the idea of our making a joint publication of the results: this we accordingly did in two papers, the first of which, containing a brief account of some of the earliest experiments, appeared in the Philosophical Magazine for March 1850, and some time afterwards in Poggendorff's *Annalen*; while the second and principal memoir appeared in the Philosophical Magazine for July 1850, and in Poggendorff's *Annalen* about January 1851*. I afterwards continued my researches in the private laboratory of Professor Magnus of Berlin, who, with prompt kindness and a lively interest in the furtherance of the inquiry, placed all necessary apparatus at my disposal. The results of this investigation are described in a paper published in the Philosophical Magazine for September 1851, and in Poggendorff's *Annalen*, vol. lxxxiii.

In these memoirs it was shown that the law according to which the axes of positive crystals are attracted and those of negative crystals repelled, was contradicted by the deportment of numerous crystals both positive and negative. It was also proved that the force which determined the position of the optic axes in the magnetic field was not independent of the magnetism or diamagnetism of the mass of the crystal; inasmuch as two crystals, of the same form and structure, exhibited altogether different effects, when one of them was magnetic and the other diamagnetic. It was shown, for example, that pure carbonate of lime was diamagnetic, and always set its optic axis equatorial; but that when a portion of the calcium was replaced by an iso-

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those possessed by the substance itself, or which at least are altogether independent of it."—Treatise on Electricity, vol. i. p. 359.

* The memoirs in the Philosophical Magazine were written by me, and the second one has, I believe, been translated into German by Dr. Krönig: the papers in Poggendorff's *Annalen* were edited by my colleague.—J. T.

morphous magnetic constituent, which neither altered the structure nor affected the perfect transparency of the crystal, the optic axis set itself from pole to pole. The various complex phænomena exhibited by crystals in the magnetic field were finally referred to the modification of the magnetic and diamagnetic forces by the peculiarities of molecular arrangement.

This result is in perfect conformity with all that we know experimentally regarding the connexion of matter and force. Indeed it may be safely asserted that every force which makes matter its vehicle of transmission must be influenced by the manner in which the material particles are grouped together. The phænomena of double refraction and polarization illustrate the influence of molecular aggregation upon light. Wertheim has shown that the velocity of sound through wood, *along* the fibre, is about five times its velocity *across* the fibre: De la Rive, DeCandolle and myself have shown the influence of the same molecular grouping upon the propagation of heat. In the first section of the present paper, the influence of the molecular structure of wood upon its magnetic deportment is described: De Senarmont has shown that the structure of crystals endows them with different powers of calorific conduction in different directions: Knoblauch has proved the same to be true, with regard to the transmission of radiant heat: Wiedemann finds the passage of frictional electricity along crystals to be affected by structure; and some experiments, which I have not yet had time to follow out, seem to prove, that bismuth may, by the approximation of its particles, be caused to exhibit, in a greatly increased degree, those singular effects of induction which are so strikingly exhibited by copper, and other metals of high conducting power.

Indeed the mere *à priori* consideration of the subject must render all the effects here referred to extremely probable. Supposing the propagation of the forces to depend upon a subtle agent, distinct from matter, it is evident that the progress of such an agent from particle to particle must be influenced by the manner in which these particles are arranged. If the particles be twice as near each other in one direction as in another, it is certain that the agent of which we speak will not pass with the same facility in both directions. Or supposing the effects to which we have alluded to be produced by motion of some kind, it is just as certain that the propagation of this motion must be affected by the manner in which the particles which transmit it are grouped together. Whether, therefore, we take the old hypothesis of imponderables, or the new, and more philosophic one, of modes of motion, the result is still the same.

If this reasoning be correct, it would follow, that, if the molecular arrangement of a body be changed, such a change will manifest itself by an alteration of deportment towards any force operating upon the body: the action of compressed glass upon light, which Wertheim in his recent researches* has so beautifully turned to account in the estimation of pressures, is an illustration in point; and the inference also receives the fullest corroboration from experiments, some of which are recorded in the papers alluded to, and which show that all the phænomena of magnecrystallic action may be produced by simple mechanical agency. What the crystalline forces do in one case, mechanical force, under the control of the human will, accomplishes in the other. A crystal of carbonate of iron, for example, suspended in the magnetic field, exhibits a certain deportment: the crystal may be removed, pounded into the finest dust, and the particles so put together that the mass shall exhibit the same deportment as before. A bismuth crystal suspended in the magnetic field, with its planes of principal cleavage vertical, will set those planes equatorial; but if the crystalline planes be squeezed sufficiently together by a suitable mechanical force, this deportment is quite changed, and the line which formerly set equatorial now sets axial†.

Thus we find that the influence of crystallization may be perfectly imitated, and even overcome, by simple mechanical agencies. It would of course be perfectly unintelligible were we to speak of any direct action of the magnetic force upon the force by which the powdered carbonate of iron, or the solid cube of bismuth, is compressed; such an idea, however, appears scarcely less tenable than another which seems to be entertained by some who feel an interest in this subject; namely, that there is a direct action of the magnet upon the molecular forces which built the crystal. The function of such forces, as regards the production of the effects, is, I believe, *mediate*; the molecular forces are exerted in placing the particles in position, and the subsequent phænomena, whether exhibited in magnecrystallic action, in the bifurcation and polarization of a luminous ray, or in the modification of any other force transmitted through the crystal, are not due to the action of force upon force, except through the intermediation of the particles referred to‡.

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* Phil. Mag. October and November 1854.

† Phil. Mag. vol. ii. Ser. 4. p. 183.

‡ The influence of molecular aggregation probably manifests itself on a grand scale in nature. The Snowdon range of mountains, for example, is principally composed of slate rock, whose line of strike is nearly north and south. The magnetic properties of this rock I find, by some preliminary experiments, to be very different along the cleavage from what they are across it. I cannot help thinking that these vast masses, in their present position, must exert a different action on the magnetic needle from that which would be exerted if the line of strike were east and west.

The foregoing introductory statement will, perhaps, sufficiently indicate the present aspect of this question. The object I proposed to myself in commencing the inquiry now laid before the Royal Society, was to obtain, if possible, clearer notions of the nature of the diamagnetic force than those now prevalent; for though, in the preceding paragraphs, we have touched upon some of the most complex phænomena of magnetism and diamagnetism, and are able to produce these phænomena at will, the greatest diversity of opinion still prevails as to the real relationship of the two forces. The magnetic force, we know, embraces both attraction and repulsion, thus exhibiting that wonderful dual action which we are accustomed to denote by the term polarity. Mr. Faraday was the first who proposed the hypothesis that diamagnetic bodies, operated on by magnetic forces, possess a polarity "the same in kind as, but the reverse in direction, of that acquired by iron, nickel, and ordinary magnetic bodies under the same circumstances*." M. W. Weber sought to confirm this hypothesis by a series of experiments, wherein the excitement of the supposed diamagnetic polarity was applied to the generation of induced currents—apparently with perfect success. Mr. Faraday afterwards showed, and his results were confirmed by M. Verdet, that effects similar to those described by the distinguished German, were to be attributed, not to the excitement of diamagnetic polarity, but to the generation of ordinary induced currents in the metallic mass. On the question of polarity Mr. Faraday's results were negative, and he therefore, with philosophic caution, holds himself unpledged to his early opinion. M. Weber, however, still retains his belief in the reverse polarity of diamagnetic bodies, whereas Weber's countryman M. von Feilitsch, in a series of memoirs recently published in Poggendorff's *Annalen*, contends that the polarity of diamagnetic bodies is precisely the same as that of magnetic ones. In this unsettled state of the question nothing remained for me but a complete examination of the nature of the diamagnetic force, and a thorough comparison of its phænomena with those of ordinary magnetism. This has been attempted in the following pages: with what success it must be left to the reader to decide.

Before entering upon the principal inquiry, I will introduce one or two points which arose incidentally from the investigation, and which appear to be worth recording.

I. ON THE MAGNETIC PROPERTIES OF WOOD.

No experiments have yet been made, to determine the influence of structure upon the magnetic deportment of this substance;

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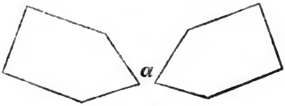
* Experimental Researches, 2429, 2430.

and even on the question whether it is magnetic, like iron, or diamagnetic, like bismuth, differences of opinion appear to prevail. Such differences are to be referred to the extreme feebleness of the force proper to the wood itself, and its consequent liability to be masked by extraneous impurity. In handling the substance intended for experiment the fingers must be kept perfectly clean, and frequent washing is absolutely necessary. After reducing the substance to a regular shape, so as to annul the influence of exterior form, its outer surface must be carefully removed by glass, and the body afterwards suspended by a very fine fibre between the poles of a strong electro-magnet.

The first step in the present inquiry was to ascertain whether the substance examined was paramagnetic* or diamagnetic. It is well known, that, in experiments of this kind, moveable masses of soft iron are placed upon the ends of the electro-magnet, the distance between the masses being varied to suit the experiment. In front of a pointed mass of iron of this kind, a cube of wood was suspended, and if, on exciting the magnet, the cube was repelled by the point, it was regarded as diamagnetic; if attracted, it was considered to be paramagnetic. The force was considerably intensified by placing the two moveable poles as in fig. 1, and suspending the cube at *a* on the same level with the points; a diamagnetic body placed there is, on the development of the magnetic force, forcibly driven *from* the line which unites the points, while a magnetic body is forcibly drawn in between them.

Fig. 1.



Having thus observed the deportment of the mass, the cube was next suspended between the *flat* ends of the poles sketched in fig. 1. The parallel faces were about three-quarters of an inch apart, and in each case the fibre of the suspended wood was horizontal. The specimen first examined was Beef-wood: suspended in the position *a*, fig. 1, the mass was repelled: suspended between the flat poles, on exciting the magnet, the cube, if in an oblique position, turned and set its fibre equatorial.

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* The effects exhibited by iron and by bismuth come properly under the general designation of *magnetic* phænomena: to render their subdivision more distinct Mr. Faraday has recently introduced the word *paramagnetic* to denote the old magnetic effects, of which the action of iron is an example, Wherever the word *magnetic* occurs, without the prefix, it is always the old action that is referred to.

By suitably breaking and closing the circuit, the cube could be turned 180° round and held in this new position. The axial position of the ligneous fibre was one of unstable equilibrium, from which, if it diverged in the slightest degree right or left, the cube turned and finally set its fibre equatorial. The following is a statement of the results obtained with thirty-five different kinds of wood:—

Table I.

Name of wood.	Deportment of mass.	Deportment of structure.	Remarks.
1. Beef-wood	Diamagnetic.	Fibre equatorial.	
2. Black Ebony	"	"	
3. Box-wood	"	"	
4. Second specimen ...	"	"	
5. Brazil-wood	"	"	
6. Braziletto	"	"	Action decided.
7. Bullet-wood	"	"	Action decided.
8. Cam-wood	"	"	
9. Cocoa-wood	"	"	
10. Coromandel-wood ...	"	"	Action strong.
11. Green Ebony	"	"	Action strong.
12. Green-heart	"	"	Action strong.
13. Iron-wood	"	"	
14. King-wood	"	"	Action strong.
15. Locust-wood	"	"	
16. Maple	"	"	Action decided.
17. Lance-wood	"	"	Action decided.
18. Olive-tree	"	"	
19. Peruvian-wood	"	"	Action strong.
20. Prince's-wood	"	"	
21. Camphor-wood	"	"	
22. Sandal-wood	"	"	
23. Satin-wood............	"	"	
24. Tulip-wood	"	"	
25. Zebra-wood	"	"	
26. Botany Bay Oak ...	"	"	Action strong.
27. Mazatlan-wood	"	"	Action decided.
28. Tamarind-wood	"	"	
29. Sycamore	"	"	Action decided.
30. Beech	"	"	Action decided.
31. Ruby-wood............	"	"	
32. Jacca	"	"	
33. Oak......................	"	"	Action strong.
34. Yew	"	"	Action feeble.
35. Black Oak	Paramagnetic.	"	Action decided.

The term "decided" is here used to express an action more energetic than ordinary, but in no case does the result lack the decision necessary to place it beyond doubt. It must also be remarked that the term "strong" is used in relation to the general deportment of wood; for, compared with the action of many other diamagnetic bodies, the strongest action of wood is but feeble. Simple as the problem may appear, it required considerable time and care to obtain the results here recorded. During a first examination of the cubes eight anomalies presented themselves—in eight cases the fibre set either oblique or axial. The whole thirty-five specimens were carefully rescraped with glass and tested once more; still two remained, which, though repelled as masses, persistently set with the fibre axial, and oscillated round this position so steadily as to lead to the supposition that the real deportment of the substance was thus exhibited. I scraped these cubes ten times successively, and washed them with all care, but the deportment remained unchanged. The cubes, for the sake of reference, had been stamped with diminutive numbers by the maker of them; and I noticed at length, that in these two cases a trace of the figures remained: on removing the whole surface which bore the stamp from each, the cubes forsook the axial position, and set, like the others, with the fibre equatorial.

The influence of the mere *form* of an impurity was here very prettily exhibited. The cubes in question had been stamped (probably by an iron tool) with the numbers 33 and 37, which lay in the line of the fibre; the figures, being dumpy little ones, caused an elongation of the magnetic impurity along the said line, and the natural consequence of this elongation was the deportment above described.

Of the thirty-five specimens examined one proved to be paramagnetic. Now, it may be asked, if the views of molecular action stated in the foregoing pages be correct, how is it that this paramagnetic cube sets its fibre equatorial? The case is instructive. The substance (bog oak) had been evidently steeped in a liquid containing a small quantity of iron in solution, whence it derives its magnetism; but here we have no substitution of paramagnetic molecules for diamagnetic ones, as in the cases referred to. The extraneous magnetic constituent is practically indifferent as to the direction of magnetization, and it therefore accommodates itself to the directive action of the wood to which it is attached.

II. ON THE ROTATION OF BODIES BETWEEN POINTED MAGNETIC POLES.

In his experiments on charcoal, wood-bark and other substances, M. Plücker discovered some very curious phænomena of rotation, which occurred on removing the substance experimented on from one portion of the magnetic field to another. To account for these phænomena, he assumed, that in the substances which exhibited the rotation, two antagonist forces were perpetually active—a repulsive force, which caused the substance to assume one position; and an attractive force, which caused it to assume a different position: that, of these two forces, the repulsive diminished more quickly than the attractive, when the distance of the body from the poles was augmented. Thus, the former, which was predominant close to the poles, succumbed to the latter when a suitable distance was attained, and hence arose the observed rotation.

Towards the conclusion of the memoir published in the September Number of the Philosophical Magazine for 1851, I stated that it was my intention further to examine this highly ingenious theory. I shall now endeavour to fulfil the promise then made, and to state, as briefly as I can, the real law which regulates these complex phænomena.

The masses of soft iron sketched in fig. 1 were placed upon the ends of the electro-magnet, with their points facing each other; between the points the body to be examined was suspended by a fine fibre, which, passing round a groove, the substance could be raised or lowered by turning a milled head. The body was first suspended on the level of the points and its deportment noted, it was then slowly elevated, and the change of position, if any, was observed. It was finally permitted to sink below the points and its deportment there noted also.

The following is a statement of the results; the words 'equatorial' (E.) and 'axial' (A.) imply that the longest horizontal dimension of the substance examined took up the position denoted by each of these words respectively. The manner in which the bars were prepared is explained further on.

Table II.

Name of substance.	Horizontal dimensions.	Deportment of mass.	Position.		
			Between poles.	Above.	Below.
1. Tartaric acid.........	0·5 ×0·1	Diamagnetic.	E.	A.	A.
2. A second specimen...	0·4 ×0·1	„	E.	A.	A.
3. Red ferrocyanide of potassium	0·6 ×0·1	Paramagnetic.	A.	E.	E.
4. A second prism	0·9 ×0·12	„	A.	E.	E.
5. Citric acid	0·55×0·25	Diamagnetic.	E.	A.	A.
6. A second specimen...	0·48×0·2	„	E.	A.	A.
7. Beryl	0·45×0·1	Paramagnetic.	A.	E.	E.
8. Saltpetre	0·6 ×0·3	Diamagnetic.	E.	A.	A.
9. Nitrate of soda	0·6 ×0·12	„	E.	A.	A.
10. Sulphate of iron	0·7 ×0·15	Paramagnetic.	A.	E.	E.
11. A second specimen...	0·6 ×0·03	„	A.	E.	E.
12. A third specimen ...	1·0 ×0·13	„	A.	E.	E.
13. Calcareous spar	0·5 ×0·1	Diamagnetic.	E.	A.	A.
14. A full crystal.........	....	„	E.	A.	A.
15. Carbonate of iron ...	0·5 ×0·1	Paramagnetic.	A.	E.	E.

(Table II. *continued.*)

Name of substance.	Horizontal dimensions.	Deportment of mass.	Position.		
			Between poles.	Above.	Below.
16. Carbonate of iron powdered and compressed	0·9 ×0·18	Paramagnetic.	A	E.	E
17. Compressed disc	0·8 ×0·08	,,	A	E.	E
18. Bismuth	0·95×0·15	Diamagnetic.	E	A.	A
19. The same compressed	0·7 ×0·05	,,	E	A.	A
20. The same powdered and compressed ...	0·6 ×0·07	,,	E.	A.	A.
21. Cylinder of the same.	1·0 ×0·15	,,	E.	A.	A.
22. Tourmaline	2·1 ×0·1	Paramagnetic.	A.	E.	E.
23. A second specimen ...	1·1 ×0·1	,,	A.	E.	E.
24. A third	0·9 ×0·1	,,	A.	E.	E.
25. Sulphate of nickel ...	0·9 ×0·3	,,	A.	E.	E.
26. A second specimen...	0·6 ×0·2	,,	A.	E.	E.
27. Heavy spar	0·38×0·18	Diamagnetic.	E.	A.	A.
28. A second specimen ...	0·4 ×0·18	,,	E.	A.	A.
29. Carbonate of tin powdered and compressed	0·34×0·04	,,	E.	A.	A.
30. A second specimen ...	length 6 times width	,,	E.	A.	A.
31. Ammonio-phosphate of magnesia powdered and compressed	0·3 ×0·06	,,	E.	A.	A.
32. A second specimen...	0·5 ×0·07	,,	E.	A.	A.
33. Carbonate of magnesia powdered and compressed	0·45×0·04	,,	E.	A.	A.
34. Sulphate of magnesia	0·32×0·2	,,	E.	A.	A.
35. A second specimen ..	0·25×0·15	,,	E.	A.	A.
36. Flour compressed ...	0·24×0·04	,,	E.	A.	A.
37. Oxalate of cobalt ...	0·6 ×0·08	Paramagnetic.	A.	E.	E.

These experiments might be extended indefinitely, but sufficient are here to enable us to deduce the law of action. In the first place we notice, that all those substances which set equatorial between the points, and axial above and below them, are *diamagnetic*; while all those which set axial between the points, and equatorial above and below them, are *paramagnetic*. When any one of the substances here named is reduced to the spherical form, this rotation is not observed. I possess, for example, four spheres of calcareous spar, and when any one of them is suspended between the points, it takes up a position which is not changed when the sphere is raised or lowered; the crystallographic axis sets equatorial in all positions. A sphere of compressed carbonate of iron, suspended between the points, also sets that diameter along which the pressure is exerted from pole to pole, and continues to do so when raised or lowered. A sphere of compressed bismuth, on the other hand, sets its line of compression always equatorial. The position taken up by the spheres depends upon the *molecular structure* of the substances which compose them; but, when the mass is *elongated*, another action comes into play. Such a mass being suspended with its length horizontal, the *repulsion of its ends* constitutes a mechanical couple which increases in power with the length of the mass; and when the body is long enough, and the local repulsion of the ends strong enough, the couple, when it acts in opposition to the directive tendency due to structure, is able to overcome the latter and to determine the position of the mass. In all the cases cited, it was so arranged that the length of the body and its structure should act in opposition to each other. Tartaric acid and citric acid cleave with facility in one direction, and, in the specimens used, the planes of cleavage were perpendicular to the length of the body. In virtue of the structure these planes tended to set equatorial, but the repulsion of the elongated mass by the points prevented this, and caused the planes to set axial. When, however, the body was raised or lowered out of the sphere of this local repulsion, and into a position where the distribution of the force was more uniform, the advantage due to length became so far diminished that it was overcome, in turn, by the influence of structure, and the planes of cleavage turned into the equatorial position. In the specimen of saltpetre the shortest horizontal dimension was parallel to the axis of the crystal, which axis, when the influence of form is destroyed, always sets equatorial. A full crystal of calcareous spar will, when the magnetic distribution is tolerably uniform, always set its axis at right angles to the line joining the poles; but the axis is the shortest dimension of the crystal, and, between the points, this mechanical disadvantage compels the influence of structure to succumb to the influence of shape. A cube of calcareous spar, in my possession, may be caused to set the optic axis from pole to pole between the points, but this is evidently due to the elongation of the mass along the diagonals; for, when the corner of the cube succeeds in passing the point of the pole, the mass turns its axis with surprising energy into the equatorial position, round which it oscillates with great vivacity. Counting the oscillations, I found that eighty-two were performed by the cube, when its axis was equatorial, in the time required to perform fifty-nine, when the axis stood from pole to pole. Heavy spar and cœlestine are beautiful examples of directive action. These crystals, as is well known, can be cloven into prisms with rhombic bases: the principal cleavage is parallel to the base of the prism, while the two subordinate cleavages constitute the sides. If a short prism be suspended in a tolerably uniform field of force, so that its rhombic ends shall be horizontal, on exciting the magnet the short diagonal will set equatorial, as shown in fig. 2. If the prism be suspended with its axis and the short diagonal horizontal, the long diagonal being therefore vertical, the short diagonal will retain the equatorial position, while the axis of the prism sets axial as in fig. 3. If the prism be suspended with its long diagonal and axis horizontal, the short diagonal being vertical, and its directive power therefore annulled, the axis will take up the equatorial position, as in fig. 4. Now as the line which sets equatorial in diamagnetic bodies is that in which the magnetic repulsion acts most strongly*, the crystal before us furnishes a perfect example of

Fig. 2.

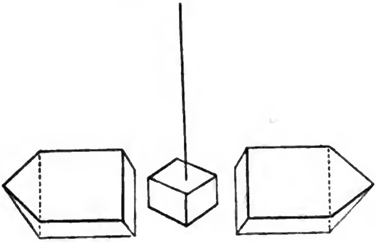


Fig. 3.

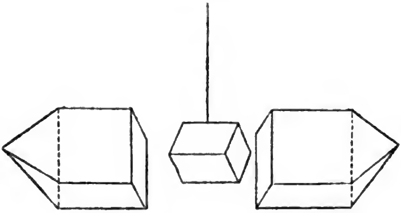
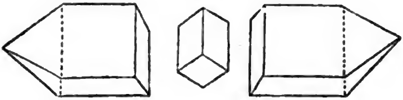


Fig. 4.



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* Phil. Mag. Ser. 4. vol. ii. p. 177.

a substance possessing three rectangular magnetic axes, no two of which are equal. In the experiment cited in Table II., the mass was so cut that the short diagonal of the rhombic base was perpendicular to the length of the specimen. Carbonate of tin, and the other powders, were compressed by placing the powder between two clean plates of copper, and squeezing them together in a strong vice. The line of compression in diamagnetic bodies always sets equatorial, when the field of force is uniform, or approximately so; but between points the repulsion of the ends furnishes a couple strong enough to overcome this directive action, causing the longest dimension of the mass to set equatorial, and consequently its line of compression axial.

The antithesis between the deportment of diamagnetic bodies and of paramagnetic ones is perfect. Between the points the former class set equatorial, the latter axial. Raised or lowered, the former set axial, the latter equatorial. The simple substitution of an attractive for a repulsive force produces this effect. A sphere of ferrocyanide of potassium, for example, always sets the line perpendicular to the crystallographic axis from pole to pole; but when we take a full crystal, whose dimension along its axis, as in one of the cases before us, is six times the dimension at right angles to the axis, the attraction of the ends of such a mass is sufficient to overcome the directive action due to structure, and to pull the crystal into the axial position between the points. In a field of uniform force, or between flat poles, the length sets equatorial, and it is the partial attainment of such a field, at a distance from the points, that causes the crystal to turn from axial to equatorial when it is raised or lowered. Beryl is a paramagnetic crystal, and when the influence of form is annulled, it always sets a line perpendicular to the axis of the crystal from pole to pole; a cube of this crystal, at present in my possession, shows this deportment whether the poles are pointed or flat; but in the specimen examined the dimension of the crystal along its axis was greatest, and hence the deportment described. It is needless to dwell upon each particular paramagnetic body: the same principle was observed in the preparation and choice of all of them; namely, that the line which, in virtue of the internal structure of the substance, would set axial, was transverse to the length of the body. The directive action due to structure was thus brought into opposition with the tendency of magnetic bodies to set their longest dimension from pole to pole: between the points the latter tendency was triumphant; at a distance, on the contrary, the influence of structure prevailed. The substance which possesses this directive action in the highest degree is carbonate of iron: when a lozenge, cloven from the crystalline mass, is suspended from the angle at which the crystallographic axis issues, there is great difficulty in causing the plate to set axial. If the points are near, on exciting the magnetism the whole mass springs to one or the other of the points; and when the points are distant, the plate, although its length may be twenty times its thickness, will set strongly equatorial. An excitation by one cell was sufficient to produce this result. In the experiment cited the residual magnetism was found to answer best, as it permitted the ends of the plate to be brought so near to the points that the mass was pulled into the axial position. When the magnet was more strongly excited, and the plate raised so far above the points as to prevent its springing to either of them, it was most interesting to watch the struggle of the two opposing tendencies. Neither the axial nor the equatorial position could be retained; the plate would wrench itself spasmodically from one position into the other, and, like the human spirit operated on by conflicting passions, find rest nowhere.

The conditions which determine the curious effects described in the present chapter may be briefly expressed as follows:—

An elongated diamagnetic body being suspended in the magnetic field, if the shortest horizontal dimension tend, in virtue of the internal structure of the substance, to set equatorial, it is opposed by the tendency of the longest dimension to take up the same position. Between the pointed poles the influence of length usually predominates; above the points and below them the directive action due to structure prevails.

Hence, the rotation of such a diamagnetic body, on being raised or lowered, is always from the equatorial to the axial position.

If the elongated mass be magnetic, and the shortest dimension of the mass tend, in virtue of its structure, to set from pole to pole, it is opposed by the tendency of the longest dimension to take up the same position. Between the points the influence of length is paramount; above and below the points the influence of structure prevails.

Hence, the rotation of magnetic bodies, on being raised or lowered, is always from the axial to the equatorial position.

The error of the explanation which referred many of the above actions to the presence of two conflicting forces, one of which diminished with the distance in a quicker ratio than the other, lies in the supposition, that the assuming of the axial position proved a body to be magnetic, while the assuming of the equatorial position proved a body to be diamagnetic. This assumption was perfectly natural in the early stages of diamagnetic research, when the modification of magnetic force by structure was unknown. Experience however proves that the total mass of a magnetic body continues to be attracted after it has assumed the equatorial position, while the total mass of a diamagnetic body continues to be repelled after it has taken up the axial one.

III. On the Distribution of the Magnetic Force between two Flat Poles.

In experiments where a uniform distribution of the magnetic force is desirable, flat poles, or magnetized surfaces, have been recommended. It has long been known that the force proceeds with great energy from the edges of such poles: the increase of force from the centre to the edge has been made the subject of a special investigation by M. von Kolke*. The central portion of the magnetic field, or space between two such magnetized surfaces, has hitherto been regarded as almost perfectly uniform, and indeed for all ordinary experiments the uniformity is sufficient. But, when we examine the field carefully, we find that the uniformity is not perfect. Substituting, for the sake of convenience, the edge of a pole for a point, I studied the phænomena of rotation described in the last section, in a great number of instances, by comparing the deportment of an elongated body, suspended in the centre of the space between two flat poles, with its deportment when suspended between the top or the bottom edges. Having found that the fibre of wood, in masses where form had no influence, always set equatorial, I proposed to set this tendency to contend with an elongation of the mass in a direction at right angles to the fibre. For this purpose thirty-one little wooden bars were carefully prepared and examined, the length of each bar being about twice its width, and the fibre coinciding with the latter dimension. The bars were suspended from an extremely fine fibre of cocoon silk, and in the centre of the magnetic field each one of them set its length axial and consequently its fibre equatorial. Between the top and bottom edges, on the contrary, each piece set its longest dimension equatorial, and, consequently, the fibre axial.

For some time I referred the axial setting of the mass, in the centre of the field, to the directive action of the fibre, though, knowing the extreme feebleness of this directive action, I was surprised to find it able to accomplish what the experiments exhibited. The thought suggested itself, however, of suspending the bars with the fibre vertical, in which position the latter could have no directive influence. Here also, to my surprise, the directive action, though slightly weakened, was the same as before: in the centre of the field the bars took up the axial position. Bars of sulphur, wax, salt of hartshorn, and other diamagnetic substances were next examined: they all acted in the same manner as the wood, and thus showed that the cause of the rotation lay, not in the structure of the substances, but in the distribution of the magnetic force around them. This distribution in fact was such, that the straight line which connected the centre of one pole with that of the opposite one was the line of weakest force. Ohm represents the distribution of electricity upon the surfaces of conductors by regarding the tensions as ordinates, and erecting them from the points to which they correspond, the steepness of the curve formed by uniting the ends of the ordinates being the measure of the increase or diminution of tension. Taking the centre of the magnetic field as the origin, and drawing lines axial and equatorial; if we erect the magnetic tensions along these lines, we shall find a steeper curve in the equatorial than in the axial direction. This may be proved by suspending a bit of carbonate of iron in the centre of the magnetic field; on exciting the magnet, the suspended body will not move to the nearest portion of the flat pole, though it may be not more than a quarter of an inch distant, but will move equatorially towards the edges, though they may be two inches distant. The little diamagnetic bars referred to were therefore pushed into the axial position by the force acting with superior power in an equatorial direction.

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* Poggendorff's *Annalen*, vol. lxxxi. p. 321.

The results just described are simply due to the recession of the ends of an elongated body from places of stronger to those of weaker force; but it is extremely instructive to observe how this result is modified by structure. If, for example, a plate of bismuth be suspended between the poles with the plane of principal cleavage vertical, the plate will assert the equatorial position from top to bottom; and in the centre with almost the same force as between the edges. The cause of this lies in the structure of the bismuth. Its position in the field depends not so much upon the distribution of the magnetic force around it, as upon the direction of the force *through* it. I will not, however, anticipate matters by entering further upon this subject at present.

IV. COMPARATIVE VIEW OF PARAMAGNETIC AND DIAMAGNETIC PHÆNOMENA.

1. *State of Diamagnetic Bodies under Magnetic Influence.*

When a piece of soft iron is brought near to a magnet, it is attracted by the latter: this attraction is not the act of the magnet alone, but results from the mutual action of the magnet and the body upon which it operates. The soft iron in this case is said to be magnetized by influence; it becomes itself a magnet, and the intensity of its magnetization varies with the strength of the influencing magnet. Poisson figured the act of magnetization as consisting of the decomposition of a neutral magnetic fluid into north and south magnetism, the amount of the decomposition being proportional to the strength of the magnet which produces it. Ampère, discarding the notion of magnetic fluids, figured the molecules of soft iron as surrounded by currents of electricity, and conceived the act of magnetization to consist in setting the planes of these molecular currents parallel to each other: the degree of parallelism, or in other words, the intensity of the magnetization, depending, as in Poisson's hypothesis, upon the strength of the influencing magnet.

The state into which the soft iron is here supposed to be thrown is a state of constraint, and when the magnet is removed, the substance returns to its normal condition. Poisson's separated fluids rush together once more, and Ampère's molecular currents return to their former irregular positions. As our knowledge increases, we shall probably find both hypotheses inadequate to represent the phænomena; the only thing certain is, that the soft iron, when acted upon by the magnet, is thrown into an unusual condition, in virtue of which it is attracted; and that the intensity of this condition is a function of the force which produces it.

There are, however, certain bodies which, unlike soft iron, offer a great resistance to the imposition of the magnetic state, but when once they are magnetized they do not, on the removal of the magnet, return to their neutral condition, but on the contrary retain the magnetism impressed on them. It is in virtue of this quality that steel can be formed into compass needles and permanent magnets. This power of resistance and retention is named by Poisson coercive force.

Let us conceive a body already magnetized, and in which coercive force exists in a very high degree—a piece of very hard steel for example—to be brought near a magnet, the strength of which is not sufficient to magnetize the steel further. To simplify the matter let us fix our attention upon the south pole of the magnet, and conceive it to act upon the north pole of the piece of steel. Let the magnetism of the said south pole, referred to any unit, be M, and of the north pole of the steel, M'; then their mutual attraction, at the unit of distance, is expressed by the product MM'. Conceive now the magnet to increase in power from M to nM, the steel being still supposed hard enough to resist magnetization by influence; the mutual attraction now will be

$$nMM',$$

or n times the former attraction; hence when a variable magnetic pole acts on an opposite one of constant power, the attraction is proportional to the strength of the former.

Let us now take a body whose magnetization varies with that of the magnet: a south pole of the strength M induces in such a body a north pole of the strength M′, and the attraction which results from their mutual action is

$$MM'.$$

Let the strength of the influencing south pole increase from M to nM; then, assuming the magnetism of the body under influence to increase in the same ratio, the strength of the above-mentioned north pole will become nM′, and the attraction, expressed by the product of both, will be

$$n^2MM';$$

that is to say, the attraction of a body magnetized by influence, and whose magnetism varies as the strength of the influencing magnet, is proportional to the *square of the strength* of the latter.

Here then is a mark of distinction between those bodies which have their power of exhibiting magnetic phænomena conferred upon them by the magnet, and those whose actions are dependent upon some constant property of the mass: in the latter case the resultant action will be simply proportional to the strength of the magnet, while in the former case a different law of action will be observed*.

The examination of this point lies at the very foundation of our inquiries into the nature of the diamagnetic force. Is the repulsion of diamagnetic bodies dependent merely on the mass considered as ordinary matter, or is it due to some condition impressed upon the mass by the influencing magnet? This question admits of the most complete answer either by comparing the increase of repulsion with the increase of power in the magnet which produces the repulsion, or by comparing the attraction of a paramagnetic body, which we know to be thrown into an unusual condition, with the repulsion of a diamagnetic body, whose condition we would ascertain.

Bars of iron and bismuth, of the same dimensions, were submitted to the action of an electro-magnet, which was caused gradually to increase in power; commencing with an excitation by one cell, and proceeding up to an excitation by ten or fifteen. The strength of the current was in each case accurately measured by a tangent galvanometer. The bismuth bar was suspended between the two flat poles, and, when the magnet was excited, took up the equatorial position. The iron bar, if placed directly between the poles, would, on the excitation of the magnetism, infallibly spring to one of them; hence it was removed to a distance of 2 feet 7 inches from the centre of the space between the poles, and in a direction at right angles to the line which united them. The magnet being excited, the bar was drawn a little aside from its position of equilibrium and then liberated, a series of oscillations of very small amplitude followed, and the number of oscillations accomplished in a minute was carefully ascertained. Tables III. and IV. contain the results of experiments made in the manner described with bars of iron and bismuth of the same dimensions.

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* This test was first pointed out in a paper on the Polarity of Bismuth, Phil. Mag. Nov. 1851, p. 333. I have reason, however, to know that the same thought occurred to M. Poggendorff previous to the publication of my paper.—J. T.

Table III.

Bar of soft iron, No. 1.
length 0·8 of an inch.
width 0·13 of an inch.
depth 0·15 of an inch.

Strength of current.	Attraction.
168	168^2
214	204^2
248	253^2
274	275^2
323	313^2
362	347^2
385	374^2
411	385^2

Table IV.

Bar of bismuth, No. 1.
length 0·8 of an inch.
width 0·13 of an inch.
depth 0·15 of an inch.

Strength of current.	Repulsion.
78	78^2
136	135^2
184	191^2
226	226^2
259	259^2
287	291^2
341	322^2
377	359^2
411	386^2

These experiments prove, that, up to a strength of about 280, the attractive force operating upon the iron, and the repulsive force acting upon the bismuth, are each accurately proportional to the square of the strength of the magnetizing current. For higher powers, both attraction and repulsion increase in a smaller ratio; but it is here sufficient to show that the diamagnetic repulsion follows precisely the same law as the magnetic attraction. So accurately indeed is this parallelism observed, that while the forces at the top of the tables produce attractions and repulsions exactly equal to the square of the strength of the current, the same strength of 411, at the bottom of both tables, produces in iron an attraction of 385^2, and in bismuth a repulsion of 386^2. The numbers which indicate the strength of current in the first column are the tangents of the deflections observed in each case: neglecting the indices, the figures in the second column express the number of oscillations accomplished in a minute, multiplied by a constant factor to facilitate comparison: the forces operating upon the bars being proportional to the squares of the number of oscillations, the simple addition of the index figure completes the expression of these forces.

In these experiments the bismuth bar set *across* the lines of magnetic force, while the bar of iron set *along* them; the former was so cut from the crystalline mass, that the plane of principal cleavage was parallel to the length of the bar, and in the experiments hung vertical. I thought it interesting to examine the deportment of a bar of bismuth which should occupy the same position, with regard to the lines of force, as the bar of iron; that is to say, which should set its length axial. Such a bar is obtained when the planes of principal cleavage are transverse to the length.

Table V.

Bar of bismuth, No. 2.

Length 0·8 of an inch; width 0·13 of an inch; depth 0·15 of an inch.

Set axial between the excited poles.

Strength of current.	Repulsion.
68	67^2
182	187^2
218	218^2
248	249^2
274	273^2
315	309^2
364	350^2
401	366^2

A deportment exactly similar to that exhibited in the foregoing cases is observed here also: up to about 280 the repulsions are accurately proportional to the squares of the current strengths, and from this point forward they increase in a less ratio.

A paramagnetic substance was next examined which set its length at right angles to the lines of magnetic force: the substance was carbonate of iron. The native crystallized mineral was reduced to powder in a mortar, and the powder was compressed. It was suspended, like the bismuth, between the flat poles, with its line of compression horizontal. When these poles were excited, the compressed bar set the line of pressure from pole to pole, and consequently its length equatorial.

Table VI.

Bar of compressed carbonate of iron.

Length 0·95 of an inch; width 0·17 of an inch; depth 0·23 of an inch.

Set equatorial between the excited poles.

Strength of current.	Attraction.
74	74^2
135	133^2
179	180^2
214	218^2
249	248^2
277	280^2
341	330^2
381	353^2

It is needless to remark upon the perfect similarity of deportment here exhibited to the cases previously recorded.

In the following instances the same law of increase is observable.

Table VII.

Sulphate of iron, No. 1.
length 0·75 of an inch.
width 0·22 of an inch.
depth 0·27 of an inch.

Set axial between the excited poles.

Strength of current.	Attraction.
71	70^2
132	133^2
217	220^2
280	275^2
328	333^2
359	348^2

Table VIII.

Sulphate of iron, No. 2.
length 0·75 of an inch.
width 0·22 of an inch.
depth 0·27 of an inch.

Set equatorial between the excited poles.

Strength of current.	Attraction.
70	68^2
121	123^2
203	207^2
271	268^2
331	308^2
370	334^2

In sulphate of iron there is one direction, which, in virtue of the molecular structure of the substance, sets strongly from pole to pole. The bar No. 1 was so cut that this direction was parallel to its length, which therefore set axial; while No. 2 had the same direction *across* it, thus causing the length of the bar to set equatorial.

Two comparative series were finally made with two prisms of iron and of bismuth, more massive than those previously examined.

Table IX.

Bar of iron, No. 2.
length 1·0 inch.
width 0·3 inch.
depth 0·3 inch.

Strength of current.	Attraction.
70	71^2
122	122^2
167	168^2
206	204^2
268	260^2
322	311^2
356	339^2

Table X.

Bar of bismuth, No. 3.
length 1·0 inch.
width 0·3 inch.
depth 0·3 inch.

Strength of current.	Repulsion.
70	72^2
126	121^2
164	166^2
206	205^2
246	248^2
276	279^2
364	344^2

These experiments can leave little doubt upon the mind, that if a magnetic body be attracted in virtue of its being converted into a magnet, a diamagnetic body is repelled *in virtue of its being converted into a diamagnet.* On no other assumption can it be explained, why the repulsion of the diamagnetic body, like the attraction of the magnetic one, increases in a so much quicker ratio than the force of the magnet which produces the repulsion. But, as this is a point of great importance, I will here introduce corroborative evidence, derived from modes of experiment totally different from the method already described. By a series of measurements with the torsion balance, in which the attractive and repulsive forces were determined directly, with the utmost care, the relation of the strength of the magnet to the force acting upon the substances named in Tables XI., XII., and XIII. was found to be as follows:—

Table XI.

Spheres of native sulphur.

Strength of magnet.	Ratio of repulsions.
96	95^2
153	158^2
222	224^2
265	264^2
316	316^2

Table XII.

Spheres of carbonate of lime.

Strength of magnet.	Ratio of repulsions.
134	134^2
172	173^2
213	212^2
259	264^2
310	311^2
370	374^2

Table XIII.

Spheres of carbonate of iron.

Strength of magnet.	Ratio of attractions.
66	66^2
89	89^2
114	114^2
141	141^2

In confirmation of these results I will cite a series obtained by M. E. Becquerel*, whose experiments first showed that the repulsion of diamagnetic bodies follows the same law as the attraction of magnetic ones.

Bar of sulphur.

Length 25 millims.; weight 840 milligrms.

Squares of the magnetic intensities.	Quotients of the repulsions by the magnetic intensities.
36·58	0·902
27·60	0·929
26·84	0·906
16·33	0·920

The constancy of the quotient in the second column proves that the ratio of the repulsions to the squares of the magnetic intensities is a ratio of equality.

I will also cite a series of experiments by Mr. Joule†, which he adduces in confirmation of the results obtained by M. E. Becquerel and myself.

Bar of bismuth.

Strength of magnet.	Repulsions.
1	1^2
2	2^2
4	4^2

Let us contrast these with the results obtained by the same gentleman, by permitting the magnet to act upon a hard magnetic needle.

Magnetic needle.

Length 1·5 of an inch.

Strength of magnet.	Attraction.
1	1
2	2
4	4

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[*] *Annales de Chimie et de Physique*, 3rd series, vol. xxviii. p. 302.

[†] Phil. Mag. 4th series, vol. iii. p. 32.

Here we find experiment in strict accordance with the theoretical deduction stated at the commencement of the present chapter. The intensity of the magnetism of the steel needle is constant, for the steel resists magnetization by influence; the consequence is that the attraction is simply proportional to the strength of the magnet.

A consideration of the evidence thus adduced from independent sources, and obtained by different methods, must, I imagine, render the conclusion certain that diamagnetic bodies, like magnetic ones, exhibit their phænomena in virtue of a state of magnetization induced in them by the influencing magnet. This conclusion is in no way invalidated by the recent researches of M. Plücker, on the law of induction in paramagnetic and diamagnetic bodies, but, on the contrary, derives support from his experiments. With current strengths which stand in the ratio of 1 : 2, M. Plücker finds the repulsion of bismuth to be as 1 : 3·62, which, though it falls short of the ratio of 1 : 4, as the law of increase according to the square of the current would have it, is sufficient to show that the bismuth was not passive, but acted the part of an induced diamagnet in the experiments. In the case of the soft iron itself, M. Plücker finds a far greater divergence; for here currents which stand in the ratio of 1 : 2 produce attractions only in the ratio of 1 : 2·76.

2. *Duality of Diamagnetic Excitement.*

Having thus safely established the fact that diamagnetic bodies are repelled, in virtue of a certain state into which they are cast by the influencing magnet, the next step of our inquiry is;—Will the state evoked by one magnetic pole facilitate, or prevent, the repulsion of the diamagnetic body by a second pole of an opposite quality? If the force of repulsion were an action on the mass, considered as ordinary matter, this mass, being repelled by both the north and the south pole of a magnet, when they operate upon it separately, ought to be repelled by the sum of the forces of the two poles where they act upon it together. But if the excitation of diamagnetic bodies be of a *dual* nature, as is the case with the magnetic bodies, then it may be expected that the state excited by one pole will not facilitate, but on the contrary prevent, the repulsion of the mass by a second opposite pole.

To solve this question the apparatus sketched in fig. 5*a*. Plate IV. was made use of. AB and CD are two helices of copper wire 12 inches long, of 2 inches internal, and of 5½ inches external diameter. Into them fit soft iron cores 2 inches thick: the cores are bent as in the figure, and reduced to flat surfaces along the line *ef*, so that when the two semicylindrical ends are placed together, they constitute a cylinder of the same diameter as the cores within the helices*. In front of these poles a bar of pure bismuth *gh* was suspended by cocoon silk; by imparting a little torsion to the fibre, the end of the bar was caused to press gently against a plate of glass *ik*, which stood between it and the magnets. By means of a current reverser the polarity of one of the cores could be changed at pleasure; thus it was in the experimenter's power to excite the cores, so that the poles PP′ should be of the same quality, or of opposite qualities.

The bar, being held in contact with the glass by a very feeble torsion, a current was sent round the cores, so that they presented two poles of the same name to the suspended bismuth; the latter was promptly repelled, and receded to the position dotted in the figure. On interrupting the current it returned to the glass as before. The cores were next excited, so that two poles of opposite qualities acted upon the bismuth; the latter remained perfectly unmoved†.

This experiment shows that the state, whatever it may be, into which bismuth is cast by one pole, so far from being favourable to the action of the opposite pole, completely neutralizes the effect of the latter. A perfect analogy is thus established between the deportment of the bismuth and that of soft iron under the same circumstances; for it is well known that a similar neutralization occurs in the latter case. If the repulsion depended upon the *abstract strength* of the poles, without reference to their *quality*, the repulsion, when the poles are of opposite names, ought to be *greater* than when they are alike; for in the former case the poles are greatly strengthened by their mutual inductive action, while, in the latter case, they are enfeebled by the same cause. But the fact of the repulsion being dependent on the quality of the pole, demonstrates that the substance is capable of assuming a condition peculiar to each pole, or in other words, is capable of a *dual* excitation‡. The experiments from which these conclusions are drawn are a manifest corroboration of those made by M. Reich with steel magnets.

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* The ends of the semicylinders were turned so as to present the blunted apex of a cone to the mass of bismuth.

† A shorter bar of bismuth than that here sketched, with a light index attached to it, makes the repulsion more evident. It may be thus rendered visible throughout a large lecture-room.

‡ Since the above was written, the opinion has been expressed to me, that the action of the *unlike* poles, in the experiment before us, is "diverted" from the bismuth upon each other, the absence of repulsion being due to this diversion, and not to the neutralization of inductions in the mass of the bismuth itself. Many, however, will be influenced by the argument as stated in the text, who would not accept the interpretation referred to in this note; I therefore let the argument stand, and hope at no distant day to return to the subject.—J. T., May 5, 1855.

If we suppose the flat surfaces of the two semicylinders which constitute the ends of the cores to be in contact, and the cores so excited that the poles P and P′ are of different qualities, the arrangement, it is evident, forms a true electro-magnet of the horseshoe form; and here the pertinency of a remark made by M. Poggendorff, with his usual clearness of perception, becomes manifest; namely, that if the repulsion of diamagnetic bodies be an indifferent one of the mass merely, there is no reason why they should not be repelled by the centre of a magnet, as well as by its ends.

[To be continued.]

XXI. *On the Function of Salt in Agriculture.* *By* A. BEAUCHAMP NORTHCOTE, *Esq., Senior Assistant in the Royal College of Chemistry*[*].

THE employment of salt in agriculture has been of late years so much extended, that the question of the advantages derived from its use, which formerly gave rise to so many discussions, can no longer be raised. Such an accumulation of facts with regard to its action has now taken place, that it is only necessary to pass judgement upon the evidence recorded; and as the practical results which have been obtained by its judicious application have been all more or less of a beneficial tendency, the verdict given cannot fail of being in its favour. It seems to be an incontrovertible fact, that the application of salt to certain lands does increase their fertility, and improve the character of the crops grown upon them; and if this is the case, it is most desirable that we should have as clear an idea as possible of the *rationale* of its action. I do not in the present paper profess to supply this explanation, although I hope that the experiments which I am about to detail may serve to throw some light upon a somewhat obscure subject.

It has long been held that the beneficial action which salt exerts upon soils is due to a power which it possesses of fixing ammonia, and with this view it has frequently been spread over the surface of dung-heaps, or other organic matters decomposing into manures (sometimes also being mixed up with them), with the view of preventing the escape of the ammonia produced in the course of eremacausis: in some cases success has attended these trials, and in some, failure. The question of its absorption of the ammonia eliminated during these changes, is a matter, the investigation of which must necessarily be attended with great difficulty; but with respect to its capability of such absorption, I am informed by my friend Mr. Prideaux, that he has habitually observed that salt mixed with guano retards the exhalation of ammonia from the latter; and he has directed my attention also to some experiments* conducted by M. Barral, one of which proves this point in a remarkable degree. M. Barral exposed to the air for fifteen days, equal weights of guano, and of guano previously mixed with half its weight of salt: the amount of nitrogen in each being determined at the end of that time, he found that the pure guano had lost 11·6 per cent. of its nitrogen, whilst that mixed with salt had lost only 5 per cent.

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[*] Communicated by the Author.

Before entering, however, upon the experiments which I have made upon this subject, and which I may here state have exclusive reference to the absorption of ammonia, I will give the analyses and table of constituents calculated from them, of three specimens of agricultural salt which I have examined†.

Table of direct results of Analysis calculated to 100 parts of the dry salt.

	No. I.		No. II.		No. III.	
Portion soluble in water... contains—		98·174		99·520		97·803
Soda	49·351		51·841		50·720	
Lime	1·711		·493		·593	
Magnesia	·084		·112		·111	
Chlorine	54·824		58·928		57·217	
Sulphuric acid	4·521		1·424		2·046	
Silica	trace		trace		trace	
Portion insoluble in water contains—		1·826		·480		2·197
Lime	·064		trace		·076	
Magnesia	·038		·013		·037	
Alumina	·026		·017		·472	
Sesquioxide of iron	·161		·116		·319	
Phosphoric acid	trace		trace		trace	
Silica	·792		·190		1·044	
Carbonic acid	·091		·014		·099	
Organic matter	·654		·130		·150	
		100·000		100·000		100·000

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* Edinburgh Quarterly Journal of Agriculture, April 1855.

† The three specimens of which the analyses are given were all produced from the Worcestershire brine-springs; No. I. being from Mr. Corbett's works at Stoke Prior; Nos. II. and III. from Messrs. Clay and Newman's and Mr. Noak's works at Droitwich, respectively.

Table of Constituents calculated to 100 parts of the dry salt.

	No. I.	No. II.	No. III.
Sulphate of lime ...	4·155	1·197	1·440
Sulphate of magnesia	·252	·336	·330
Sulphate of soda ...	3·388	·880	1·734
Chloride of sodium ...	90·342	97·106	94·287
Soluble in water ...	98·137	99·519	97·791
By experiment...	98·174	99·520	97·803
Carbonate of lime ...	·114	trace	·135
Carbonate of magnesia	·079	·027	·077
Phosphate of alumina	trace	trace	trace
Alumina	·026	·017	·472
Sesquioxide of iron	·161	·116	·319
Silica	·792	·190	1·044
Organic matter	·654	·130	·150
Insoluble in water	1·826	·480	2·197
Total	99·963	99·999	99·988

Since the ammonia which occurs in nature, whether eliminated directly from decaying organic matters, or existing as atmospheric ammonia, is invariably in the immediate presence of an enormous excess of carbonic acid, we may safely conclude that it is always in the state of carbonate; and as, on the one hand, it is brought down to the earth from the atmospheric regions dissolved in rain or dew, and, on the other, when met with in the salt itself, is usually in the presence of abundance of moisture, we may infer that the carbonate of ammonia is presented to absorbent agents in the soil for the most part in a state of solution. I do not think, therefore, that any great departure from the natural course of things can be alleged against the following experiments, which have been made with a solution of the commercial so-called sesquicarbonate of ammonia, containing 4·3 per cent. of ammonia (NH^4O).

Taking, then, these specimens of agricultural salt in the state in which they are supplied at the salt works, the first thing was to ascertain whether any absorption occurred upon mixing them in that condition with this solution of carbonate of ammonia. Quantities of from 2 to 3 ozs. of the salts were placed in wide-mouthed bottles, and small quantities of the standard ammonia solution added, containing an absolute amount of ammonia (NH^4O) varying from ·15 to ·04 of a grain. The contents of the bottle were shaken together, and an extremely delicate red litmus-paper inserted; it was completely blued in a minute or two; and as the same action took place after allowing the salt and ammonia solution to remain all night in contact, the fact that no absorption had occurred was considered to be clearly proved. Water was then added in quantity just sufficient to moisten the salt, and upon the re-insertion of the test-paper, a diminished blueing showed that the ammonia was being absorbed*; water was again added, yet in insufficient quantity to dissolve the whole of the salt, and the action upon the litmus was gone. In this way, by alternating the addition of the ammonia and the solution of the salt, the presence of the ammonia might be rendered more or less distinct, or unrecognizable. The reality of the absorption was thus rendered undeniable; and it was equally evident that the soluble portion of the salt contained the absorbing agents, since the disappearance of the ammonia bore a direct proportion to the completeness of their solution. It then became a question as to which of the constituents of the soluble portion this agency was due.

Now the chloride of sodium being the largest ingredient, it was desirable at once to ascertain the part which it played in the matter: a saturated solution of the pure substance was therefore prepared, measured quantities of it taken, placed in bottles, and shaken with successive portions of carbonate of ammonia solution until a decidedly blue tinge was produced in the litmus-paper, which, for the purpose of having a standard test, was left enclosed in the atmosphere within the bottle for the space of five minutes. As the amount of ammonia added approached the maximum, its absorption became correspondingly slower, and it was necessary to leave it in contact with the salt solution for periods of from one to twelve hours; a point was, however, always reached at which a certain tinge of colour was communicated to the paper, the intensity of which was not lessened if the test was applied again after a lapse of several days.

The following are the results which I have obtained in experimenting with a saturated solution of pure chloride of sodium in the above manner:—

Experiment.	100 grains of saturated solution of pure chloride of sodium will absorb	Hence 100 grains of solid chloride of sodium will absorb
I.	·0324 grain of NH^4O.	·1222 grain of NH^4O.
II.	·0233 do.	·0881 do.
III.	·0292 do.	·1097 do.
Mean	·0283 do.	·1066 do.

These numbers require a slight correction to make them absolutely true expressions for the absorbing power of chloride of sodium, because, in addition to the trifling excess unavoidably added in order to obtain the blue tint on the litmus-paper (which is so slight that it need not, and indeed could not, be regarded), the water in which the salt is dissolved exercises a certain amount of absorption: this is not, however, much; the quantity of water contained in the above 100 grains of saturated solution masking ·0046 of a grain of ammonia (NH^4O), which would make the true amount absorbed by 100 grains of salt solution, ·0237 of a grain; and that absorbed by 100 grains of solid salt, ·0893 of a grain. This, however, will not affect the subsequent experiments.

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* With regard to the delicacy of the litmus-paper here employed, I may state that a quantity of the ammonia solution containing ·04 of a grain of NH^4O, being placed in an empty bottle, the paper was intensely blued almost immediately after its insertion.

But the amount of ammonia absorbed by this constituent, although really very considerable, and of great importance in an agricultural point of view, did not yet correspond to the apparent absorbing power possessed by the specimens of agricultural salt. The following comparative experiments were therefore made with the view of ascertaining the absolute amount of ammonia which these samples absorbed. The solutions used for these experiments were prepared by digesting great excess of the salt with cold water for some time, so as to approximate the manner of making the solution as closely as possible to that which would occur in nature. The experiments themselves were varied by diluting both the ammonia and salt solutions, without any very great difference of result.

Agricultural Salt, No. I.		
Experiment.	100 grains of the solution of this salt will absorb	Hence 100 grains of the solid salt (contained in the aqueous solution) will absorb
I.	·1479 grain of NH^4O.	·6207 grain of NH^4O.
II.	·1517 do.	·6361 do.
III.	·1517 do.	·6361 do.
Mean	·1504 do.	·6309 do.
Agricultural Salt, No. II.		
I.	·1304 grain of NH^4O.	·4918 grain of NH^4O.
II.	·1349 do.	·5049 do.
III.	·1349 do.	·5049 do.
Mean	·1334 do.	·5005 do.
Agricultural Salt, No. III.		
I.	·1486 grain of NH^4O.	·5533 grain of NH^4O.
II.	·1527 do.	·5685 do.
III.	·1543 do.	·5746 do.
Mean	·1518 do.	·5654 do.

Now as soon as the solution of carbonate of ammonia is dropped into these salt solutions, there occurs (as might have been anticipated) a precipitation of the sulphate of lime which they contain as carbonate; this must be attended with a corresponding fixing of the ammonia, the latter combining with the sulphuric acid to which the lime was previously united; and by this reaction, the immense difference observed between the absorbing power of these solutions and that of a solution of pure chloride of sodium would appear to be accounted for. Whether, then, by its own immediate agency, as in the case of manuring with gypsum, or by the intervention of the absorbent power of the chloride of sodium, the soluble lime-salt present is really the most powerful agent in the absorptive process—a fact which was still further demonstrated by the addition of a few drops of solution of chloride of calcium to some of the pure chloride of sodium solutions, to which, in the previous experiments, the maximum of ammonia solution had been added,—instant absorption, and consequently diminished action on the test-paper, was of course the result.

The absorptive power of the specimens examined ought therefore, if so dependent upon the amount of lime-salt, to be almost in the ratio in which that exists in their soluble portion. The subjoined Table will, however, show a discrepancy in this particular.

Composition of the portion soluble in water, calculated to 100 parts.

	No. I.	No. II.	No. III.
Sulphate of lime	4·234	1·203	1·474
Sulphate of magnesia	·256	·338	·336
Sulphate of soda	3·452	·884	1·773
Chloride of sodium	92·058	97·575	96·417
	100·000	100·000	100·000
Ammonia absorbed by 100 parts of the solid salt.......	·6309	·5005	·5654

If, now, we compare the quantity of sulphate of lime which these salts respectively contain with the amount of ammonia which they are capable of absorbing, we find no such proportion as might have been expected. This arises from the extreme insolubility of the sulphate of lime compared with the solubilities of the other constituents of the salt; and fresh lime-determinations in the solutions actually employed in the absorption experiments showed, that notwithstanding the different composition of the three salts, the amount existing in the solutions prepared as before described was very much the same. The other constituents, from their possessing a comparatively equal solubility with chloride of sodium, cannot be supposed to vary much; and the true composition of the salts contained in the solutions employed, as given below, will be seen to present a very close coincidence between the per-centage of lime-salt and of ammonia absorbed.

Real composition of the salts dissolved for the absorption experiments, calculated to 100 parts.

	I.	II.	III.
Sulphate of lime (determined).	1·356	1·039	1·182
Sulphate of magnesia	·264	·338	·337
Sulphate of soda	3·555	·886	1·779
Chloride of sodium	94·825	97·737	96·702
	100·000	100·000	100·000
Ammonia absorbed by 100 parts of the solid salt.......	·6309	·5005	·5654

We ought, then, to have an index to the absorbing power of a salt by a knowledge of the quantities of chloride of sodium and of soluble lime-salt which it contains; an equivalent of lime-salt fixing an equivalent of ammonia,—and the chloride of sodium, the per-centage which it has been above shown to absorb. Let us now apply this method of determination to the samples under experiment.

No. I.

Total number of grains of ammonia absorbed by 100 grains of salt	·6309
Deduct the absorbing power of 1·356 grain of sulphate of lime	·5187
	·1122
Deduct the absorbing power of 94·825 grains of chloride of sodium	·1000
	·0122

No. II.

Total number of grains of ammonia absorbed by 100 grains of salt	·5005
Deduct the absorbing power of 1·039 grain of sulphate of lime	·3973
	·1032
Deduct the absorbing power of 97·737 grains of chloride of sodium	·1041
	*

No. III.

Total number of grains of ammonia absorbed by 100 grains of salt	·5654
Deduct the absorbing power of 1·182 grain of sulphate of lime	·4519
	·1135
Deduct the absorbing power of 96·702 grains of chloride of sodium	·1030
	·0105

It will be seen that in Nos. I. and III. a slight excess of ammonia remains unaccounted for; this is partly, of course, due to an unavoidable excess which must in every experiment be added; but I have observed, that throughout the operation, even when the absorption is most active, and even after several hours' contact, there is yet a feeble tinge imparted to the litmus-paper when left in the atmosphere within the bottle for five minutes. This would seem to show, that at all stages of the process there is a certain amount of counteraction going on, and it becomes a great question whether this action—which appears trivial when we deal with a comparatively large amount of ammonia, as in these experiments—would not in practice nullify to a great extent, or at least diminish considerably, the absorbent power of this agent, when, spread on the surface of the soil, minute quantities only of ammonia came within the sphere of its action, and the absorption was distributed over a considerable portion of time.

I believe that this counteraction and liberation of ammonia may be traced to the solubility of the carbonate of lime in the absorbing liquid: upon the addition of the first few drops of carbonate of ammonia solution to the solutions of the salts, the small quantity of precipitate which was produced was immediately and perfectly dissolved by agitating the liquid; this was doubtless partly due to the excess of carbonic acid which was evolved from the sesquicarbonate of ammonia, but a considerable portion of the action must be attributed to another cause. It is some time since Professor Connell has shown, that, upon producing carbonate of lime by the action of carbonate of soda upon chloride of calcium in a medium of distilled water, the carbonate of lime so produced (if produced at all) is held in perfect solution[*]. I have found the same action to operate to a much larger extent in the case of a saturated solution of chloride of sodium: taking two solutions of chloride of calcium and carbonate of soda, of which equal bulks contained equivalents, and adding equal portions successively to a known quantity of a saturated solution of pure chloride of sodium, taking care to mix whichever was first added thoroughly with the solution before adding the other, I have obtained the following results, by which it is shown that a saturated solution of chloride of sodium dissolves $\frac{1}{5649}$ of carbonate of lime, or about $5\frac{1}{2}$ times as much as Professor Connell gives as the amount dissolved ($\frac{1}{31523}$) by the common water of St. Andrews. The chloride of sodium was of course perfectly freed from carbonic acid before trying the experiments.

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[*] Phil. Mag. S. 3. vol. xxxi. p. 124.

Experiment.	100 grains of saturated solution of pure chloride of sodium will dissolve	Hence 100 grains of the solid salt will decompose ?
I.	·0165 of a grain of carbonate of lime.	·0623 of a grain of carbonate of lime.
II.	·0181 do.	·0683 do.
III.	·0186 do.	·0703 do.
Mean	·0177 do.	·0669 do.

The addition of the solutions to the chloride of sodium was continued until a slight film of carbonate of lime was deposited upon the mixture standing for half an hour; a deduction was made for the excess added; the numbers therefore represent the amount of carbonate of lime which can be permanently held dissolved by the chloride of sodium. I say this, because I believe that a far larger amount may be retained in solution for a limited time; for if the first trivial precipitate is disregarded, twice or three times as much of the two solutions may be added without producing any such increase of the precipitate as ought to occur, if at that stage the chloride of sodium absolutely refused any further action. But this somewhat curious result I think of prosecuting further. It must not, however, be supposed that it is only on carbonate of lime in the nascent state (if the expression may be allowed) that the chloride of sodium exercises this power; if the two solutions are allowed to mix by pouring them simultaneously upon the surface of the salt solution, so as to produce there a stratum of recently precipitated carbonate of lime, the chloride of sodium will dissolve this with the utmost facility upon agitation. The clear solution may be boiled without any separation of carbonate of lime occurring; it is partly thrown down by boiling with carbonate of ammonia, partly also by boiling with ammonia, and apparently completely by oxalate of ammonia. Whether there exists in this solution chloride of sodium dissolving carbonate of lime, or chloride of calcium not decomposed by carbonate of soda, we have no present means of determining;

although from its partial precipitation by carbonate of ammonia I should almost think that it existed in the latter form; since it is more rational to suppose that carbonate of ammonia, being a less permanent combination than carbonate of soda, should transfer its carbonic acid to chloride of calcium, taking hydrochloric acid in return, than that it should exert any action upon carbonate of lime dissolved as such. The idea of the considerable temporary solubility of recently formed carbonate of lime, receives considerable support from a peculiar phænomenon which took place when I endeavoured to make carbonic acid determinations in the Worcestershire brine-springs. I adopted the ordinary process of adding to the water excess of ammoniacal chloride of calcium,—a very trifling precipitate occurred, which was attributed to the comparative absence of carbonates and carbonic acid; upon examining the bottles at home, however, their interior was covered with most beautiful crystals of selenite, of the most perfect forms,—some complete hemitropes. From their figure and size, these had evidently been deposited slowly; and I do not doubt now that the chloride of sodium at first prevented the precipitation of the carbonate of lime, then that the large quantity of sulphate of soda present parted with its sulphuric acid to the lime so dissolved (by degrees, as the chloride of sodium relaxed its solvent power); the liquid was soon saturated with sulphate of lime, and the surplus was thus gradually deposited. I need not say that the amount of carbonate of lime actually thrown down was quite insufficient to meet the requirements of the analysis; nor is this the only instance in which I have observed a similar result.

The results, then, at which we must arrive are, that agricultural salt is a most energetic absorbent of ammonia, both in virtue of its chloride of sodium and of its soluble lime-salt, and that the proportion of the latter especially most powerfully affects its action;—but that at the same time its agency does not seem to be altogether a permanent one: it will collect the ammonia, but it is questionable whether it can retain it for any great length of time, because in the very decompositions which happen in order to render the ammonia more stable, salts are formed which have a direct tendency to liberate ammonia from its more fixed combinations. It may, however, retain it quite long enough for agricultural purposes: if the young plants are there ready to receive it, its state of gradual liberation may be for them the most advantageous possible; and to this conclusion all experiments on the large scale appear most obviously to tend. It is described as an excellent check to the too forcing power of guano; and from M. Barral's experiment we see that it either prevents the too rapid eremacausis of the latter, or stores up the ammonia as it is formed. As a manure for growing crops, all experience and all theoretical considerations therefore show it to be most valuable; but when employed to mix with manure heaps which have to stand for considerable periods of time, theory would pronounce, as practice has in many cases done, that its power of retaining ammonia under those circumstances is at the best doubtful.

XXII. *An Experimental Inquiry undertaken with the view of ascertaining whether any* Force *is evolved during Muscular Contraction analogous to the* Force *evolved in the Gymnotus and the Torpedo. By* H. F. Baxter, *Esq.*[*]

CONVINCED that a calm inquiry after truth is liable to be lost in controversial disputes, we shall just refer in a general note to the principal sources from whence we have been enabled to obtain views such as are confirmed by experimental evidence, and which are now entertained upon the subject of our present inquiry[†].

The fact long since obtained by Matteucci, viz. that of causing the muscles of a galvanoscopic frog to contract by placing its nerve upon the muscles of another animal during their contraction, is well known; and in his last communication[‡] to the Royal Society, Matteucci has adduced strong reasons for believing that an electric *disequilibrium* is produced during muscular contraction.

By means of the galvanometer, Du Bois-Reymond[§] has satisfactorily shown that an effect occurs upon the needle during muscular contraction. These results have been confirmed by Zantedeschi[||], Buff[¶] and Tyndall[**].

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[*] Communicated by the Author; having been read at the Royal Society May 10, 1855.

[†] *Annales de Chimie et de Physique*, 3^{e} série, vol. xv. p. 64; vol. xxx. pp. 119, 179; vol. xxxix. p. 114. *Comptes Rendus*, vol. xxviii. pp. 570, 641, 653, 663, 782; vol. xxx. pp. 349, 406, 479, 512, 563, 699; vol. xxxi. pp. 28, 91, 318; vol. xxxii. p. 131. *Bibliothèque Universelle de Genève*, Fev. 1850, Juin 1853. On Animal Electricity; being an Abstract of the Discoveries of Emil du Bois-Reymond, &c., edited by H. Bence Jones, M.D., F.R.S. London: Churchill. 1852. 8vo. *Lettre de Charles Matteucci à M. H. Bence Jones, F.R.S.* Florence: Imprimerie Le Monnier, 1853. 8vo. On Signor Carlo Matteucci's Letter to H. Bence Jones, M.D., F.R.S. &c. By Emil du Bois-Reymond. London: Churchill. 1853. 8vo.

[‡] Philosophical Transactions, 1850. Ninth Series.

[§] *Comptes Rendus*, vol. xxviii. p. 641.

[||] *Bibliothèque Universelle de Genève*, Fev. 1850.

[¶] Ibid.

[**] British and Foreign Medical Review for Jan. 1854, p. 141.

The results obtained by Despretz*, Becquerel† and Matteucci‡, have thrown strong doubts upon the conclusions deduced by Du Bois-Reymond. Several experiments undertaken by the author of the present paper, both prior and subsequent to the knowledge of Du Bois-Reymond's researches, tended to confirm in his opinion the doubts expressed by these inquirers. But the positive evidence adduced by Du Bois-Reymond by means of his galvanometer, combined with that of Matteucci obtained by means of the frog, only made it incumbent upon the opponents of Du Bois-Reymond to disprove his conclusions, if capable of disproof, more by stronger experimental evidence than by theoretical arguments; hence the renewal of the present inquiry.

In relating the experiments, to avoid unnecessary prolixity, we shall endeavour to be as concise as possible, incurring, however, some risk of being considered as not having sufficiently eliminated the sources of error. Our great point will be to establish facts; and whatever observations we might think necessary to make in reference to the *origin* of the effect produced, these will form the subject of the concluding remarks, and may be considered as distinct from the main object of the paper. We shall relate in a general manner some results that were obtained by means of a galvanometer consisting of but few coils. To two brass handles of an ordinary medical electro-magnetic machine, were attached thick copper wires, each about 8 inches in length, and bent. Similar copper wires were connected with the galvanometer, their free ends communicating with mercurial cups, these cups forming the means of connexion with the brass handles. Holding the metallic handles firmly, grasping one of them and contracting the muscles of the arm powerfully, and doing this alternately with each arm, we at first failed to obtain any, or if any, but indecisive results. Upon repeating the experiments on another occasion, we ultimately obtained some definite result and a clue to our former failures. It was found that when the muscles of the arms were contracted alternately at definite periods, and continuing this act for some time, that *as the hands became moist* a decided effect upon the needle occurred, indicating the contracting arm to be *positive*§ to the other. We considered that these results were due partly to the reaction of the acid secretions of the hand upon the metallic electrodes, partly to the skin becoming a better conductor than the dry cuticle, and partly perhaps to thermo-electric action.

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* *Comptes Rendus*, vol. xxviii. p. 653.

† Ibid. p. 663. ‡ Ibid. p. 782.

§ If we take an elementary circle, such as zinc, platinum, and dilute acid, the current goes from the zinc, through the fluid, to the platinum, the platinum being the *positive* electrode.

The question now arose, could not the effect which might be due to muscular contraction coincide with that which arose from the action of the cutaneous secretions? From the results obtained by Du Bois-Reymond*, it would appear that the *direction* of the current due to muscular contraction is *inverse*, i. e. *from* the hand *to* the shoulder.

Several other experiments were undertaken in which different solutions were employed to moisten the hands; the effect upon the needle was now much greater. When one hand was moistened with a weak alkaline solution and the other with water, upon contracting the muscles of the arm holding the alkaline electrode, this became *negative* to the other; but by proper management, by not having the solution too strong, we could make this arm become *positive*, the current occurring during muscular contraction overcoming the influence of the alkaline solution. Feeling the force of the objections that might be raised in reference to the use of the metallic electrodes, we do not think it necessary to relate any other experiments made with the use of these instruments.

A galvanometer† of the following construction was now employed. It consisted of two coils, one placed above the other so as to produce the full effect upon both needles, with an index to indicate the amount of deflection. Each coil was 2 inches in length and the same in breadth, consisting of 1500 turns; the thickness of the wire 37 gauge. The needles were rather less than 2 inches in length; the index, of ivory, 3 inches in length. As our object was to ascertain, if possible, the *existence* of the force rather than the *amount*, a further description of its delicacy will not be necessary.

Two copper clamps were made, each 4 inches in length, tapering at one extremity so as to be connected with the binding-screws of the galvanometer, the other extremity being bent down at right angles to the extent of half an inch, presenting a surface 1¼ inch in breadth, and to which was also attached a piece of copper by means of two screws; by these clamps the platinum electrodes were attached, and those that were generally used consisted of platinum foil, each 2½ inches in length and 1 inch in breadth.

The vessels usually employed to hold the solutions were two cupping-glasses, each 2¼ inches in diameter, 2¾ inches in depth, and contained rather more than four ounces of fluid. The glasses being half-filled with a weak solution of common salt, covering the electrodes to the extent of an inch, two and sometimes three fingers of each hand were held perpendicularly in the vessels, the middle finger resting upon the bottom of the glass vessel. When first introduced, a slight tremulous motion of the needle was observed; upon taking the fingers out of one vessel and reintroducing them, and repeating this act, a slight effect appeared, at first definite, but this soon subsided. Similar effects were observed when the fingers of the other hand were used in a similar manner. The fingers last introduced were not always *positive* or *negative* to the others. Keeping the fingers in, then moving one of the fingers so as to make the fluid rise and fall upon the surface of the electrode in one vessel, produced but little motion of the needle. After the fingers had been in for some time, and the needle had become quite stationary, upon contracting the muscles of one arm an effect occurred upon the needle indicating the contracted arm to be *positive* to the other 3°; by contracting the muscles of each arm alternately, the effect amounted to 5°. The result was definite, and the motion of the needle steady.

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* Rapport sur les Mémoires rélatifs aux phénomènes électro-physiologiques présentés à l'Académie par M. E. du Bois-Reymond, *Comptes Rendus*, vol. xxxi. p. 28.

† Vide Faraday's mode of employing a galvanometer. Phil. Trans. Series xxviii. 1852.

Du Bois-Reymond* considers that there are five circumstances as influential in producing a current; viz. 1, température inégale; 2, durée inégale de l'humectation avec le liquide conducteur; 3, tension inégale de la peau; 4, lésion de l'une des places de la peau; 5, transpiration inégale. We could not refer the effects we obtained to either of these circumstances;—to the tension of the skin, for instance.

Three solutions were now prepared; the first consisting of a concentrated solution of common salt; the second of sulphuric acid, one part of strong acid to six parts of water; and the third of one part of liq. potassæ (Pharm. Lond.) to four parts of water.

It was found necessary that the following precautions should be strictly attended to. The solutions should be prepared and well mixed previous to their use, and the electrodes covered to the same extent. The hands and fingers should be thoroughly *clean*; to attain this object they were first washed with the ordinary curd or Windsor soap, and afterwards well rinsed in common water and wiped comparatively dry with a clean towel. The same towels should not be employed when using the different solutions, and it was found better to work on different days with each solution; for the liquids soaking in between the nails and the fingers became a source of some difficulty to remove, and a cause of some embarrassment in judging of the final result.

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* *Bibliothèque Universelle de Genève*, Juin 1853.

We need hardly state that the surfaces of the electrodes should be clean and the contacts perfect.

With the solution of common salt. Upon the first introduction of the fingers, a slight tremulous motion of the needle occurred. Withdrawing the right-hand fingers and reintroducing them immediately afterwards, and repeating this act a few times, a slight *positive* effect was produced upon the introduction of the fingers. Withdrawing the left-hand fingers in the same manner, a slight *negative* effect was at first obtained, but after a short time the effect became indefinite, and ultimately slightly *positive*. Keeping the fingers in and contracting the muscles of the arm, the fingers of the contracted arm were *positive* 4° to 5°, and made to increase.

The solution in one vessel was now diluted to one-half. Upon the first introduction of the fingers, the strong solution was positive 5°. Keeping the fingers in and waiting until the needle had become motionless, the contracted arm was *positive* to the other; if the arm connected with the strong solution was contracted, the effect was greater than when the other was contracted, the current in the former case rising to 5°, in the latter to 2° or 3°.

With the acid solution. Upon the first introduction of the fingers, the needle would sometimes go 30° or 40°, but generally speaking it would oscillate first to one side and then to the other. Withdrawing the fingers of one hand and then reintroducing them, this hand was *positive* to the other. Keeping the fingers quiescent, and contracting the muscles of the arm, the corresponding fingers were *positive* 4°.

The solution in one vessel was diluted to half the strength of the former; the strong solution was *positive* to the other upon the introduction of the fingers. If the difference between the two solutions was not too great (the exact strength being difficult to state), we might determine a slight current to pass in a constant direction, and then make the current arising during muscular contraction overcome this constant current. Strong solutions were found best for this purpose, from their forming, perhaps, a better conducting liquid than the weaker solutions.

With the alkaline solution. Upon the first introduction of the fingers no definite result, the needle tremulous. Withdrawing the fingers and reintroducing them, the effects varied, but, generally speaking, the reintroduction of the fingers produced a *negative* result. The effect due from muscular contraction was definite from 2° to 4°, the fingers of the contracted arm being *positive*.

The solution in one vessel was diluted to half the strength of the other. Upon the introduction of the fingers, the diluted solution was *positive* to the stronger. We were enabled in this instance, as with the acid solution, to obtain a solution of such strength as to give a constant current in a certain direction, and then make the current due from muscular contraction overpower this current. As we consider this fact of some importance, it will be necessary to state the mode of ascertaining this constant current. If we find, either upon the introduction of the fingers of both hands at the same time, or upon the introduction of the fingers of one hand, that a current existed in one direction to the amount of about 2°, the current occurring during muscular contraction, if it coincided with this constant current, would cause the needle to advance to 4° or 5° or more; but if it has to overcome this constant current, the needle would only move 2° or 3°. We need scarcely add, that the needle of the galvanometer should continue to indicate this current whilst the fingers remain in the solution, which may be indicated by the needle receding to its normal position upon the withdrawal of the fingers. A great difficulty is frequently experienced in obtaining this constant current.

Several other experiments might be related in which the electrodes were made to differ in size, one being narrow, the other broad; or where one was made to dip deeper into the solution than the other. The general results indicated that a difference in extent between the surfaces of the platinum electrodes and the solutions, occasioned an effect upon the needle upon the first introduction of the fingers. In other experiments larger vessels were employed, so as to insert the whole of the hand and wrist; the results coincided with those we have related, the effect being greater.

The solutions were made to vary as to temperature by placing the glass in a vessel containing hot water, and then carefully stirring the solution so as to make it completely uniform. One vessel was at the temperature 65° F., and the other 115°. The results upon the first introduction of the fingers were indecisive. The hot solution was not always positive to the cold solution. We could always obtain the effect during muscular contraction.

We agree with Du Bois-Reymond as to the importance of attending to the five circumstances to which he refers, and which we have already alluded to. The circumstances that appear to us to be the most influential in masking the ultimate result, are, —1st, the action of the fluid upon the surface of the electrodes; and 2ndly, the reaction of the cutaneous secretions upon the fluids. Although we cannot remove these sources of error, we have it nevertheless in our power to counteract their effects by showing that the current consequent upon muscular contraction can be made to overcome that arising from either of the two former actions.

We have not yet alluded to the difference which appears to exist between the results obtained by Du Bois-Reymond and ourselves, viz. the *direction* of the current. Du Bois-Reymond's experiments indicate that the current is *inverse*, i. e. *from* the hand *to* the shoulder during muscular contraction; our results, on the other hand, point out that the current is *direct*, i. e. *from* the shoulder *to* the hand. We do not think that our results differ much from those of Du Bois-Reymond; but as the discussion of this question will involve certain theoretical views, whatever observations we shall have to make will be deferred to our concluding remarks*.

With the assistance of three friends an attempt was made to increase the effect upon the needle by forming a pile, as it were; we could obtain the effect, but there was no decided increase.

Several experiments were undertaken with the *rheoscopic frog*† in lieu of the galvanometer; the results were not so satisfactory as could be desired, to justify their being recorded in the present paper.

The posterior limbs of a frog, separated at the pelvis, but connected by means of the lumbar nerves and a portion of the vertebral column, were each placed in separate vessels containing the solution of common salt; the muscles of one limb were then made to contract, and an immediate effect upon the needle was observed, the contracted limb being *positive* to the other from 3° to 4°. To prevent the fluid from being scattered upon the electrode, a piece of glass was attached to the part as a weight to avoid too great motion of the limb. In some instances both feet were removed: the effect upon the needle was still the same.

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* Since this paper was presented to the Royal Society, we have had the opportunity of attending the interesting lectures of Du Bois-Reymond at the Royal Institution. Du Bois-Reymond considers, as far as we could understand, that when the whole of the hands are immersed in the solution, and the muscles of the arm are made to contract, then the current is due to the swollen state of the skin, and perhaps to other circumstances; but should we be justified in concluding, that when the *fingers alone* are in the vessel, then the effect is due to their swollen state? If Du Bois-Reymond maintains that the current due to muscular contraction is *inverse*, i. e. *from* the hand *to* the shoulder, we can only add that we have never been able to obtain that constant effect. We cannot deny that it frequently happens, upon the first repetition of the experiment, that the current may appear in favour of this opinion. Despretz has remarked, that the current may appear first in one direction and then in another. We cannot insist too strongly upon the necessity of having the hands perfectly clean, and we are convinced that the failures and contradictory results which arise are due more to the want of attention on this point than to anything else. The *direction* of the current is a fact of the utmost importance; and as we shall refer to this subject again in the concluding remarks, and as no reasons have occurred to lead us to alter our views, we prefer leaving the paper in its original state. (June 19.)

† The term *rheoscopic* has been recommended in the Report of the Committee of the Academy of Paris, in preference to that of *galvanoscopic*. We have employed both terms. When it is used for the detection of the *current*, the former term is most applicable; but the frog *may* be a test of a force in which the *current* force, in accordance with our present notions of force, cannot be shown to exist. If the *dynamic* condition and the *current* condition of force be considered as equivalent terms, then *rheoscopic* would be unobjectionable.

Similar results were obtained with the *acid* solution, but with the *alkaline* solution the effects varied, especially so if the solution were strong. The alkali acted apparently upon the mucous secretion of the skin.

Great care was required to have the cutaneous surfaces clean previous to the introduction of the limbs into the solutions, so that no current could arise therefrom.

Is it necessary to place the nerve of a galvanoscopic frog in contact with the longitudinal and transverse section of a muscle in order to obtain the necessary contractions? Repeating the experiments of Matteucci*, we found that the contraction might be obtained without placing the nerve in contact with the muscle. At the same time we must add, that the effect upon the galvanoscopic frog appeared greater when its nerve was placed transverse to the muscular fibres than in any other position.

The results of our experiments tend to establish the following conclusions, viz.—

First. That during muscular contraction in man and in frogs, an effect upon the galvanometer may be obtained indicating the manifestation of an electric current.

Secondly. That this manifestation of an electric current is due, in a great measure, to secondary reactions, viz. between the animal secretions and the solutions on the one hand, and between the solutions and the platinum electrodes on the other; but that there nevertheless remains a *residual effect* which we cannot refer to either of these actions, or to those pointed out by Du Bois-Reymond.

Concluding Remarks.

The numerous instances in which an electric current may be shown to exist, naturally renders any conclusion deduced from results obtained by means of the galvanometer extremely doubtful. If these remarks are applicable to physical researches, where we have such control over our experiments as to afford us facilities favourable for eliminating sources of error, how easily may we conceive that they would apply with far greater force to physiological researches devoid of such facilities. The strong prejudices which exist in reference to electro-physiological inquiries cannot therefore be a matter of much surprise, but must create a great difficulty to any individual who shall attempt the elucidation of any electro-physiological problem experimentally; for not only has he to satisfy, from the mixed character of the inquiry, the extreme views of the physicist on the one hand, and those of the physiologist on the other, but they also afford the indolent inquirer a ready means of apparent refutation, and a powerful weapon to the controversialist. That these opinions are not ill-founded we might easily prove*. Feeling the importance of basing the conclusions in these inquiries upon strictly experimental evidence, in discussing the different views that may be entertained in regard to the evidence upon which our experiments justify us in concluding that some force *is* evolved during muscular contraction, we shall confine our observations to the experimental results obtained by Matteucci and Du Bois-Reymond, and endeavour to avoid everything of a purely controversial character.

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* Phil. Trans. 1850. Ninth Series.

Matteucci† has proved and shown by means of the galvanoscopic frog, that during muscular contraction the muscles of the galvanoscopic frog may be excited to contract, and that for this purpose it is not necessary that the nerve should be brought into contact with *different* portions of the muscular fibre. In repeating these experiments, we have been able to confirm the results obtained by Matteucci.

Matteucci‡, in his endeavours to ascertain whether the muscular current was increased during muscular contraction by forming piles with muscular elements, failed to obtain any decisive result. Du Bois-Reymond, by means of his galvanometer, has satisfactorily shown that when the electrodes are so arranged with a muscular element that the current (muscular) passes through the instrument, that then, upon the contraction of the muscle, the needle of the instrument recedes, and this he calls "the negative variation" of the muscular current. This fact, established by Du Bois-Reymond, and also the results obtained by Matteucci, have been confirmed by some of our own experiments. But we cannot help thinking with Matteucci[*], that the falling or the receding of the needle in Du Bois-Reymond's experiment may be attributed in a great measure to the variation in the contacts between the surfaces of the muscle and the electrodes during the act of contraction. We are not aware that the existence of the muscular current is disputed by anyone; it has been attributed to nutritive actions by Matteucci[†], and some experiments of our own[‡] tended to confirm the views of Matteucci. We now have two distinct questions before us:—1st, *Is* there, or is there *not*, any force evolved during muscular contraction? 2ndly, Is the muscular current affected *during* muscular contraction? and this brings us to speak of the results we have obtained in the present paper.

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* Strange to say, it has been urged that the *time* has not yet arrived for the prosecution of these experiments. This is the only tangible objection that the author has yet had an opportunity of refuting. That objections may be started he is perfectly convinced; but as the inquiry professes to be *experimental*, the objections must be supported by experimental evidence before he can notice them.

† Phil. Trans. 1850. Ninth Series.

‡ Ibid. 1845.

Du Bois-Reymond's results lead him to consider that in the frog the current is *direct*, and in man *inverse*, as shown by the galvanometer. Our experiments lead us to believe that the current is *direct* in both instances. Upon carefully looking over Du Bois Reymond's[§] last paper, it appears to us that he frequently obtains the *direct* current; but laying so much stress, as he does, upon the necessity of a *current* (muscular) circulating through the instrument prior to contraction before he can conclude that the resulting action upon the needle is what he calls "the negative oscillation" of the muscular current, that his object was to ascertain whether the muscular current in the human subject was not affected *during* muscular contraction, as in the frog. We may be mistaken in our opinion. As we have not been able to obtain this *inverse* current, and if Du Bois-Reymond has been endeavouring to obtain this *inverse* current to prove "the negative variation" of the muscular current in the human subject, then the object that each of us has had in view differs: ours has been to ascertain whether any force *is* evolved during muscular contraction; Du Bois-Reymond's, whether the muscular current is affected *during* muscular contraction. The importance of Du Bois-Reymond's researches we do not deny; but it appears to us to be of extreme importance that the two questions should be kept perfectly distinct, although perhaps intimately connected with each other.

The results of our inquiries lead us to believe, that during muscular contraction a force *is* evolved, as in the fish, but that it is only during extraordinary muscular exertion that it can become manifest to the galvanometer. We are perfectly aware of the objections that may be urged in reference to the fish being provided with a special apparatus. In our first endeavours also to obtain some result with the galvanometer, and in which we failed to obtain any evidence of the existence of a force being evolved, we were then led to the conclusion as to the improbability of any force becoming free, as it were, during muscular contraction; that whatever force might be present would be expended or converted during the act of contraction; but we could never get over the difficulty which the results of Matteucci, obtained by means of the frog, presented for our consideration, and which is doubly increased by the results we have now related in the present paper. In reference to the fish, also, we must remember that the force evolved bears some relation, according to Matteucci*, "to the activity of the functions of circulation and of respiration, and of every act of nutrition." The apparatus may be a *means* for the evolution of the force, but not a *producer* of the force; and we have some reasons for believing that the electric condition of the blood in the living animal must not be overlooked.

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[*] Phil. Trans. 1850. Ninth Series.

[†] Ibid. 1845, p. 301.

[‡] Proceedings of the Royal Society, Nov. 25, 1852.

[§] *Bibliothèque Universelle de Genève,* Juin 1853.

We have purposely avoided referring in this discussion to the influence and relations which the nervous system must bear to muscular contraction; and as it is our wish that these researches should be considered as strictly experimental, whatever remarks we might wish to make must be deferred to a future opportunity, until circumstances will enable us to pursue the inquiry, and with greater advantages than we now possess.

XXIII. *On the Induction Apparatus and its Effects.*
By J. C. Poggendorff.

[Concluded from p. 137.]

Appendix I.

On the Thermal Effect of the Induction Sparks.

In the foregoing memoir†, it was incidentally remarked that a greater thermic action evidently exists in the stream of sparks at the place of interruption of the induction wire, than in that wire itself. This assertion is based not only upon the facts there recorded, but also upon the prevalent opinion that electric sparks always contain incandescent material particles, in fact that they cannot otherwise be conceived to exist; an opinion which many phænomena I have observed appear to corroborate. Nevertheless it may appear desirable to produce a direct proof of the above assertion, the more so as former observations on the sparks of frictional electricity have led either to no results at all, or only to uncertain ones.

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* Phil. Trans. 1847, p. 241.

† Phil. Mag. vol. x. p. 122.

With the induction sparks obtained without trouble from a continually interrupted current, such a proof is without the least difficulty. It is even furnished in a very perceptible manner by a good mercurial thermometer. With this instrument of course no measurements can be made, although comparative results can be obtained by securing a continuous development of sparks, by means of as uniform a motion and condition of the hammer as can be possibly attained, as well as by paying attention to other precautions which will be easily recognized as necessary.

The following are some of the experimental results which were obtained:—

1. When a delicate thermometer is placed in or close to the stream of sparks of the induction apparatus, a considerable rise immediately takes place, contrasting greatly with the small development of heat in the wire itself. This is the more remarkable, because, according to Joule's law, the heat generated in a given time throughout the whole circuit of the induction current is smaller when the circuit is broken by air than when it is continuous.

2. All other things being equal, this elevation of temperature is different according to the nature of the metals or bodies from which the sparks issue.

In this respect, however, platinum, copper, iron, and silver differ little from one another, although amongst them silver appeared to act strongest, and platinum weakest*. But a great difference exists between these metals and those which are but little coherent, and are easily fused or vaporized. On an average, their action is double that of the former.

The following are the elevations of temperature observed during the space of one minute in a series of experiments with different metallic points. The cylindrical reservoir of the thermometer was 11 millims. long and 3·5 millims. in diameter; its axis was placed between the points from which sparks issued, and at a distance of 2¼ millims. from each.

Platinum	18½° C.
Lead	30½ ...
Tin	33 ...
Antimony	34¼ ...
Zinc	35 ...
Bismuth	37 ...

The difference was still greater when the distance between the points amounted only to 1½ millim., and both were touched by the thermometer. The elevations of temperature within one minute were then with—

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* Graphite alone produced a still smaller degree of heat.

Platinum	23° C.
Copper Iron	24° to 26° C.
Silver	27° C.
Tin	51° C.

When placed near the stream of sparks, though scarcely in contact with it, the thermometer rose 28° C. with tin points.

3. The inequality in the temperature of the two poles of the induction wire may also be clearly shown by the thermometer. When the points were 4·5 millims. apart, the elevation of temperature during one minute was always 6 or 7 degrees more at the negative than at the positive pole, both pole wires being of zinc or tin, and about 1 millim. thick.

4. When the pole wires consist of two different metals, the rise of the thermometer placed in the stream of sparks is greatest when the easily fused and vaporized metal constitutes the negative pole. Within one minute, and when the points were 4·5 millims. asunder, the elevations of temperature were,—

— Pole.	+ Pole.	°
Platinum.	Platinum	18½ C.
Platinum.	Tin	23½ ...
Tin.	Platinum	31 ...
Platinum.	Bismuth	18½ ...
Bismuth.	Platinum	30 ...

5. The greater temperature of the sparks with easily fused and vaporized metals appears to be caused by the volatilization of particles of the same. This is clearly shown by the thermometer suspended in the stream of sparks; when zinc, tin, bismuth, and lead are used, the positive side is coloured white, and the negative brown (at least with bismuth and lead).

6. This volatilization appears also, by improving the conductibility between the poles, to produce a greater transfer of sparks, and consequently a greater intensity of current. It is true the galvanometric deviations caused by the induction wire during the time that sparks are produced are very irregular, so much so that it is exceedingly difficult to determine their magnitude, nevertheless I am inclined to think that the deviations are greater with tin than with platinum points.

7. This suggested a method of proving whether the facility with which sparks pass from one pole to the other is modified by the

nature of the points which may be employed. The method consists in the division of the current between two pairs of points of different metals. The experiments (like the similar ones of Faraday's with different gases) involve some difficulties, and require to be carefully interpreted; because if by accident the stream of sparks should be better conducted through one pair of points than through the other, the former may, by the air becoming warm, and by the volatilization of metallic particles, obtain an excess over the latter which is not natural to it. In general, when the distances between points are equal, sparks which are simultaneously obtained from both pairs of points differ in colour and brilliancy according to the nature of the metals, but apparently they do not differ in the spark-distance. Very often the one or the other stream of sparks ceased for a while without any very apparent cause; on the other hand, however, when zinc, or still better tin, was compared with platinum, it was always found that at small distances (1 or 2 millims.) sparks only passed between the former metals, at greater distances (3 or 5 millims.) only between the latter metal. Without doubt at small distances the volatilized zinc or tin particles conduct so well that they usurp the whole current.

8. The luminous discharges in partial vacuum present similar phænomena to sparks in air. The increase of temperature thereby is very considerable, although not so great as with sparks. Although the strength of the current increases with increasing rarefaction, the elevation of temperature decreases, owing to the decreased resistance.

9. In partial vacuum, too, the inequality in the temperature of the poles is maintained, although it does not appear to be so great as in free air.

In one experiment the air was rarefied to about one inch of mercury pressure, at which time, although the electric light was continuous, it still produced a jingling noise, like that at the commencement of the crackling of the sparks in free air. Two thermometers stood at the distance of about a line from the brass knobs between which the discharge took place, and which were $1\frac{1}{4}$ inch asunder. During one minute the thermometer at the negative pole rose 15° or 15°·5 C., the one at the positive pole 12° or 13° C.

All the above results were obtained by employing the thicker and shorter of the described induction coils, a battery of two of Grove's elements, and the condenser.

APPENDIX II.

On a new method of increasing the Induction Current[*].

Amongst the reasons which induced me to construct Neef's hammer as an independent instrument[†], I may mention that I intended to use it under the air-pump, because I anticipated that the induction current would be influenced by interrupting the inducing current in a partial vacuum, instead of in free air.

Recent experiments have fully satisfied my anticipations, by proving that the partial vacuum increases the action of the current-breaker to such an extent, that in those classes of phænomena wherein the action of the condenser was greatest, the latter may now be dispensed with.

As the effects produced by this manner of using Neef's hammer are in other respects interesting, I may here be allowed to enter into a few particulars.

I placed the hammer upon the extra plate, before described, in an atmosphere of about 1 or 1½ lines of mercury pressure, and connected it with the inducing apparatus in such a manner that the galvanic current could traverse the wires of the primary coil (which were 100 feet long and 1 millim. thick, and encircled a core of soft iron), either alongside or behind one another in the same direction. The current was excited sometimes by one, and sometimes by two of Grove's elements, and allowed to act alternately upon the shorter and upon the longer of my induction wires. Arrangements were made, lastly, to connect the hammer vibrating in the partial vacuum with the large oiled-silk condenser, if necessary.

The *same* hammer was afterwards employed in air under the same circumstances, and also supported by the same condenser, so that its action here could be compared with that which was observed when it worked in a vacuum[‡].

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[*] From Poggendorff's *Annalen* for May 1855, p. 156.

[†] Phil. Mag. vol. x. p. 12.

[‡] In order to make this comparison with two different hammers, it is necessary that they should be exactly alike. Under otherwise equal circumstances, the sparks, and particularly the spark-distance, of the induction coil are very much influenced by the motion of the hammer; it is not so essential that it should vibrate quickly as that it should vibrate with a certain force, *i. e.* that the tongue should separate itself quickly from the pin, and that the amplitude of its oscillation should not be too small. For this purpose it is therefore convenient to have a rather heavy armature at the end of the tongue; in one of my instruments, where the armature is too light, I usually attach a piece of brass to it by means of wax. On the other hand, in order to obtain very strong galvanometric or voltametric actions, when the induction current is interrupted by a piece of dry blotting-paper, it is convenient to employ a light armature, so that the tongue may vibrate very quickly.

As a general result of this comparison, it was quite evident that the action of the *sparks* of the induction coil was in all cases quite as strong when *the hammer worked in a vacuum without the condenser, as when it worked in air with a condenser*; in some cases, indeed, it was considerably stronger.

This was particularly the case with the thicker and shorter induction wire, when the wires of the primary coil were at the same time connected side by side, so as to form a wire 100 feet long and $\sqrt{2}$ millims. in diameter.

When the wires of the primary coil were connected in this manner, the combination of the condenser with the hammer in a vacuum had apparently no influence upon the spark-distance of the induction coil, at least no increasing influence; the sparks were merely stronger sometimes. On the contrary, when the primary wires were connected one after the other, so as to form a wire 200 feet long and 1 millim. in diameter, the combination of the hammer and oiled-silk condenser considerably diminished the spark-distance; a phænomenon which at first sight appeared strange. At the same time, however, a continual crackling could be heard in the condenser. The remarkable phænomenon, therefore, was caused by the extra current, owing to its increased intensity, penetrating the condenser in the form of sparks*.

The luminous phænomena of the induction current in the electric egg are also very well developed when the hammer vibrates in a vacuum without the condenser; apparently just as well as when the hammer works in air, and is connected with the condenser. It is here of importance to what degree the rarefaction is carried; if it amounts to about 1 line of mercury pressure, and at the same time a great intensity is given to the current by developing it in a wire 10,000 feet long, the combination of the condenser with the hammer vibrating in a vacuum is without influence upon the phænomenon; in the opposite case it increases the phænomenon in the same manner as when, at a former time†, the hammer played in free air.

With reference to the cause of the action of the hammer in a vacuum, I will only remark that it appears to me to be of the same nature as that of the condenser.

The vibrations of the hammer in a vacuum are very interesting and instructive in the study of the extra current which is developed in the proper circuit of the galvanic current, partly by the mutual action of the coils of wire, partly also and principally through the influence of the soft iron core in connexion with the interrupting hammer.

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* The condenser is of course perforated by this, so that I have always discontinued the experiment after a very short time. Small holes, however, appear to injure the action of the condenser but little, if at all.

† Phil. Mag. vol. x. p. 127.

For this purpose it is best to remove the induction coil for the time, and to retain only the primary coil in connexion with the galvanic battery and the hammer.

If, in the first place, the wires of the primary coil be connected either side by side, or one after the other, in such a manner that the current must traverse them in opposite directions, instead of in the same direction, as is usually the case, the production of the extra current will be effectually prevented, and on this account only a very small spark of a purely galvanic nature is observed at the point of interruption where the hammer vibrates in a vacuum*. The insertion of a soft iron core into the primary coil has no influence upon this result, for the iron, as will be readily seen, is not magnetized. The addition of a condenser is equally without effect.

Passing from this combination to that wherein the current traverses the wires in the same direction, the influence of the extra current shows itself immediately and in a remarkable manner. The scarcely perceptible galvanic spark expands itself into a luminous phænomenon, whose magnitude is actually surprising when we consider that it is obtained from a wire only 100, or at most 200 feet long. Notwithstanding its magnitude, however, it is explained by the fact, that, previous to its development, the above wire constituted a complete circuit to a current of comparatively great intensity.

In general this phænomenon consists of two parts: a more or less luminous spark at the place where the current is interrupted, and a soft, clear, blue light surrounding the neighbouring parts of the hammer. The formation of both, however, is different according as the galvanic current is excited by one, two, or three of Grove's elements; according as the wires are connected side by side or one after the other, or a soft iron core inserted in the coil or not; and lastly, according as a condenser is used or not. I have generally employed a battery of two of Grove's elements, and connected them with the hammer so that the pin formed the negative pole. Under these circumstances the following was observed.

By combining the primary wires side by side, the spark is pretty strong, but the blue light very weak, provided no soft iron is employed. When the soft iron core is inserted in the coil, the intensity of both increases, but it is the spark which especially gains magnitude and brilliancy, and in these respects is at least three times greater than the spark in free air. It actually assumes the form of a small flame bursting forth on all sides from under the pin, in the midst of which the point of the pin may be distinguished as a brilliant luminous point. The blue light, on the contrary, covers all the neighbouring parts of the pin, which forms the negative pole, with its beautiful brightness, and would undoubtedly spread itself more were the hammer suitably constructed*.

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* This method of connecting the wires is perhaps the only practical means of excluding the influence of induction currents when measuring the velocity with which electricity is propagated; an influence to which must be chiefly ascribed the extraordinary disparity between the results already obtained.

When employing soft iron, the condenser has scarcely any influence upon the phænomenon; without soft iron it weakens the spark a little, and destroys the blue light completely.

The primary wires being connected end to end, the spark becomes smaller and less luminous. The blue light, on the contrary, is more developed, if not more beautifully blue.

When the current is reversed, it is the vibrating hammer which becomes covered with the blue light; this is not only beautiful to observe, but also gives rise to a remarkable occurrence: the anterior part of the tongue between the pin and the armature, which is not traversed by the current, receives a luminous covering.

The above-mentioned phænomena are well calculated to render perceptible the influence of the soft iron core in its several forms. A massive soft iron bar introduced into the primary coil weakens the action which this coil exerts when alone, in exactly the same manner as it decreases the spark when the hammer vibrates in free air. On the contrary, a hollow bundle of wires, whose walls are tolerably thick, is just as effectual as a full bundle, and its action is little or not at all diminished when a solid bar is introduced into the hollow axis. The most effectual means of increasing the luminous phænomena, however, is to place the coil in the middle of a very long bundle of soft iron wires; for this purpose I employed the one before described†, which is three times as long as the coil itself.

The insertion of the induction coil into the primary coil has no influence upon any of the phænomena here described, so long as the former remains unclosed, and does not present a closed circuit in its interior. When closed, however, either by metallic contact or by a well-exhausted electric egg, its insertion weakens the luminous phænomena at the hammer, vibrating in a vacuum very much. This is a very clear proof of the reaction of the induction current upon the extra current.

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* Neef himself allowed the hammer to vibrate in a vacuum (Pog. *Ann.* vol. lxvi. p. 432), but the small intensity of his apparatus did not, of course, permit him to see the phænomenon in its full development.

† Phil. Mag. vol. x. p. 6.

It may also be mentioned, that when the hammer vibrates in a vacuum, and when the parts which beat against one another are of platinum, the latter are very much attacked. The platinum part of the tongue, the brass part above it, the screw of the pin, and the mountings are all greatly blackened. This black powder evidently consists of finely pulverized platinum, which, when the hammer is in activity, can be seen scattered in small sparks from the point of the pin around in all directions.

Owing to this strong action upon the hammer, the above-described method of increasing the intensity of the induction current is scarcely to be recommended in practice; nevertheless, theoretically considered, it is certainly not less interesting than the three methods already known; by the condenser, by allowing the hammer to vibrate under water, and by interrupting the current between the poles of a magnet, which last method was investigated by Rijke.

The action upon the hammer, however, can be considerably diminished by substituting a silver in place of the platinum pin. The sparks then assume a green colour, which is particularly visible in daylight.

XXIV. *On an Electrical Apparatus which acts the part of a Valve.*

By M. J. M. Gaugain.

M. GAUGAIN has recently communicated to the Academy of Sciences in Paris, a Note on an Electrical Apparatus which acts the part of a valve*. The subject has been further examined by M. Riess, an account of whose researches we intend to appear in a subsequent Number of the Philosophical Magazine. M. Gaugain remarks, that there is a numerous class of electric currents which are considered as being formed by the succession of several other currents, with directions alternately opposed to each other. To fix definitely the true constitution of these composite currents, it was thought useful to isolate the partial currents which form them; and to attain this end, he proposed to devise an apparatus which should possess (like a valve) the power of arresting currents having a certain direction, while it should leave to those flowing in an opposite direction perfect freedom of passage. Several combinations which fulfil this condition more or less completely were successively studied; they were based on the known properties of points, and on the experiment of the pierced card. We will here, however, limit ourselves to the description of an apparatus wherewith M. Gaugain has obtained results much more satisfactory than with any other, and which is founded upon an observed fact which he believes to be new.

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* *Comptes Rendus*, vol. xl. No. 12.

Let an ordinary *electric egg** be taken, and let the upper knob be coated with an insulating substance, the stem which supports the knob being similarly coated, leaving naked only an excessively small portion of the surface of the knob: let the egg thus prepared be placed in the circuit of a Ruhmkorff's coil: introducing a galvanometer also into the same circuit, the following facts may be observed:—When the induced currents which correspond to the interruption of the inducing current (the only ones which traverse the vacuum of the egg) pass from the covered to the naked knob, the intensity of the current indicated by the galvanometer goes on increasing as the air within the egg is more and more rarefied; but this is not the case when the induced currents pass through the egg from the naked to the covered knob. In this case the intensity of the current goes on, at first advancing as the pressure is diminished; but when this pressure descends below a certain limit, the deviation of the galvanometer diminishes; for a certain pressure it becomes zero, and finally changes the sign when the vacuum is as perfect as can be obtained with a good air-pump. M. Gaugain limited himself to the establishment of this fact without seeking an interpretation; this sufficed for the object he had in view, namely, to show that the currents traversed the egg freely when they passed from the covered knob to the naked one, and could not pass in the opposite direction, when the vacuum was suitably made. From this it follows that the electric egg, disposed as indicated, can act, in relation to certain currents, the part of a valve with respect to liquids.

M. Gaugain believes that this electric valve may be useful in a certain number of researches, and he has already applied it to the solution of a question raised by M. du Moncel in one of the last communications made by the latter to the Academy of Sciences. When a condenser is interposed in the induced circuit of the apparatus of Ruhmkorff, the motion of the electricity continues, as is proved by the physiological and luminous effects produced on the circuit: but two different hypotheses may be made as to the nature of this movement. It might be supposed that the electricity propagates itself across the insulating film of the condenser in the same manner as it passes through a conducting body, and in this case its direction would be constantly the same; it might, on the contrary, be supposed that the two electricities, developed by the induction apparatus, accumulate on the two surfaces of the condenser during the time the electromotive force is active, and that they recombine as soon as this force ceases to act. According to this latter hypothesis, the current ought to pass in opposite directions alternately; the rigorous discussion of the facts M. Gaugain believes sufficient to decide which of those hypotheses is the true one; but the question may be resolved in a decisive manner by means of the electric valve.

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* A glass receiver shaped like an egg.

To render our language definite, let us suppose the condenser employed to be a Franklin's plate, placed horizontally, and that its inferior surface is in communication with the negative pole of the induced circuit of the induction apparatus; let two different communications A and B be established between the positive pole of the apparatus and the superior armature of the condenser: in each of these portions of the circuit let a galvanometer and valve be introduced, and let the two valves be disposed in such a manner that the currents can pass in the circuit A from the pole to the condenser, and that, on the contrary, they can only pass in the circuit B when they are directed from the condenser towards the pole: it is easy to see what must take place acording to each of the hypotheses between which it is our wish to decide. If the direction of the currents be constant, they will pass exclusively in the circuit A, or exclusively in the circuit B; if, on the contrary, the motion of the electricity consists of a succession of two currents alternately opposed to each other, the two circuits A and B will be traversed simultaneously by the opposed currents; and the direction of these currents, determined only by the disposition of the valve, will be independent of the direction of the inducing current. Now it is in this latter way that the facts exhibit themselves; the existence of currents which traverse at the same time the circuits A and B may be proved, either by the simultaneous appearance of light in the electric eggs, or by the deviation of the galvanometers. The intensities of the two currents differ very little from each other; this may be inferred from the following numbers: in one of the experiments the deviation corresponding to the current which charged the condenser was 63 degrees; the deviation due to the current which discharged the condenser was 61 degrees. It evidently results from this experiment, that the motion of the electricity in a circuit interrupted by the interposition of an insulating film is formed by the succession of two alternate currents.

In a recent number of the *Comptes Rendus*, M. Gaugain has described further experiments of this nature; we shall take occasion to refer to them in an early Number of this Journal.

XXV. *On the Law of Electro-chemical Equivalents.*

By M. I. Soret[*].

In a previous memoir I have shown that equal weights of copper are obtained, by submitting to the action of the same current different electrolytes in which copper forms the positive element, or what is called the *cation*. I proposed also to study the different electrolytes of which hydrogen is the electro-positive radical, and had even commenced some experiments; but I have given up this investigation, which has become useless since M. Despretz published his researches upon this subject, and announced his intention of pursuing them. I shall here limit myself to the relation of the following observation upon the decomposition of water.

When distilled water is submitted to the action of a somewhat powerful battery (sixty small Grove's elements), the gaseous bubbles which are disengaged at one of the electrodes are attracted by the other electrode, probably in consequence of the great difference of tension of the electricity at the surface of the two poles. The molecules of gas, and perhaps of water, electrized by their contact with one of the poles, discharge themselves upon the other.

It is possible that, by this *mechanical* transfer, if I may call it so, there may be a passage of a certain quantity of inefficacious electricity, which will necessarily be greater the nearer the electrodes are to each other. But I do not know whether the amount of this may become considerable.

The same thing does not take place in acidulated water, the conductibility of which is much greater, because the difference of tension of the two electrodes is much less considerable.

To return to the law of electro-chemical equivalents. We may, I think, consider that the above-mentioned researches have demonstrated, that, in taking separately the groups of different electrodes from which the current separates the same electro-positive radical or *cation*, the law of Faraday is exact, within the limits of errors of observation. Buff has proved a second point; he has shown with nitrate of silver that the point of silver separated is exactly in proportion to the quantity of electricity which traverses the electrolyte, whatever may be the intensity of the current[†].

To complete the verification of Faraday's law, it is necessary also to show that a similar current traversing electrolytes, the electro-positive elements of which are different, separates weights of these elements proportionate to their chemical equivalents.

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[*] From the *Comptes Rendus* for August 6, 1855, p. 220.

[†] Liebig's *Annalen*, vol. lxxxv. p. 1.

It is with this view that I have undertaken the following experiments, by means of which I have compared the quantities of copper, hydrogen, and silver separated by the current. I have also tried some experiments upon lead, but they did not lead to a satisfactory result.

I. *Comparison of the quantities of copper and hydrogen separated by the same current.*—The comparison of the quantities of copper and hydrogen separated by electrolysis has been effected by means of a somewhat complicated apparatus, which it would be difficult to describe completely without the aid of a figure. I shall therefore confine myself to indicating briefly the mode in which I have operated.

An electrical current traversed simultaneously a solution of sulphate of copper, and some acidulated water. The copper deposited on a platinum wire was weighed directly. The hydrogen was determined by combustion, as in an organic analysis.

In preparing the sulphate of copper, in its electrolysis and the weighing of the deposit, I operated exactly in the same manner as in the researches upon the salts of copper previously referred to. Considerable difficulty is met with in the exact determination of the quantities of water decomposed by the pile. In fact it is to be feared that a portion of the gas set free combines afresh. The circumstances which may be in favour of this recombination are,—1, the mixture of the hydrogen and oxygen in the presence of the platinum wires, especially if a part of the oxygen is in the state of ozone; 2, the contact of the hydrogen dissolved in the water with oxygen in the nascent state; 3, the formation of binoxide of hydrogen, that is to say, of a very oxidizing body which may burn the hydrogen. These causes of error are avoided principally by separating the gases from the moment of their formation, and raising the temperature of the electrolyte. The best mode of separating the gases appeared to me to consist in plunging the electrodes in two separate test-tubes, united by a siphon filled with acidulated water. They were kept at a temperature of 140° to 158° F. The hydrogen disengaged from one of these test-tubes passed first through a series of desiccating apparatus, and then traversed a combustion-tube. The water was absorbed in weighed tubes. A current of atmospheric air which traversed the apparatus at the same time, served to draw the whole of the hydrogen into the combustion-tube at the conclusion of the experiment, and to prevent the detonations which would inevitably have been produced if the proportion of hydrogen had been considerable.

I made a certain number of preliminary experiments to determine the strength of the pile which it would be necessary to employ, and to ascertain the limit of exactitude in the observations;

I believe that we cannot hope in these experiments to measure the quantity of water absorbed with certainty within 0·001.

The definitive experiments were made with twenty Bunsen's elements. 395·6 was taken as the equivalent of copper, and 112·5 as that of water. The results are given in the following Table:—

Numbers of the experiments.	Duration of the experiments.	Weight of copper deposited.	Weight of water absorbed.		Absolute differences.	Relative differences.
			Calculated.	Found.		
	hours.	gr.	gr.	gr.		
1	6	0·4133	0·1176	0·1180	+0·0004	$+\frac{1}{295}$
2	6	0·4856	0·1381	0·1382	+0·0001	$+\frac{1}{1382}$
3	12	1·0133	0·2882	0·2871	−0·0011	$-\frac{1}{261}$
			0·5439	0·5433	−0·0006	$-\frac{1}{905}$

It appears that the absolute differences do not exceed the limits of exactitude to be expected. The greatest difference is $\frac{1}{261}$ of the total quantity of water absorbed, and the average relative difference only extends to $\frac{1}{905}$. Faraday's law is therefore verified by these experiments.

II. *Comparison of the quantities of silver and copper separated by the same current.*—The comparison of the weights of copper and silver separated by the action of the pile is much easier. The electrolytes which I have employed are pure sulphate of copper and nitrate of silver.

The electrolysis of the sulphate of copper was effected as in the preceding experiments, and that of the nitrate of silver nearly in the same manner. A neutral solution of nitrate of silver was placed in a tube closed at one end. In this a platinum wire was immersed as a negative electrode, and a silver wire as the positive one. The silver deposited was washed with distilled water, then left to dry in the open air, and weighed. My first experiments did not present complete exactness, from certain causes which were afterwards avoided. 1350 was taken as the equivalent of silver. The following Table contains the results of the final experiments:—

Numbers of experiments.	Duration of the experiments.	Weight of silver deposited.	Weight of copper deposited.		Absolute differences.	Relative differences.
			Calculated.	Found.		
	hours.	gr.	gr.	gr.		
1	$1\frac{1}{4}$	0·8781	0·2573	0·2572	−0·0001	$-\frac{1}{2572}$
2	,,	0·3999	0·1172	0·1168	−0·0004	$-\frac{1}{292}$
3	8·20	1·6084	0·4713	0·4708	−0·0005	$-\frac{1}{942}$
			0·8458	0·8448	−0·0010	$+\frac{1}{845}$

The absolute differences may be entirely referred to possible errors in the weighing. We may observe that the values of the chemical equivalents themselves are not determined with greater accuracy, for if we only admit 395·1 as the equivalent of copper instead of 395·6, the law is mathematically verified by the average of these experiments.

Conclusion.—The weights of copper, hydrogen and silver, separated by the same electrical current, are in proportion to the chemical equivalents of these bodies, and Faraday's law is verified within the limits of errors of observation.

XXVI. *Notices respecting New Books.*

A Treatise on the Calculus of Operations. By the Rev. ROBERT CARMICHAEL, *A.M., Fellow of Trinity College, Dublin, and Member of the Royal Irish Academy.* London: Longman, Brown, Green, and Longmans. 1855.

THIS is a work which will be welcome to every mathematical student, for it will, in a great measure, supply a want which has long been felt. The Calculus of Operations ranks amongst the most important improvements which modern mathematicians have introduced. It originated in a profound investigation of the essential nature of all existing operations, whereby it was found, that, although they differ considerably in their objects, they have nevertheless so much in common that the results obtained by one operation may, with a few modifications, be translated into the language of another; that each, in fact, is a particular interpretation of far more general results, which are susceptible of an unknown number of interpretations.

A wide field of discovery is thus opened to the mathematician. Not only are operations in differential calculus, in integral calculus, and in the calculus of finite differences, in some cases, immensely facilitated, but an intimate relation between these operations is established, and the key obtained to still higher and more complicated ones.

Such being the importance of the subject, no doubt can be entertained that the calculus of operations might be introduced into our elementary course of instruction at a far earlier period than it is at present; but in order to do so, its elementary principles must be first demonstrated with all that lucid precision and completeness which is necessary to interest and carry conviction to the young student, and thus enable him to employ the calculus with confidence and facility. This is scarcely the object of the book under review. The author has furnished an admirable text-book to those who are already, to some extent, acquainted with the calculus. He has studiously avoided "all prolixity of detail as alike unnecessary and wearisome" to them, but we may add, important and *not necessarily* wearisome to the less advanced student. To such intentions, of course, no one can take objection; and the less so, as in this instance they have been faithfully executed. The researches of other mathematicians on the subject have been carefully collected, and not unfrequently considerably improved; besides which the author has added much of his own, and treated the subject throughout with great ability and elegance. Nevertheless it is worthy of consideration, whether, by expanding the second chapter on 'Elementary Principles,' and by entering more fully into those details of which we have before spoken, the sphere of the book's usefulness could not be considerably increased.

The Elements of Plane Practical Geometry, with Illustrative Applications. By E. W. DALLAS, *F.R.S.E.* London: John W. Parker and Son, West Strand. 1855.

The author's object in writing this work was "not so much to educate mathematicians as to provide the ordinary workman with an intelligent reason for many of those problems of geometry that occur in the operations in which he is daily employed, and a knowledge of the principles of which must tend to correctness and excellence of execution." It is a difficult task to avoid embarrassing such a class of readers with too much theory, and at the same time to preserve the necessary mathematical precision and sequence. This task the author has accomplished with praiseworthy skill; for although a mathematician would certainly detect imperfections, they are of such a nature as not to diminish the value of the book to those for whose perusal it was written.

The number of theorems in the first part is as small as the 'workman' can desire, and they are demonstrated concisely and clearly. The practical problems in the second part are, for the most part, well chosen and skilfully solved. Amongst them the mathematical student will recognize many 'deducibles' whose solutions have cost him much labour, and he will certainly find it instructive to compare his own solutions with those here given. He must not forget, however, that in his case the principal value of these problems is to be found in *solving* them, rather than in knowing their solutions.

The 'observations and applications' appended to many of these problems are not so skilfully chosen as we could desire; they are intended to "afford some indication to the reader of the very general application of geometry," but they do so very inadequately, and it it is quite possible that their perusal may be a source of some disappointment to the ardent student. Many of them he will certainly find interesting, others superfluous, because he would undoubtedly have made them for himself; others, again, he may find trivial, and a few, such as the remark about diverging rays in the sky (p. 65), are of doubtful accuracy. Nevertheless the work will be a welcome one to those for whom it was intended; and the fact that it was undertaken at the suggestion of the Honourable the Board of Trustees for the Encouragement of Manufactures, &c. in Scotland, is a good guarantee that such a work was needed.

Recent Cambridge Mathematical Works.

A Treatise on the Differential Calculus, and the Elements of the Integral Calculus, with numerous Examples. By I. TODHUNTER, *M.A., Fellow of St. John's College, Cambridge.* Cambridge: Macmillan and Co. 1852.

A Treatise on Analytical Statics, with numerous Examples. By I. TODHUNTER, *M.A. &c.* Cambridge: Macmillan and Co. 1853.

Elementary Mechanics. By J. B. PHEAR, *M.A., Fellow and Mathematical Lecturer of Clare Hall, Cambridge.* Cambridge: Macmillan and Co. 1850.

Elementary Hydrostatics, with numerous Examples. By J. B. PHEAR, *M.A. &c.* Cambridge: Macmillan and Co. 1852.

Arithmetic and Algebra, in their Principles and Application, with numerous systematically arranged Examples, taken from the Cambridge Examination Papers, with especial reference to the ordinary Examination for B.A. degree. By BARNARD SMITH, *M.A., Fellow of St. Peter's College, Cambridge. Second Thousand, with an additional Appendix.* Cambridge: Macmillan and Co. 1854.

The first thing that strikes us in all these publications of Messrs. Macmillan and Co., is the beautiful way in which they are all printed and "got up." Those which contain diagrams and figures have them on the same page as the text, which is a very great convenience to the reader. Most mathematical books of late years have all the plates at the end of the volume (for the sake of cheapness), an arrangement which gives constant trouble and annoyance to the student. We would gladly pay a shilling or two extra, if necessary, to save all this trouble of turning to the plates every time the figure has to be looked at. We come now to the merits of the works as text-books on their respective subjects.

The 'Differential Calculus' of Mr. Todhunter is an excellent one, and has already taken its place at Cambridge as *the* text-book on that subject. We are far from thinking that the author has done all that can be done towards making the principles of the Calculus clear and satisfactory; but he has done a great deal. We can strongly recommend the work to all students, whether at Cambridge or elsewhere.

The 'Analytical Statics,' by the same author, is, as he states in the preface, little more than a reprint of those portions of Pratt's 'Mechanical Philosophy' which treat of the same subject. The well-known work of Pratt has been the text-book at Cambridge on all the subjects, almost, of which it treats (from elementary mechanics up to the planetary theory), ever since its first publication. And few books have ever deserved their reputation better. It is a first-rate book, and we cannot understand why it should be allowed to go out of print, as it now is. The publishers, indeed, announce their intention of issuing the remaining portions of the work under other editors, which may certainly afford an opportunity of improving the book in various respects (for instance, placing the diagrams on the same page as the text instead of at the end); but the whole work will thus become much more expensive. There is one argument in favour of this arrangement, viz. that it will enable students to purchase only those parts which they immediately want; and those, for example, who only want the elementary subjects, need not buy the part on the lunar and planetary theories, &c.

Notwithstanding the typographical beauty of this new edition, there are misprints and errors in the examples which the editor will probably correct in a future edition. The additional examples form, indeed, the chief recommendation of this new edition over the original; but the greatest care ought to be bestowed to ensure the perfect *accuracy* of these examples, which otherwise will only serve to confuse and perplex the student, especially if he has no tutor. The few additions to the original text of 'Pratt' are derived chiefly from MSS. which have long been familiar to the Cambridge student.

Mr. Phear's 'Elementary Mechanics' is also a good elementary text-book. The examples are numerous, and apparently well chosen. We extract two simple 'questions,' one from his 'Mechanics,' and the other from his 'Hydrostatics,' as good specimens of a *class* of questions which we should be glad to meet with more frequently in examinations and books:—

"A shopkeeper has a false balance, and thinks to make his customers' consequent losses and gains balance each other by weighing the goods which he sells alternately in the one scale and in the other; does he succeed?" (Mechanics, p. 107.)

"Is it advantageous to a buyer of diamonds that the weighing of them should be made when the barometer is high or when it is low, supposing their specific gravity to be less than that of the substance used as the weight?" (Hydrostatics, p. 39.)

The great fault of all Cambridge mathematical books is, that they do not bear sufficiently on *practical* subjects. Ingenious problems are framed, having no earthly connexion with any practical purpose or application, whilst hundreds of questions might be proposed quite as good in the way of illustrating mathematical principles and calculations, and much more *useful*. Engineers and other practical men would often be led to consult mathematical works, and to study mathematics, if they found *what they wanted* in such a course; but unfortunately they find scarcely anything of practical utility to them. Cambridge men have themselves acknowledged this defect in their system, but few have done anything towards correcting it. The following quotation from Whewell's preface to the first part of his 'Dynamics' (published in 1832), is just as applicable now as ever: "Analytical as well as geometrical speculations may be unprofitable. The really important applications of mathematics are so numerous, that it is by no means desirable to employ the student's time on detached and useless problems. . . . . . There can be no doubt of the advantage which students derive from working out the results of these and similar problems as exemplifications of the application of their principles. But those who would really use their mathematical acquirements for the improvement of their fellow-students in this place, may easily find better subjects for their skill. There are at present a number of branches of natural science to which mathematical calculation has been so far successfully applied, that they might form a portion of the subjects of study here, if we had clear and convenient treatises upon them." (Preface, pp. xvii and xviii.)

We may also quote the following from a really practical work by one of the most distinguished Cambridge men of the present day, whose example, it is much to be regretted, has not been followed by more men of his own university:—

"I am not without hopes, that, in addition to its principal object of giving a scientific and systematic form to its subject, the results of the volume which I now venture to present to the world may be found a useful addition to mathematical studies in general, by affording simple illustrations of the application and interpretation of formulæ, and by suggesting new subjects for problems, and for further investigation." (Willis's 'Principles of Mechanism,' Preface.)

Mr. Phear's 'Hydrostatics' is not sufficiently copious and explanatory. He (like so many other Cambridge writers) has sacrificed clearness to brevity. But there are some good examples, and the work, like all the others, is admirably printed.

Of Mr. Barnard Smith's 'Arithmetic and Algebra,' we need say no more than repeat the encomium of Dr. Peacock, and recommend it as the best book we know on the subject. We may state that the 'Arithmetic' is published separately for the use of schools.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 149.]

May 24, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On a Decimal Compass Card." By James M. Share, Esq., Master R.N.

The mariners' compass-needle having of late years received great improvements, I am of opinion it is high time the card, as at present arranged, should take its place by the side of such things as are superseded by others better adapted to the advancing spirit of the times.

I venture to make an attempt to innovate on an old custom, by suggesting the substitution of a compass card containing thirty-six points of ten degrees each—every degree being one-tenth of a point.

By the use of this card the mariner will avoid the constantly recurring trouble of turning degrees into points, and *vice versâ*.

The ship's course having been worked out in degrees, the deviation and local attraction have but to be applied to adapt it to the decimal steering card, thus rendering the "traverse table for points" no longer necessary to those steering by it; the course N. 35° E. being the same as "north three and a half points east," &c. The same remark applies also to astronomical bearings, azimuths, amplitudes, &c.

Should the decimal card be adopted, the old-fashioned method of "boxing the compass," which takes young people so long to become familiar with, will be entirely superseded, and I think the sooner such method becomes obsolete the better it will be for the interests of the mariner, for, together with other advantages, the tedious operation of a "day's work" will be divested of half the usual trouble.

When giving a course to the "quarter-master," or "man at the wheel," no mistake, so liable to be the case at present, can well occur; it will merely be necessary to direct him to steer, for instance, "north five points east," or more briefly, "north five east," "south six west," &c. &c.

I recollect an instance of a vessel steering N.W. by N. ½ N., instead of W. by N. ½ N. during thick weather in the Bristol Channel, thus running into danger from the similarity of sound between the courses alluded to.

The practical application of the decimal card would not materially affect the charts previously published, which could have printed compasses containing thirty-six points pasted over the others. Such might be sold by any chart-seller.

"Observations on the Human Voice." By Manuel Garcia, Esq.

The pages which follow are intended to describe some observations made on the interior of the larynx during the act of singing. The method which I have adopted is very simple. It consists in placing a little mirror, fixed on a long handle suitably bent, in the throat of the person experimented on against the soft palate and uvula. The party ought to turn himself towards the sun, so that the luminous rays falling on the little mirror, may be reflected on the larynx. If the observer experiment on himself, he ought, by means of a second mirror, to receive the rays of the sun, and direct them on the mirror, which is placed against the uvula. We shall now add our own deductions from the observations which the image reflected by the mirror has afforded us.

Opening of the Glottis.

At the moment when the person draws a deep breath, the epiglottis being raised, we are able to see the following series of movements:—the arytenoid cartilages become separated by a very free lateral movement; the superior ligaments are placed against the ventricles; the inferior ligaments are also drawn back, though in a less degree, into the same cavities; and the glottis, large and wide open, is exhibited so as to show in part the rings of the trachea. But unfortunately, however dexterous we may be in disposing these organs, and even when we are most successful, at least the third part of the anterior of the glottis remains concealed by the epiglottis.

Movement of the Glottis.

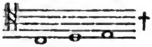
As soon as we prepare to produce a sound, the arytenoid cartilages approach each other, and press together by their interior surfaces, and by the anterior apophyses, without leaving any space, or intercartilaginous glottis; sometimes even they come in contact so closely as to cross each other by the tubercles of Santorini. To this movement of the anterior apophyses, that of the ligaments of the glottis corresponds, which detach themselves from the ventricles, come in contact with different degrees of energy, and show themselves at the bottom of the larynx under the form of an ellipse of a yellowish colour. The superior ligaments, together with the aryteno-epiglottidean folds, assist to form the tube which surmounts the glottis; and being the lower and free extremity of that tube, enframe the ellipse, the surface of which they enlarge or diminish according as they enter more or less into the ventricles. These last scarcely retain a trace of their opening. By anticipation, we might say of these cavities, that, as will afterwards appear clearly enough in these pages, they only afford to the two pair of ligaments a space in which they may easily range themselves. When the aryteno-epiglottidean folds contract, they lower the epiglottis, and make the superior orifice of the larynx considerably narrower.

The meeting of the lips of the glottis, naturally proceeding from the front towards the back, if this movement is well managed, it will allow, between the apophyses, of the formation of a triangular space, or inter-cartilaginous glottis, but one which, however, is closed as soon as the sounds are produced.

After some essays, we perceive that this internal disposition of the larynx is only visible when the epiglottis remains raised. But neither all the registers of the voice, nor all the degrees of intensity, are equally fitted for its taking this position. We soon discover that the brilliant and powerful sounds of the chest-register contract the cavity of the larynx, and close still more its orifice; and, on the contrary, that veiled notes, and notes of moderate power, open both so as to render any observation easy. The falsetto register especially possesses this prerogative, as well as the first notes of the head-voice*. So as to render these facts more precise, we will study in the voice of the tenor the ascending progression of the chest-register, and in the soprano that of the falsetto and head-registers.

Emission of the Chest-voice.

If we emit veiled and feeble sounds, the larynx opens at the notes

do, ré, mi,  †;
2 2 2

and we see the glottis agitated by large and loose vibrations throughout its entire extent. Its lips comprehend in their length the ante-

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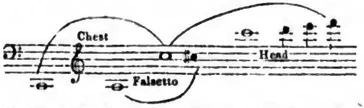
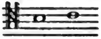
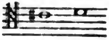
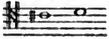
[*] Let us here observe, that three registers of voice are generally admitted,—chest, falsetto, and head. The first begins lower in a man's voice than in a woman's; the second extends equally in both voices; the third reaches higher in the female voice.

Table of the Human Voice in its full extent.

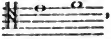


[†] The musical limits we establish in the course of these pages vary a little in each individual.

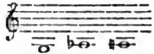
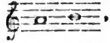
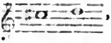
rior apophyses of the arytenoid cartilages and the vocal cords; but, I repeat it, there remains no triangular space.

As the sounds ascend, the apophyses, which are slightly rounded on their internal side, by a gradual apposition commencing at the back, encroach on the length of the glottis; and as soon as we reach the sounds si$_3$, do$_3$, , they finish by touching each other throughout their whole extent; but their summits are only solidly fixed one against the other at the notes do♯$_3$, ré$_3$, . In some organs these summits are a little vacillating when they form the posterior end of the glottis, and the two or three half-tones which are formed show a certain want of purity and strength, which is very well known to singers. From the do♯$_3$, ré$_3$,  the vibrations, having become rounder and purer, are accomplished by the vocal ligaments alone, up to the end of the register.

The glottis at this moment presents the aspect of a line slightly swelled towards its middle, the length of which diminishes still more as the voice ascends. We also see that the cavity of the larynx has become very small, and that the superior ligaments have contracted the extent of the ellipse to less than one-half.

When instead of veiled and feeble sounds, we make use of full and vibrating ones, the glottis becomes visible only at the sounds mi$_3$, fa$_3$, , and those above them, a limit which depends to a certain extent on the dexterity of the singer. For all the rest, the organs act as we have just said, but with a double difference: 1. The cavity of the larynx contracts itself more when the voice is intense, than when it is feeble. 2. The superior ligaments are contracted so as to reduce the small diameter of the ellipse to a width of two or three lines. But however powerful these contractions may be, neither the cartilages of Wrisberg, nor the superior ligaments themselves, ever close sufficiently to prevent the passage of the air, or even to render it difficult. This fact, which is verified also with regard to the falsetto and head-registers, suffices to prove that the superior ligaments do not fill a generative part in the formation of the voice. We may draw the same conclusion by considering the position occupied by the somewhat feeble muscles which correspond to these ligaments; they cover externally the extremity of the diverging fibres of the thyro-arytenoid muscles, and take part especially in the contractions of the cavity of the larynx during the formation of the high notes of the chest- and of the head-registers.

Production of the Falsetto.

The low notes of the falsetto,  sol$_2$, la♭$_2$, la♮$_2$, show the glottis infinitely better than the unisons of the chest-voice and produce vibrations more extended and more distinct. Its vibrating sides, formed by the anterior apophyses of the arytenoid cartilages, and by the ligaments, become gradually shorter as the voice ascends; at the notes la$_3$, si$_3$, , the apophyses take part only at their summits; and in these notes there results a weakness similar to that which we have remarked in the chest-notes an octave below. At the notes do♯$_4$, ré$_4$, , the ligaments alone continue to act; then begins the series of notes called *head-voice*. The moment in which the action of the apophyses ceases, exhibits in the female voice a very sensible difference at once to the ear and in the organ itself. Lastly, we verify, that, up to the highest sounds of the register, the glottis continues to diminish in length and in width.

If we compare the two registers in these movements, we shall find some analogies in them: the sides of the glottis, formed at first by the apophyses and the ligaments, become shorter by degrees, and end by consisting only of the ligaments. The chest-register is divided into two parts, corresponding to these two states of the glottis. The register of falsetto-head presents a complete similarity, and in a still more striking manner.

On other points, on the contrary, these same registers are very unlike. The length of the glottis necessary to form a falsetto note, always exceeds that which produces the unison of the chest. The movements which agitate the sides of the glottis are also augmented, and keep the vibrating orifice continually half opened, which naturally produces a great waste of air. A last trait of difference, is in the increased extent of that elliptic surface.

All these circumstances, which we shall refer to again, show in the mechanism of the falsetto, a state of relaxation, which we do not find in the same degree in the chest-register.

Manner in which the sounds are formed.

As we have just said, and what we have seen proves it, the inferior ligaments, at the bottom of the larynx, form exclusively the voice, whatever may be its register or its intensity; for they alone vibrate at the bottom of the larynx*. But by virtue of what principle is the voice formed? It seems to me, that the answer to this question can be but this; the voice is formed in one unique manner,—*by the compressions and expansions of the air, or the successive and regular explosions which it produces in passing through the glottis.*

The ligaments of the glottis are situate about the mean level of the upper border of the cricoid, close the passage, and present a resistance to the air. As soon as the air has accumulated sufficiently, it parts these folds and produces an explosion. But at the same instant, by virtue of their elasticity, and the pressure from below being relieved, they meet again to give rise to a fresh explosion. A series of these compressions and expansions, or of explosions, occasioned by the expansive force of the air and the reaction of the glottis, produces the voice.

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* We gladly acknowledge that this most important fact has been already announced by J. Müller, although we have our objections to the theory which accompanies it.—*Handbuch der Physiologie des Menschen.*

This theory, though now generally admitted for reeds, and undoubtedly evident in the liquid vein, the toothed-wheel of Savart, the syrène of the Baron Cagnard Latour &c., has not to my knowledge, been yet applied to the glottis*. If we consider that the lips of this aperture, taken separately, can give no kind of sound, however we may try to make them speak, we must admit that the sounds which they give forth by their mutual action, are only owing to the explosions of the air produced by their strokes†. It is not necessary in order to obtain the explosion of sound, that the glottis should be perfectly closed each time after its opening; it suffices that it should oppose an obstacle to the air capable of developing its elasticity. In this case the rushing of the air is heard accompanying the sounds, and they take a veiled, and sometimes an extremely muffled character; an observation which we have already presented to the reader's notice in speaking of the falsetto.

Conjectures on the Formation of the different Registers.

As the entire system of vibrations arises solely from the inferior ligaments, it is evident that the cause of the different tones called registers, must be sought for in the muscles which set these ligaments in motion; and that the other parts of the larynx must be considered only as apparatus for strengthening the sounds obtained, and for modifying their quality. In our efforts to discover the more intimate processes of the vocal organs which produce the sounds, we shall recur at once to the observations already mentioned, to some anatomical remarks which we are going to make, and to the sensations which we feel in the organ itself whilst it is producing sounds.

If we detach one of the halves of the thyroid cartilage, we shall see a large muscular surface of oblique fibres, which fills all the space between the arytenoid and thyroid cartilages. At its upper end is to be seen the muscle corresponding to the superior vocal ligaments, and which sometimes extends to the notch in the thyroid. After detaching this generally frail muscle, all the fibres constituting this muscular surface seem to start from two opposite centres, viz. the anterior surface of the arytenoid, and the re-entering angle of the thyroid. These centres, occupying the extremities of a diagonal line, send their fibres towards each other in parallel lines. Those which start from the anterior face of the arytenoid descend obliquely; the most external ones go to the cricoid, whose posterior half they cover at the side; the most internal ones descend to the vocal membrane‡, which they cover entirely. The fibres which terminate at the membrane become longer, as they become more internal. Those which start from the re-entering angle of the thyroid, reascend obliquely to the summit of the arytenoid, then diverge in order to form the sides of the ventricles, and then disappear in the aryteno-epiglottidean folds and even the under surface of the epiglottis. If we cut it away in successive layers, proceeding *from* the outside *to* the in, we reach a thick bundle of fibres, perfectly horizontal, which line the outer aspect of the vocal ligament, and which go from the anterior apophyses of the arytenoid to the re-entering angle of the thyroid*.

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* I find that Dr. Müller hints at the possibility of the voice being thus formed, but only to attack and reject the notion.—*Handbuch der Physiologie des Menschen.*

† Many controversies have arisen respecting the sounds sometimes emitted by animals after the section of the superior and recurrent laryngeal nerves; sounds which have been perhaps occasioned by the struggling of the animal causing a swelling of the neck and a mechanical contact of the vocal ligaments. However, without doubt, after the section of these nerves, voice, as a voluntary act, can no longer take place.

‡ We thus designate that part of the membrane which goes from the bottom of the vocal ligament, to the edge of the cricoid.

This bundle has its posterior half covered by the lateral crico-arytenoid muscle, and its anterior half by the diverging fibres which start from the thyroid. If we cut away the horizontal bundle in successive layers, we see that the fibres are not all of the same length; the most external fibres are the longest, and the succeeding ones get gradually shorter as they become more internal; but they all originate in the anterior cavity of the arytenoid, and the muscle is inserted in the manner above explained throughout the whole length of the vocal ligaments, the thyro-arytenoid portion of it excepted. As the fibres all begin from the arytenoid, and terminate successively at more distant points of the membrane, we see that the muscle is thicker behind than before.

Thus the vocal ligament, and the membrane which depends from it, the sole sources of all vocal sounds, are under the direct action of the fibres which come from the anterior cavity of the arytenoid; the ligament under the action of the horizontal bundle, the membrane under that of the oblique fibres. The long horizontal fibres, extending from one cartilage to the other, are placed at the exterior of the short horizontal fibres, and at the interior of the oblique fibres. The diverging fibres which start from the thyroid, acting only on the superior vocal ligaments and the folds, seem to influence by their contractions only the quality and volume of the voice.

The remarkable arrangement of the fibres which we have just examined, enables us to explain a fundamental fact,—the elevation of the voice. The fibres of the horizontal bundle being placed over each other, in layers, one covering the other, and getting gradually longer and longer, as they become more external, extend their action to the more anterior parts of the edges of the glottis. This progressive action from the back to the front, encroaches gradually on the length of the vibrating portion of the ligament, and likewise increases its tension, and its faculty of accelerating its pulsations.

Another portion of the thyro-arytenoid muscle at the same time stretches and raises the vocal membrane more and more, causing a lesser depth of the ligaments to be in contact, in proportion as the sounds become higher, and thus assists by increasing the mobility of the ligaments.

We shall see in a few moments that the rotatory movement, which the external fibres of the lateral crico-arytenoid muscles give to the arytenoid, by making the vocal membrane deeper, partly counteracts the above effect, and produces the chest-register.

The crico-thyroid muscle, on the contrary, is a powerful auxiliary in the elevation of the voice. This muscle, which at the same time causes the thyroid to come forwards and downwards, gives rise to a mechanical tension, not only in the vocal ligament, but even in the whole vocal membrane. The meeting of the thyroid and cricoid cartilages, which we can feel by the touch, becomes especially marked when the inter-ligamentous glottis alone produces the sounds, which takes place as we have seen at the notes do$_3$♯, ré$_3$, in the chest-register, and an octave above for that of the head; with this difference, however, that for the latter a more vigorous and complete connexion is necessary.

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* Another portion of the thyro-arytenoid muscle.

Let us now see what we may learn from the sensations we feel in the vocal organ. When we produce a chest-note, the least attention enables us to distinguish a "*pinching*" at the posterior part of the glottis, which becomes more vigorous as the notes ascend. This pinching seems to be formed by extension of the depth of the touching surfaces, and may become very painful; whilst the notes of falsetto, when higher than chest ones, give comparatively great relief to this part, and the surfaces in contact seem to have become thinner.

If we combine these sensations with the different remarks which have been furnished to us by the examination of the muscles, we can fix the particular mechanism of each register.

Chest Register.

In fact, when the arytenoid muscles have brought in contact the arytenoid cartilages, and closed the glottis, the voice may take two very different characters; nay, more, it will be produced in pitches widely apart from one another, and will give forth the chest, or falsetto registers, according as the fibres of the thyro-arytenoid attached to the vocal membrane are active or not. By the action of these fibres, as we have seen, this muscle raises the vocal membrane, and makes its apposable part thinner; whereas the lateral crico-arytenoid gives a rotatory movement to the cartilage, which brings the apophyses into deep contact. This deep contact, which continues even after the apophyses no longer partake in the vibrations, gives a deep tension to the membranes, increases the depth of their contact*, and, as a necessary consequence, augments the resistance they present to the air. It is to the extent of this resistance that we attribute the formation of the chest-register, so distinct by its particular amplitude. To it we attribute also the slowness of the beats of the glottis, and the consequent low pitch of the sounds, a pitch which, even in the highest tenor voices, is at least an octave lower than the head notes of ordinary soprani.

Register of Falsetto.

When, on the contrary, the external fibres of the lateral crico-arytenoid muscle remain inactive, we produce the falsetto. The lips of the glottis, stretched by the horizontal bundle of the thyro-arytenoid, come in contact by their edge alone, formed at once by the ligament and the apophyses, and offer little resistance to the air. Hence arises the great loss of this agent, and the general weakness of the sounds produced here.

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* It is then that we feel the pinching of which we have spoken.

But as soon as we reach the sound do$_4$, the beats are produced by the ligaments exclusively, and we have attained the head-register. It is certain, as we may deduce from the movement of the ligaments, that then the vocal membrane is raised by the action of the fibres of the thyro-arytenoid muscle, and its surface is diminished to an edge; but we think that the external fibres of the lateral crico-arytenoid, which would prevent this movement, remain inactive. Then also the very decided tension, which the crico-thyroid muscle effects on the vocal tendons, and which accelerates their movements, takes place.

During the chest-register, therefore, the vocal ligaments are stretched, and are in contact to an extent corresponding with the depth of the anterior apophyses of the arytenoid, whilst in the falsetto the edges alone of the ligaments are stretched and apposed; in both cases the sounds being formed, not by the actual vibrations of either the whole or part of the tendons, but by the successive explosions which they allow.

Pressure of the Air.

Until now, in our remarks on the manner in which the voice is formed, we have only referred to the rigidity of the glottis, a rigidity necessary to accomplish the 1056 vibrations in one second*, which form the do$_4$ of the chest-voice, and to accomplish the double number which produces the octave above in the head-voice. There is, notwithstanding, another indispensable element for the production of vocal sounds, the pressure of the air. Pressure, as is well known, developes an elastic force in this agent, in a degree inverse to the volume which it occupies. It is by means of this power that the intensity of the sounds is obtained. The intensity of the sound can only depend on the quantity of air which goes to each *sharp* explosion. I say *sharp* explosion, as an express condition: the glottis should close itself perfectly after every vibration; for if the air found a constant passage, as in the notes of falsetto, then the greatest movements of the glottis, and the greatest waste of air, would produce precisely the weakest notes. To reject this theory would be to attribute the intensity of the sound to the extent of the vibrations accomplished by the lips of the glottis, and to suppose that these lips, each taken separately, possess the power of producing sounds, suppositions quite contrary to the facts.

The elastic force of the air arises not only from the compression of the lungs, but also from the contractions of the trachea, which adjusts its calibre to the different dimensions of the glottis. It is by means of this force that the air conquers the continually-increasing obstacle presented by the lips of the glottis when they produce sounds more and more intense.

Thus the problem of the elevation of the voice, always complicated with that of its intensity, in order to be complete, ought to show the connexion which exists between the tension of the lips of the glottis, the pressure of the air, and the number and intensity of the explosions obtained. As a consequence, we may state that the greater pressure of air necessary to produce the greater intensity, would at the same time increase the number of pulsations, and so raise the tone; but to prevent this, the glottis must at the same time be lengthened, and *vice versâ*; or, in other words, that the different lengths of the glottis can, under different degrees of pressure, produce the same number of shocks, but at different degrees of intensity.

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* Pouillet, *Physique*, Sixth Edition, vol. ii. page 77.

Of the Qualities of the Voice.

Various simultaneous causes modify the qualities of the voice:—1, according as the glottis partially or entirely closes the passage between the explosions, it produces veiled or brilliant sounds; 2, the tube which surmounts and surrounds it also greatly affects the quality of the voice; by its contractions it gives brilliancy to it and its widening volume; 3, the epiglottis also plays a very important part, for every time that it lowers itself, and nearly closes the orifice of the larynx, the voice gains in brilliancy; and when, on the other hand, it is drawn up, the voice immediately becomes veiled.

ROYAL INSTITUTION OF GREAT BRITAIN.

June 1, 1855.—"On the Currents of the Leyden Battery." By Professor Tyndall, F.R.S.

In our conceptions and reasonings regarding the forces of nature, we perpetually make use of symbols, which, when they possess a high representative value, we dignify with the name of theories. We observe, for example, heat propagating itself through a bar of metal, and help ourselves to a conception of the process by comparing it with water percolating through sand, or travelling by capillary attraction through a lump of sugar. In some such way we arrive at what is called the material theory of heat. The thing seen is thus applied to the interpretation of the thing unseen, and the longing of the human mind to rest upon a satisfactory reason, is in some measure satisfied. So also as regards the subject of the present evening's discourse; we are not content with the mere facts of electricity; we wish to look behind the fact, and, prompted by certain analogies, we ascribe electrical phænomena to the action of a peculiar fluid. Such conceptions have their advantages and their disadvantages: they afford peaceful lodging to the intellect for a time, but they also circumscribe it; and by and by, when the mind has grown too large for its mansion, it often finds a difficulty in breaking down the walls of what has become its prison instead of its home. Thus, at the present day, the man who would cross the bounds which at present limit our knowledge of electricity and magnetism finds it a work of extreme difficulty to regard facts in their simplicity, or to rid them of those hypothetical adornments with which common consent has long invested them.

But though such is the experience of the earnest student of Natural Philosophy at the present—though he may be compelled to refuse his assent to the prevalent theoretic notions, he may nevertheless advantageously make use of the language of these theories in bringing the facts of a science before a public audience; and in speaking of electricity, the speaker availed himself of the convenient hypothesis of two fluids, without at all professing a belief in their existence. A Leyden jar was charged. The interior of the jar might be figured as covered with a layer of positive electricity, and the exterior by a layer of negative electricity; which two electricities, notwithstanding their mutual attraction, were prevented from rushing together by the glass between them. When the exterior and interior coating are united by a conducting body, the fluids move through the conductor and unite; thus producing what is called an electric current. The mysterious agent which we darkly recognise under this symbol is capable of producing wonderful effects; but one of its most miraculous characteristics is its power of arousing a transitory current in a conductor placed near it. The phænomena of voltaic induction are well known; and it is interesting to inquire whether frictional electricity produces analogous phænomena. This question has been examined by Dr. Henry, and still more recently by that able and experienced electrician M. Riess, of Berlin.

A wooden cylinder was taken, round which two copper wires, each 75 feet in length, were wound; both wires being placed upon a surface of gutta-percha, and kept perfectly insulated from each other. The ends of one of these wires were connected with a universal discharger, whose knobs were placed within a quarter of an inch of each other; when the current of a Leyden battery was sent through the other wire, a secondary current was aroused in that connected with the discharger, which announced itself by a brilliant spark across the space separating the two knobs.

The wires here used were covered externally with a sheet of gutta-percha; and lest it should be supposed that a portion of the electricity of the battery had sprung from one wire to the other, two flat discs were taken. Each disc contained 75 feet of copper wire, wound in the form of a flat spiral, the successive convolutions of which were about two lines apart. One disc was placed upon the other one, the wire being so coiled that the convolutions of each disc constituted, so to say, the impress of those of the other, and the coils were separated from each other by a plate of varnished glass. The ends of one spiral were connected with the universal discharger, between whose knobs a thin platinum wire, 10 inches long, was stretched. When the current of the Leyden battery was sent through the other spiral, the secondary current, evoked in the former, passed through the thin wire, and burnt it up with brilliant deflagration. A pair of spirals were next placed 6 inches apart, and a battery was discharged through one of them; the current aroused in the other was sufficient to deflagrate a thin platinum wire 4 inches in length.

We have every reason to suppose that the secondary current thus developed is of the same nature as the primary which produced it; and hence we may infer, that if we conduct the secondary away and carry it through a second spiral, it, in its turn, will act the part of a primary, and evoke a *tertiary* current in a spiral brought near it. This was illustrated by experiment. First, two spirals were placed opposite to each other, through one of which the current of the battery was to be sent; the other was that in which the secondary current was to be aroused. The ends of the latter were connected by wires with a third spiral placed at a distance, so that when the secondary current was excited it passes through the third spiral. Underneath the latter, and separated from it by a sheet of varnished glass, was a fourth spiral, whose two ends were connected with the universal discharger, between the knobs of which a quantity of gun-cotton was placed. When the battery was discharged through the first spiral, a secondary current was aroused in the second spiral, which completed its circuit by passing through the third spiral: here the secondary acted upon the spiral underneath, developed a tertiary current which was sufficiently strong to pass between the knobs, and to ignite the gun-cotton in its passage. It was shown that we might proceed in this way and cause the tertiary to excite a current of the fourth order, the latter a current of the fifth order, and so on; these children, grandchildren, and great grandchildren of the primary being capable of producing all the effects of their wonderful progenitor.

The phænomena of the *extra current*, which exists for an instant contemporaneously with the ordinary current in a common voltaic spiral, were next exhibited; and the question whether a spiral through which a Leyden battery was discharged exhibited any similar phænomena, was submitted to examination. It was proved, that the electric discharge depended upon the *shape* of the circuit through which it passed: when two portions of such a circuit are brought near each other, so that the positive electricity passes in the same direction through both of them, the effect is that the discharge is *weaker* than if sent through a straight wire; if, on the contrary, the current flow through both portions in opposite directions, the discharge is *stronger* than if it had passed through a straight wire. A flat spiral was taken, containing 75 feet of copper wire; one end of the spiral was connected with a knob of the universal discharger, and the other knob was connected with the earth: between the knobs of the discharger about 4 inches of platinum wire were stretched; on connecting the other end of the spiral with the battery, a discharge passed through it of such a strength that it was quite unable to raise the platinum wire to the faintest glow. The same length of copper wire was then bent to and fro in a zigzag manner, so that on every two adjacent legs of the zigzag the current from the battery flowed in opposite directions. When these 75 feet of wire were interposed between the battery and the platinum wire, a discharge precisely equal to that used in the former instance, raised the platinum wire to a high state of incandescence, and indeed could be made to destroy it altogether.

When a primary and a secondary spiral are placed opposite to each other, a peculiar reaction of the secondary upon the primary is observed. If the ends of a secondary (50 feet long) be connected by a thick wire, the effect upon the primary current is the same as when the ends of the secondary remain wholly unconnected. If the ends of the secondary be joined by a long thin platinum wire, the reaction of the secondary is such as to enfeeble the primary.

This enfeeblement increases up to a certain limit as the resistance is increased, from which forwards it diminishes until it becomes insensible. This would appear to prove, that to react upon the primary the secondary requires to be retarded; and that the greater the amount of the retardation, up to a certain limit, the greater is the enfeeblement. But by increasing the resistance we diminish the strength of the secondary, and when a certain limit is attained, this diminution is first compensated for by the influence of retardation, from which point forwards with every increase of the resistance, the enfeeblement of the primary is diminished. A primary current which fuses a certain length of platinum wire where the ends of the secondary are disunited, or where they are united by a thick wire, fails to do so when they are united with a thin wire. But if, instead of a thin wire, a body of much greater resistance, a column of water for example, be introduced, the platinum wire is fused as before.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON TABASHEER. BY M. GUIBOURT.

TABASHEER is a siliceous concretion found in the interior of the stem of the large Indian Bamboo (*Bambusa arundinacea*, Schreb.). The attention of the orientals was first directed to this substance by the writings of the Arabian physicians. The Turks and Arabs only know it under the name of Tabasheer, which is of Persian origin. In India it has other names, such as *Vedroo-paloo* (bamboo-milk), *Vedroo-carpooram* (bamboo-camphor), and *Mangil-upoo* (bamboo-salt). The orientals regard it as one of the most valuable medicines, whilst it is only interesting to us from its being a concretion of silica deposited in a vegetable organism, which has some resemblance to the hydrophane of Haüy.

Towards the end of the last century, Macie stated that tabasheer was nothing but silica. As, however, Vauquelin found it to contain 30 per cent. of potash and lime, Macie's determination was regarded with doubt. Brewster has examined its optical and physical properties.

The author has received a quantity of this substance from Professor Royle, and submitted it again to analysis. The analysis gave—

Silica	96·94
Water	2·93
Potash and lime	0·13
Organic matter	traces

Tabasheer is imperfectly transparent, and sometimes perfectly opake. When immersed in water a quantity of air-bubbles escape from it, and it becomes more transparent; hydrophane behaves in the same manner. When completely saturated with water its spec. grav. is as follows:—

2·158, Macie; 2·169, Cavendish; 2·059 to 2·412, Brewster; and 2·148, Guibourt.

When tabasheer is weighed after soaking in water, it is found to have more than doubled its weight. The statements put forward upon this point are as follows:—

	Weight.	Increased to.	Absorption.
Macie	75·70 grms.	154·20 grms.	79·50 grms. water.
Brewster..	6·65 ..	14·10 ..	7·45 ..
Guibourt..	2·526 ..	5·224 ..	2·695 ..

If we calculate from these numbers the density of the porous tabasheer, by adding the weight of the absorbed water (by which the volume of the pores is determined) to the loss of weight undergone by the body in water (which gives the volume of the solid matter), and then divide the sum by the weight of the dry body, so as to obtain the density of the porous body in the natural state, we get 0·727, 0·6227, 0·6520.

But the tabasheers are heavier than water; they sink in it immediately, and have consequently a greater specific gravity than that thus calculated. The author shows that Brewster observed this contradiction, but without troubling himself about the cause, determined the space occupied by the silica and the pores from this incorrect specific gravity, so that the space occupied by the pores in proportion to that filled by the silica was, according to him, as 2·307 : 1; whilst, from the author's experiments upon the natural density of tabasheer, it appears that this proportion is 0·0689 : 1, which agrees with the behaviour of the body in water.

Tabasheer which has been completely permeated by water and become transparent, becomes perfectly opake in the air. This behaviour, which Brewster believed to occur only with this body, induced him to set up a peculiar theory upon the small refractive power of this substance, which he found to be less than that of any other known body. But without taking into consideration that the number 976·10, which Brewster adopted for the refractive power of tabasheer, is too small, as the author calculates it at 1500·5, we must also take into account the opacity which is peculiar to the molecular state of this substance.

Tabasheer is evidently a mass produced from a jelly by desiccation. To obtain an insight into its production, the author has investigated the inorganic constituents of the bamboo. The small quantity of pith which is seen when a bamboo is split longitudinally, contains iron, potash, lime and silica. The ashes of the wood consist of—

Soluble salts.		Insoluble salts.	
Carbonate of potash..	1·9872	Phosphate of lime..	0·0928
Sulphate of potash ..	0·2905	Phosphate of iron..	0·0130
Phosphate of potash..	0·1593	Silica	0·0408
Chloride of potassium.	0·0766	Loss	0·0371
Silica	0·0204		0·1837
	2·5340		

The author found that the silica occurred in different quantities in different parts of the cane. The pith contained 0·448 per cent. The inner wood held much less, and the greatest proportion occurred in the external wood. Consequently, the author thinks the formation of tabasheer is easily explained in the following manner: at the time when the straw is developed, the outer wood has no longer any necessity for silica, which is carried inwards and deposited in the cavity of the straw.—*Journ. de Pharm.* vol. xxvii. pp. 61 and 252.

NOTE ON THE INFLAMMABILITY OF HYDROGEN.

BY M. BAUDRIMONT.

It is said by Berzelius and others, that hydrogen prepared in the dry way does not possess the property of inflaming by contact with spongy platinum; this phænomenon, first indicated by Faraday, having been attributed by Berzelius to an allotropic condition of the hydrogen, we have thought it worth while to repeat the experiment. For this purpose vapour of water was decomposed by red-hot iron, and the hydrogen thus produced (which is that said to be prepared in the dry way) was directed upon spongy platinum, which, as we had ascertained, possessed the power of igniting the ordinary gas. We then found that it also inflamed the gas obtained from the vapour of water. On collecting some of this same gas in a bottle with a ground glass stopper, and afterwards driving it out by means of a current of water, so as to direct it upon a small mass of spongy platinum, the hydrogen was again ignited. If, therefore, this gas may acquire various allotropic conditions, these cannot be proved by the fact advanced by Faraday, and which we have not been able to realise.—*Comptes Rendus*, August 5, 1855, p. 177.

METEOROLOGICAL OBSERVATIONS FOR JULY 1855.

Chiswick.—July 1. Very fine. 2. Heavy clouds: slight rain. 3, 4. Very fine. 5. Clear: hot and dry. 6. Foggy: very fine. 7, 8. Very fine. 9. Hazy: very fine: thunder, lightning and rain at night. 10. Fine: cloudy: very fine. 11. Constant and very heavy rain from early in the morning till night. 12, 13. Very fine. 14. Cloudy: very fine. 15. Cloudy and fine: rain. 16. Cloudy: heavy rain at night. 17. Fine: cloudy. 18. Very fine. 19. Rain. 20. Very fine. 21. Exceedingly fine. 22. Very fine. 23. Slight fog: sultry: rain. 24. Rain: cloudy. 25. Rain. 26. Densely overcast: constant and very heavy rain. 27. Overcast: heavy clouds and showers. 28. Cloudy: heavy showers. 29. Very fine. 30. Slight haze: very fine: overcast: rain at night. 31. Heavy rain: showery: clear and fine at night.

Mean temperature of the month	62°·99
Mean temperature of July 1854	61 ·59
Mean temperature of July for the last twenty-nine years	63 ·12
Average amount of rain in July	2·428 inches.

Boston.—July 1—4. Fine. 5—7. Cloudy. 8. Fine. 9. Cloudy. 10, 11. Cloudy: rain A.M. 12. Fine. 13. Cloudy. 14. Rain and hail A.M. 15. Fine. 16, 17. Cloudy: rain A.M. and P.M. 18. Cloudy. 19, 20. Cloudy: rain, with thunder A.M. and P.M. 21—23. Cloudy. 24, 25. Rain A.M. and P.M. 26. Fine: rain P.M. 27. Cloudy: rain P.M. 28. Fine: rain P.M. 29, 30. Fine. 31. Rain A.M. and P.M.

Sandwick Manse, Orkney.—July 1. Damp A.M.: showers P.M. 2. Showers A.M.: drops P.M. 3. Rain A.M.: damp P.M. 4. Bright A.M.: cloudy P.M. 5. Cloudy A.M.: fog P.M. 6. Bright, fine A.M.: clear, fine P.M. 7. Bright, fine A.M. and P.M. 8. Bright, fine A.M.: clear, fine P.M. 9. Clear, fine A.M.: vapour, fine P.M. 10. Hazy A.M.: clear, fine P.M. 11. Fine A.M. and P.M. 12. Bright, fine A.M.: fine, fog P.M. 13. Fog A.M.: fine, fog P.M. 14. Fog A.M.: thunder showers, fog P.M. 15. Fog A.M.: showers P.M. 16. Rain A.M.: showers P.M. 17. Damp A.M.: cloudy P.M. 18. Cloudy A.M. and P.M. 19. Bright A.M.: hazy, fine P.M. 20. Bright A.M.: clear, fine P.M. 21. Clear A.M.: drops P.M. 22. Cloudy A.M. and P.M. 23. Cloudy A.M.: showers P.M. 24. Bright A.M.: fine, fog P.M. 25. Cloudy A.M.: fine, fog P.M. 26. Rain A.M.: fog P.M. 27. Rain, fog A.M.: showers, thunder and lightning, clear P.M. 28. Bright A.M.: fine, drops P.M. 29. Rain A.M.: damp P.M. 30. Cloudy A.M.: fine, vapour P.M. 31. Cloudy A.M.: fine P.M.

Mean temperature of July for twenty-eight previous years	55°·08
Mean temperature of this month	59 ·19
Mean temperature of July 1854	55 ·25
Average quantity of rain in July for fifteen previous years...	2·41 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. July.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·163	30·161	29·52	29·78	29·96	76	60	68	59	53	sw.	ssw.	wsw.	....	....	·32
2.	30·193	30·153	29·58	29·97	29·97	78	57	72·5	55	53	sw.	ssw.	w.	·01	....	·17
3.	30·181	30·082	29·63	30·00	30·10	79	56	61	52	52½	n.	nw.	nw.	....	....	·08
4.	30·100	30·091	29·58	30·18	30·18	77	37	64	55½	52	n.	ne.	nne.	....	....	....
5.	30·121	30·038	29·63	30·14	30·09	81	38	57·5	52½	50½	ne.	nnw.	e.	....	....	....
☾ 6.	30·045	30·027	29·62	30·07	30·07	77	44	59·5	56½	54	w.	n.	e.	....	....	....
7.	30·079	30·064	29·64	30·08	30·07	74	50	64	67	59	e.	nne.	se.	....	....	....
8.	30·025	29·867	29·55	30·04	29·97	79	55	65	68½	57½	se.	e.	se.	....	....	....
9.	29·775	29·599	29·43	29·89	29·85	80	59	57	64	53	e.	e.	e.	·14	....	....
10.	29·608	29·585	29·13	29·79	29·77	82	59	73	57	54	sw.	s.	ese.	·48	·05	....
11.	29·541	29·516	29·12	29·77	29·83	71	55	58·5	70½	60	n.	n.	se.	1·07	·40	....
12.	29·840	29·747	29·27	29·83	29·90	81	52	66	74½	60	nw.	n.	nw.	....	....	....
13.	29·959	29·931	29·44	29·92	29·87	79	55	71	58	55	sw.	sw.	ese.	....	....	....
● 14.	29·858	29·822	29·33	29·82	29·70	76	53	61	66	57	sw.	nne.	sse.	·29	·10	....
15.	29·919	29·772	29·34	29·67	29·70	75	55	68	61	56	sw.	ssw.	nw.	·41	·40	·01
16.	29·617	29·464	29·10	29·59	29·58	73	46	68	59½	57	sw.	s.	ne.	·40	....	·12
17.	29·648	29·543	29·00	29·61	29·65	66	51	58	58	56	sw.	w.	n.	....	1·35	·05
18.	29·744	29·724	29·20	29·62	29·59	76	46	65	58	56	sw.	w.	......	·01	·13	....
19.	29·654	29·567	29·12	29·55	29·56	64	46	60	58	55	s.	s.	......	·36	....	....
20.	29·951	29·745	29·22	29·71	29·92	73	41	62·5	60	55	w.	nnw.	......	....	·28	....
21.	30·107	30·049	29·58	29·92	29·91	80	45	59	62	60	w.	nw.	s.	....	·16	....
☽ 22.	30·156	30·114	29·62	29·95	30·04	78	52	72	70	62	w.	w.	se.	....	....	....
23.	30·020	29·765	29·47	29·98	29·80	83	59	75	72	62½	sw.	se.	se.	·68	....	....
24.	29·696	29·606	29·10	29·70	29·71	71	52	67	65	62	sw.	sw.	e.	·14	·10	·15
25.	29·716	29·672	29·13	29·76	29·82	73	49	63·5	65	60	sw.	ene.	e.	·20	·21	....
26.	29·777	29·720	29·28	29·82	29·82	63	50	63	60	60	s.	sw.	e.	1·22	·26	·14
27.	29·842	29·782	29·29	29·62	29·78	75	49	64	60	59	sw.	sw.	ese.	·45	·29	·78
28.	29·876	29·870	29·34	29·83	29·83	72	50	64	65½	58	sw.	ssw.	w.	·06	·19	·03
○ 29.	29·927	29·899	29·34	29·68	29·79	77	53	63	58	55½	w.	w.	se.	....	·03	1·08
30.	29·872	29·828	29·34	29·87	29·83	75	55	64	63	56	sw.	nw.	se.	·24	....	·04
31.	29·750	29·736	29·20	29·76	29·70	71	42	65	61	58	sw.	w.	e.	·14	·15	....

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OCTOBER 1855.

XXIX. *On a new and advantageous Method of preparing Aluminium. By* H. Rose, *Professor of Chemistry in the University of Berlin*[*].

SINCE the discovery of aluminium by Wöhler, Deville has recently taught us the means of procuring this metal in large solid masses, in which condition it exhibits properties with which we were previously unacquainted in its more pulverulent form as procured by Wöhler's method. While, for instance, in the latter state it burns vividly to white earthy alumina on being ignited, the fused globules may be heated to redness without being perceptibly oxidized. These differences may be ascribed to the greater amount of division on the one hand and of density on the other. According to Deville, however, Wöhler's metal contains platina, by which he explains its difficulty of fusion (although it affords white alumina by combustion).

Upon the publication of Deville's researches, I also tried to procure aluminium by the decomposition of the chloride of aluminium and sodium by means of sodium. I did not follow exactly the method of Deville, but placed the salt in alternate layers with sodium and applied heat. I did not, however, obtain satisfactory results.

Moreover, Professor Rammelsberg, who followed exactly the method of Deville, obtained but a very small product, and found it very difficult to prevent the cracking of the glass tube in which the experiment was conducted by the action of the vapour of sodium on the chloride of aluminium. It appeared to me that a great amount of time, trouble and expense, as well as long practice, was necessary to obtain even small quantities of this remarkable metal.

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[*] From Poggendorff's *Annalen* for September, p. 152. Communicated by T. H. Henry, Esq., F.R.S.

The employment of chloride of aluminium and its compounds with the chlorides of alkaline metals is particularly inconvenient, owing to their volatility, to their deliquescence, and to the necessity of preventing all access of air during their treatment with sodium.

It very soon occurred to me that it would be better to use the fluoride of aluminium instead of the chloride, or rather the combinations of the fluoride with the alkaline fluorides, such as we know them through the investigations of Berzelius, who pointed out the strong affinity of fluoride of aluminium for the fluorides of sodium and potassium, and that the mineral occurring in nature under the name of kryolite, was a pure compound of fluoride of aluminium with fluoride of sodium.

This compound is equally fitted for the preparation of aluminium by means of sodium with the chloride of aluminium, or its combination with chloride of sodium. Moreover, as kryolite is not volatile, is readily reduced to the most minute state of division, is free from water and does not attract moisture from the atmosphere, it offers peculiar advantages over the above-named compounds*.

In fact, I succeeded with much less trouble in preparing aluminium by exposing it together with sodium to a strong red heat in an iron crucible, than by using the chloride of aluminium and its compounds. The scarcity of the mineral, however, prevented my pursuing the experiments.

In consequence, however, of receiving very recently from Prof. Krantz of Bonn a considerable quantity of the purest kryolite at a very moderate price (2 dollars the kilogramme), I was enabled to renew the investigation. I was particularly stimulated by finding most unexpectedly that kryolite was to be obtained here in Berlin commercially at an inconceivably low price.

Prof. Krantz had already informed me that he had heard that kryolite occurred in commerce in bulk, but could not learn where. Shortly after, M. Rudel, the manager of the chemical works of H. Kunheim, gave me a sample of a white coarse powder, large quantities of which were brought from Greenland by way of Copenhagen to Stettin under the name of *mineral soda*, and at the price of *three dollars the centner* (about 9*s*. per cwt.). Samples of 40 lbs. had been sent to the soap-boilers, and a soda ley had been extracted from it by means of quicklime, especially adapted to the preparation of many kinds of soap, probably from its containing alumina.

I found this powder to be kryolite, of equal purity to that received from M. Krantz. It dissolved without residue in hydrochloric acid (in platina vessels); the solution evaporated to dryness with sulphuric acid and heated until the excess of acid was dissipated, gave a residue which dissolved completely in water with the aid of a little hydrochloric acid. From the solution ammonia precipitated a considerable quantity of alumina. The solution filtered from the precipitate furnished, on evaporation, a residue of sulphate of soda free from potash. Moreover, the powder gave the well-known reactions of fluorine in a marked degree.

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* Dr. Percy also suggested kryolite as a source of aluminium, and some specimens prepared from this mineral were exhibited by Dr. Faraday at one of the Friday evening meetings of the Royal Institution last season.—T. H. H.

This powder is kryolite of great purity; therefore, the coarse powder I first obtained was not the form in which it is originally introduced. It is now to be obtained in Berlin in great masses; for the preparation of aluminium, however, it must be reduced to a very fine powder.

In my experiments on the preparation of this metal, which were performed in company with M. Weber and with his most zealous assistance, I made use of small iron crucibles of 1¾ inch in height, and 1⅜ inch upper diameter, which I had cast here. In these I placed the finely-powdered kryolite between thin layers of sodium, pressed it down tight, covered it with a good layer of chloride of potassium, and closed the crucible with a well-fitting porcelain cover.

I found chloride of potassium the most advantageous flux to employ; it has the lightest specific gravity of any which could be used, an important point when the slight density of aluminium is taken into consideration; it also increases the fusibility of the fluoride of sodium. I usually employed equal weights of kryolite and chloride of potassium, and for five parts of kryolite I employed two parts of sodium; the most fitting quantity for the crucible was found to be 10 grms. of powdered kryolite. The whole was raised to a strong red heat, by means of a blowpipe supplied with a mixture of atmospheric air and coal-gas, through tubes, on the principle of Daniel's tube for the oxy-hydrogen blowpipe.

It was found most advantageous to maintain the heat for about half an hour and not longer, the crucible being kept closely covered the whole time; the contents are then found to be well fused. When quite cold, the melted mass is removed from the crucible by means of a spatula; this is facilitated by striking the outside with a hammer. The crucible may be employed several times; at last it is broken by the blows from the hammer.

The melted mass is treated with water, when at times only, a very minute evolution of hydrogen gas is observed, which has the same unpleasant odour as the gas evolved during the solution of cast iron in muriatic acid. The carbon contained in this gas is derived from the very slight trace of naphtha adhering to the sodium after drying it. On account of the difficult solubility of the fluoride of sodium, the mass is very slowly acted upon by the water, although the insolubility is somewhat diminished by the presence of the chloride of potassium. After twelve hours the mass has become so far softened that it may be removed from the liquid and broken down in a porcelain mortar. Large globules of aluminium are then discovered, weighing from 0·3 to 0·4 grm. (5 to 6 grains), which may be separated; latterly I have found them of 0·5 grm. (7·7 grs.).

The smaller globules cannot well be separated from the undecomposed kryolite and the alumina always produced by washing, owing to their being specifically lighter than the latter. The whole is treated with diluted nitric acid in the cold; the alumina is not dissolved thereby, but the little globules then first assume their true metallic lustre. They are dried and rubbed on fine silk muslin, the finely-powdered undecomposed kryolite and alumina pass through, while the globules remain on the gauze.

The melted mass should be treated with water in a platinum or silver vessel; a porcelain capsule would be powerfully acted upon by the fluoride of sodium. The solution, after standing till clear, may be evaporated to dryness in a platinum capsule in order to obtain the fluoride of sodium, mixed, however, with much chloride of potassium.

The small globules of aluminium may be united by fusion in a small, well-covered porcelain crucible under a layer of chloride of potassium. They cannot be united without a flux. They cannot be united by mere fusion, like globules of silver for instance, for though they do not appear to oxidize on ignition in the air, they become coated with a scarcely perceptible film of oxide which prevents their running together into a mass. This fusion with chloride of potassium is always attended with a loss of aluminium. A button weighing 3·85 grms. lost, when so treated, 0·05 grm. The chloride of potassium when dissolved in water, left a small quantity of alumina undissolved, but the solution contained none. Another portion of the aluminium had undoubtedly decomposed chloride of potassium, and a portion of chloride of aluminium and chloride of potassium must have been volatilized during the fusion. Other metals, as copper and silver*, behave in a similar manner.

I therefore followed the instructions of Deville, and melted the globules under a stratum of chloride of aluminium and sodium in a covered porcelain crucible. The salt was first melted, and then the globules of metal added to the melted mass. There is no loss, or a very trifling one of a few milligrams of metal, by this process.

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* Poggendorff's *Annalen*, vol. lxviii. p. 287.

When the aluminium is fused under chloride of potassium, its surface is not perfectly smooth, but exhibits minute concavities; with chloride of aluminium and sodium, this is not the case.

The readiest method of preparing the chloride of aluminium and sodium for this purpose, is by placing the mixture of alumina and carbon in a glass tube as wide as possible, and in this a tube of less diameter, open at both ends, and containing chloride of sodium; if the spot where the mixture is placed be very strongly heated, and that where the chloride of sodium is situated more moderately, while a current of chlorine is passed through the tube, the vapour of the chloride of aluminium is so eagerly absorbed by the chloride of sodium, that no chloride of aluminium, or at most a trace, is deposited in any other part of the tube.

If the smaller tube be weighed before the operation, the amount absorbed is readily determined. It is not uniformly combined with the chloride of sodium, for that part which is nearest to the mixture of the charcoal and alumina will be found to have absorbed the most.

I have varied in many ways the process for the preparation of aluminium, but in the end have returned to the one described. I often placed the sodium at the bottom of the crucible, the powdered kryolite above it, and then the chloride of potassium above all. On proceeding in this manner, it was observed that much sodium was volatilized, burning with a strong yellow flame, which never occurred when the sodium was cut into thin slices and placed in alternate layers with the kryolite, in which case the process goes on very quietly. When the crucible begins to get red-hot, the temperature suddenly rises, owing to the commencement of the decomposition of the compound; no lowering of the temperature should be allowed, but the heat should be steadily maintained, not longer, however, than half an hour. By prolonging the process a loss would be sustained, owing to the action of the chloride of potassium on the aluminium. Nor does the size of the globules increase on extending the time even to two hours; this effect can only be produced by obtaining the highest possible temperature. If the process be stopped, however, after five or ten minutes of a very strong heat, the produce is very small, as the metal has not had sufficient time to conglomerate into globules, but is in a pulverulent form and burns to alumina during the cooling of the crucible. No advantage is gained by mixing the kryolite with a portion of the chloride before placing it between the layers of sodium. Neither did I increase the product by using the chloride of sodium and aluminium to cover the mixture instead of the chloride of potassium.

I repeatedly employed chloride of sodium (common salt decrepitated) as a flux in the absence of chloride of potassium without remarking any important difference in the amount of metal produced, although a higher temperature is required in that case.

The operation may also be conducted in unglazed crucibles of difficultly fusible stone-ware of the above dimensions, although they do not resist the action of fluoride of sodium at very high temperatures so well, but fuse in one or more places. The iron crucibles fuse, however, when exposed to a very high temperature in a charcoal fire.

The produce in metal was found to vary much, even when operating exactly in the manner recommended, and with the same quantities of the materials. I never succeeded in reducing the whole amount of metal contained in the kryolite; it contains only 13 per cent. of aluminium; by operating on 10 grms. of the mineral, the quantity I always employed in the small iron crucibles, the most successful result was 0·8 grm. But 0·6 grm., or even 0·4 grm. may be considered favourable. Many times I obtained only 0·3 grm., or even less.

These very different results depend on various causes, more particularly, however, upon the degree of heat obtained. The greater the heat, the greater the amount of larger globules, and the less amount of minutely divided aluminium to oxidise during the cooling of the crucible. I succeeded once or twice in reducing nearly the whole of the metal obtained to one single button, weighing 0·5 grm. at a very high temperature in a stoneware crucible.

I could not always attain the same heat with the blowpipe, as it depended in some degree upon the pressure in the gasometer at the gas-works, which varies at different periods of the day. The following experiment will show how great the loss of aluminium may be owing to the oxidation of the metal during the slow cooling of the crucible and contents.

In a large iron crucible were placed 35 grms. of kryolite in alternate layers with 14 grms. of sodium, and the whole covered with a thick stratum of chloride of potassium. The crucible closed with a porcelain cover, was placed in a larger earthen one also covered, and the whole exposed to a good heat in a draft furnace for one hour, and cooled as slowly as possible. The produce in this case was remarkably small, for 0·135 grm. of aluminium was all that could be obtained in globules.

The difference in the amount reduced depends also in some degree on the more or less successful stratification of the sodium with the powdered kryolite, as much of the latter sometimes escapes decomposition. The greater the amount of sodium employed, the less likely is this to be the case; however, owing to the great difference in the price, I never employed more than 4 grms. of sodium to 10 grms. of kryolite. In order to avoid this loss by oxidation during cooling, I tried another method of preparation.

20 grms. of kryolite were heated intensely in a gun-barrel in a current of hydrogen, and then the vapour of 8 grms. of sodium passed over it. This was effected simply by placing the sodium in a little iron tray in a part of the gun-barrel, without the fire, and pushed forward when the kryolite had attained the maximum temperature. The operation went on very well, the whole being allowed to cool in the current of hydrogen. After the treatment with water, in which the fluoride of sodium dissolved very slowly, I obtained a black powder, consisting for the most part of iron; its solution in hydrochloric acid gave small evidence of alumina.

The small amounts I obtained, however, should not deter others from pursuing these experiments. These are the results of first experiments, on which I have not been able to expend much time. Now that kryolite can be procured at so very moderate a price, and sodium, by the improvement in the preparation of which Deville has performed a most important service, will in future become so much cheaper, it is in the power of every chemist to employ himself in the preparation of aluminium, and I have no doubt in a short time methods will be found affording a much more profitable result.

For the rest, I am of opinion that kryolite is the best adapted of all the compounds of aluminium for the preparation of this metal. It deserves the preference over chloride of aluminium and chloride of aluminium and sodium, and it might still be employed with great advantage even if its price were to rise considerably.

The attempts at preparing aluminium direct from alumina have as yet been unattended with success. Potassium and sodium appear only to reduce metallic oxides when the potash and soda produced are capable of forming a compound with a portion of the oxide remaining as such. Pure potash and soda, with whose properties we are very slightly acquainted, do not appear to be formed in this case. Since, however, alumina combines so readily with the alkalies to form aluminates, one would be inclined to believe that the reduction of alumina by the alkaline metals should succeed.

But even were it possible to obtain the metal direct from alumina, it is very probable that kryolite would long be preferred should it remain at a moderate price; for it is furnished by nature in a rare state of purity, and the aluminium is combined in this substance with sodium and fluorine only, which exercise no prejudicial influence on the preparation of the metal, whereas alumina is rarely found in nature in the pure state, and in a dense and compact condition; and to prepare alumina on a large scale, freeing it from those substances which would act injuriously on the preparation of the metal, would be attended with great difficulties.

The buttons of aluminium which I have prepared are so malleable that they may be beaten and rolled out into the finest foil without cracking on the edges; they have a strong metallic lustre. Some small pieces, not globular, however, which were found at the bottom of the crucible and occasionally adhering to it, cracked on being hammered, and were different in colour and lustre. They were evidently not so pure as the greater number of the globules and contained iron.

On sawing through a larger button of aluminium weighing 3·8 grms., it could readily be observed that the metal for about half a line from the exterior was brittle, while in the interior it was soft and malleable; sometimes the interior of the globules contained concavities.

With Deville I have occasionally obtained the aluminium crystalline. A large button became striated and crystalline on the under side on cooling. Deville believes he has observed regular octahedra, but does not state this positively. According to my brother's examination, these crystalline indications do not belong to the regular form.

As I chanced on one occasion to attempt the fusion of a large button of rather impure aluminium after flattening it out, without a flux, I observed before the heat was sufficient to fuse the mass small globules sweating out of the surface. The impure aluminium being more infusible than the pure metal, the latter expands on fusing, and comes to the surface. A similar phænomenon has been remarked by Schneider with impure bismuth.

I have mentioned above that kryolite is used under the name of mineral soda here in Berlin, for the preparation of soda-ley, which, on account of the alumina it contains, is admirably adapted to the manufacture of soap. In fact, powdered kryolite is completely decomposed when boiled with quicklime and water. The fluoride of calcium formed contains no alumina, which is all dissolved in the caustic soda solution, and this, on its side, is free from fluorine or contains only a very minute trace.

XXX. *On Alloys.* *By* F. CRACE CALVERT, *F.C.S., M.R.A. of Turin, &c.; and* RICHARD JOHNSON, *M.Ph.S. Manchester*[*].

THERE are few subjects in chemistry which have been less examined than alloys, and yet when we reflect on the important services which they render to manufactures and the arts, we must come to the conclusion that all information which may throw light on their composition and properties must enhance their value and applications.

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[*] Communicated by the Authors.

Alloys have been made, up to the present time, according to habit; and the proportions used to compose them have been the results of arbitrary judgements, instead of fixed and determined chemical proportions. It seemed to us probable, that as all the compounds of chemistry result from the combination of their elements in definite proportions, the same must occur with metals operated upon under circumstances which would allow them to combine freely. If, in ordinary circumstances, definite compounds are not formed, it is owing to the presence of an excess of one or more of the metals which enter into the composition of the alloys, and to the real definite compound being solidified in the mass of the metal in excess; just in the same way as when bismuth or sulphur are melted in a crucible and allowed to cool, as we all know, the well-defined crystals of bismuth or sulphur are immersed in the excess of bismuth or sulphur which has not been able to crystallize.

We were therefore inclined to believe, that by producing alloys having a definite composition, we should point the way to much cheaper and better alloys than hitherto; for there can be no doubt that the irregularity of the quality of the alloys of commerce is owing, not only to an excess of one of the metals, but also to a portion of this excess combining with a definite alloy and modifying the composition; thus, if it is a very fusible metal which is in excess, it will remain liquid and combine with the last portion of the real alloys, and form one which will have a different composition from that which was formed at first on the outside of the mass; whilst, on the contrary, if it is a less fusible metal which is used in excess, it will solidify before the predominant alloy of the mass of the casting and prevent its homogeneity. It is to avoid this serious evil that bronze cannons are cooled at the present day shortly after being cast, so as to keep the mass as uniform as possible; and the result has been, that instead of having, as formerly, one-third of the bronze pieces of ordnance defective, now only about one-tenth are so.

Before entering into the details of our researches, it is our duty to state, that during our experiments, Messrs. Levol, Rieffel, and Joule have published papers on some alloys and amalgams having definite proportions.

The alloys of gold and silver, of gold and copper, and of silver and lead, analysed by M. Levol, are very interesting, and many of them, we have no doubt, are definite compounds, as their respective equivalent proportions are low; but as to those analysed by M. Rieffel, we have doubts as to their definite composition, as some of them have the following constitution:—

1 equivalent of copper,
48 equivalents of tin,

or

98 equivalents of copper,
1 equivalent of tin.

Our object was not to examine a few isolated cases, or only the alloys existing in commerce, but to produce a great variety of new alloys, in definite proportions, and to examine their physical and chemical properties, &c.

The first class of alloys which we shall describe is not only quite new but highly interesting, as being the first alloys which have been prepared with iron in definite proportions. It is certainly remarkable, considering the low price of iron, that no efforts have been made to alloy this useful and cheap metal with those of a more expensive nature, such as alloys of iron and tin, or iron and copper, &c., which, if effected, would have given to commerce cheaper alloys than those now in use.

Our primary object in preparing the following alloys of iron, was to render this metal less oxidizable by alloying it with a more electro-positive metal than itself. We were convinced that if we effected this, it would be the most practical method of obtaining the long-looked for object of diminishing the injury which that valuable metal sustains by exposure to the atmosphere; but we were deceived in our anticipations, for the affinity of iron for oxygen is such that it is not diminished, except in one case, when combined with potassium or aluminium.

Alloys of Iron and Potassium.

The first trial we made consisted in heating to a very high temperature some bitartrate of potash and iron, mixed with excess of bitartrate of potash; the result was only a melted mass of carbonate of potash, with a button of cast iron, no doubt owing to the fact that the iron, being in a state of minute division, united first with carbon, and then was unable to unite with potassium.

We then made a mixture of fine iron filings and bitartrate of potash in the following proportions:—

12 equivs. of iron . . .	336 grs.	or 3 equivs.	of iron,
8 equivs. bitartrate of potash,	1504 ...	2 ...	potassium;

with a view to produce an alloy having the composition of the sesquioxide, and submitted the mixture to a high temperature in a crucible, and obtained a large button, which was found on analysis to be composed as follows:—

Iron	74·60
Potassium . . .	25·40
	100·00

which leads to the following formula:—

4 equivalents of iron . . .	112	=	74·17
1 equivalent of potassium . .	39	=	25·83
	151	=	100·00

Therefore, instead of obtaining an alloy having the definite proportions we had employed, we obtained one containing much more iron. This alloy had all the appearance of malleable iron, and could be forged and welded; but what was very extraordinary, and which much surprised us, was its extreme hardness, so much so that at natural temperatures it was hardly dented when hammered with a heavy sledge hammer, and was barely affected by the file; and what was not less interesting, was, that although this alloy contained upwards of 25 per cent. of potassium, or one of the most powerful electro-positive substances, still the iron of the alloy oxidized rapidly in the atmosphere and under water. It is certainly to be much regretted that such is the result; for if, by alloying with a metal more electro-positive than itself, we had succeeded in obtaining an alloy of iron not oxidizable, we should have discovered a most valuable fact for manufactures, and one which has occupied so deeply the thoughts of all those connected with such improvements.

We made another experiment with the same proportions of bitartrate of potash and iron, only we added some charcoal finely pulverized, and obtained the following result:—

Iron	81·42
Potassium . . .	18·58
	100·00

which leads to the formula:—

6 equivalents of iron . . .	168	or	81·16
1 equivalent of potassium . .	39	...	18·84
	207	...	100·00

We are led to think that this alloy contained an excess of iron, for there was on its surface a thin layer of cast iron; and although we filed it off as carefully as possible, still no doubt some of it had introduced itself into the mass. As this alloy presented all the properties of the former, we shall not enter into details respecting it. With the view of obtaining this alloy, if possible, by a cheaper process, we submitted for several hours to an intense heat a mixture of fine iron filings with a quantity of carbonate of potash, containing as much potassium as exists in the bitartrate used in the previous experiments, or—

336 of iron,
552 of pure carbonate of potash,

but no results were obtained.

Alloys of Iron and Aluminium.

We were induced to make a great many experiments to produce these new alloys, in consequence of the remarkable properties which aluminium possesses, as discovered by M. St. Clair Deville; and what increased our desire was, that we hoped they would not only present useful properties, but might be the means of obtaining aluminium cheaper than by the process already discovered.

We shall not describe all the fruitless essays we made, but shall confine our observations only to those which gave us satisfactory results.

The first alloy of aluminium and iron was obtained by heating to a white heat, for two hours, the following mixture:—

8 equivalents of chloride of aluminium	1076
40 equivalents of fine iron filings	1120·0
8 equivalents of lime	224·0

The lime was added to the mixture with the view of removing the chlorine from the chloride of aluminium, so as to liberate the metal aluminium; by forming a fusible chloride of calcium, and subtracting the lime from the above proportions, we ought to have obtained an alloy having the composition of—

1 equivalent of aluminium	14	=	9·09
5 equivalents of iron	140	=	90·91
	154	=	100·00

whilst the alloy which we found at the bottom of the crucible was composed of 100 parts as follows:—

Aluminium	12·00
Iron	88·00
	100·00

which leads to the following formula:—

1 equivalent of aluminium	11·11
4 equivalents of iron	88·89
	100·00

This alloy, as it will be remarked, has the same composition as one of those of potassium and iron, and, like it, it was extremely hard, and rusted when exposed to a damp atmosphere; still it could be forged and welded.

We obtained a similar alloy on adding to the above mixture some very finely-pulverized charcoal, and submitting the whole to a high temperature in a forge furnace for two hours. This alloy gave on analysis the following composition:—

Aluminium . . .	87·91
Iron	12·09
	100·00

But in the mass of chloride of calcium and charcoal remaining in the crucible, there were a great number of globules varying in size from that of a large pea to that of a pin-head. These globules were as white as silver, extremely hard; and what rendered this alloy particularly interesting, was the property which it possessed of not rusting when exposed to a moist atmosphere, or even to hyponitric fumes; its analysis gave these results on 100 parts:—

Aluminium . . .	24·55
Iron	75·45
	100·00

which correspond to:—

2 equivalents of aluminium . .	28	=	25·00
3 equivalents of iron	84	=	75·00
	112	=	100·00

Therefore this alloy has the same composition as alumina, the iron taking the place of the oxygen which exists in the latter.

We treated these globules with weak sulphuric acid, which removed the iron and left the globules of aluminium; these had exactly the same form as before removing the iron, and the aluminium thus obtained had all the properties which have been ascribed to that curious metal.

We have made trials with the following mixture, but although they have yielded results, still they are not sufficiently satisfactory for us to describe in this paper, which is the first of a series we intend publishing on alloys:—

Kaolin or silicate of alumina . .	1750 parts.
Chloride of sodium	1200 ...
Iron	875 ...

This mixture has yielded a metallic mass and globules.

Alloys of Aluminium and Copper.

To obtain these alloys, we had recourse to the same chemical reaction as we employed to prepare those of iron, namely,—

20 equivalents of copper	640
8 equivalents of chloride of aluminium .	1076
10 equivalents of lime	280

We mixed these substances intimately together, and after having submitted them to a high temperature for one hour, we found at the bottom of the crucible a melted mass covered with chloride of copper, and in this mass small globules, which on analysis gave the following results:—

Copper	91·53
Aluminium . . .	8·47
	100·00

which correspond with the formula:—

5 equivalents of copper . .	160	=	91·96
1 equivalent of aluminium .	14	=	8·14
	174	=	100·00

We made another mixture of chloride of aluminium and copper in the proportions above stated, but left out the lime; we also obtained an alloy, which yielded the following results:—

Copper	87·18
Aluminium . . .	12·82
	100·00

which lead to the formula:—

3 equivalents of copper . .	96	=	87·27
1 equivalent of aluminium .	14	=	12·73
	110	=	100·00

Alloys of Iron and Zinc.

We also analysed a deposit formed constantly at the bottom of a metallic bath, composed of melted zinc and tin, and which is used for galvanizing iron. This deposit was found to be composed of—

Iron	6·06
Zinc	93·94
	100·00

which corresponds with the formula:—

1 equivalent of iron . . .	28	=	6·79
12 equivalents of zinc . . .	384	=	93·21
			100·00

This alloy had not the lamellar appearance of zinc, but a crystalline one, and was extremely hard and slightly fusible. We thought, perhaps, that the bath was saturated with iron, which had combined with the zinc and had gradually deposited. We accordingly took some of the melted alloy of zinc and tin in various parts of the bath and examined them, but found only traces of iron. It is certainly remarkable that the iron does not remain diffused in the metallic bath, which is constantly kept in a state of fusion, but combines in definite proportions with zinc, forming a crystalline compound, and that it deposits at a temperature of at least 800° F.

This fact induced us to examine the composition of the metallic bath, which being a very large one, gave us a good opportunity of ascertaining a very interesting problem, viz. whether when we employed definite proportions of zinc and tin to compose it, and melted them together, the mass of the bath would have a uniform composition, or would vary according to the depth. The bath we used was 2½ feet wide, 10 feet long, and 3½ feet deep, and it contained fourteen tons of melted zinc and tin. As this definite compound of iron and zinc existed in large quantities at the bottom of the metallic bath, in order to take our samples we made a wrought iron tube, of two sections, with flanges the entire length, joined together by means of screws, which brought the parts of the tube into close contact. This tube, which had a tap fitted on the upper end, being air-tight when closed, was plunged into the melted metallic bath, and after having reached a given depth, a little alloy was allowed to run in by slightly opening the tap and then closing it. By this means we obtained the following samples; one at the top, one at from 21 to 24 inches deep, and one at the bottom, and we found these to have the following composition:—

	Top.	21 to 24 in.	Bottom.
Zinc . .	81·48	87·72	90·04
Tin . .	13·60	10·03	8·64
Lead . .	4·92	2·25	1·32
	100·00	100·00	100·00

These numbers therefore clearly show that the metallic bath of zinc and tin (for the lead may be considered as an impurity of the zinc) had a different composition in its various parts, and that the chemical affinity was not powerful enough to maintain a uniform composition through the mass. It is also curious to observe that the heaviest metals, viz. tin and lead, are in larger proportions at the top of the bath than at the bottom. We shall not, however, venture to give any explanation of this curious fact until we have further data.

There is a fact which appears to us also worth notice; it is, that if we suppress from the above per-centages the proportion of lead which they contain, and then calculate the zinc and tin into new per-centages, we find that the results obtained perfectly correspond with the quantities required to represent definite compounds, or alloys having an equivalent composition; in fact:—

	Found.	Calculated.	Formula.
Top:—			
Tin . .	14·30	13·89	1Sn.
Zinc . .	85·70	86·11	11Zn.
Middle:—			
Tin . .	10·26	9·98	1Sn.
Zinc . .	89·74	90·02	16Zn.
Bottom:—			
Tin . .	8·76	8·54	1Sn.
Zinc . .	91·24	91·46	19Zn.

Although the quantities found by analysis so nearly correspond with those obtained by calculations, still we hesitate to admit that the metals composing the metallic bath exist in it in a state of definite compounds, the more so as the equivalent of zinc is so high comparatively with that of tin.

Our object being to obtain cheaper alloys than those now in use called brass and bronze, in which copper predominates, we prepared several alloys in definite proportions, in which zinc predominated. To obtain these alloys we melted tin, and added gradually to it the zinc, or the zinc and lead, and then poured this mixture into a melted mass of copper; stirring the mixture well and casting the whole into bars, we succeeded in obtaining the following alloys:—

No. 1.

	Found.	Calculated.
6 equivalents of zinc . .	68·32	68·55
1 equivalent of tin . . .	20·62	20·34
1 equivalent of copper .	11·06	11·11
	100·00	100·00

No. 2.

	Found.	Calculated.
10 equivalents of zinc . .	62·64	62·85
1 equivalent of tin . . .	11·32	11·18
1 equivalent of lead . .	19·94	19·86
1 equivalent of copper .	6·10	6·11
	100·00	100·00

No. 3.

	Found.	Calculated.
20 equivalents of zinc . .	69·56	69·77
2 ... tin . . .	12·58	12·41
1 ... lead . .	11·06	11·04
2 ... copper . .	6·80	6·78
	100·00	100·00

Alloys of Copper.

We also prepared a series of alloys in which copper predominated; the *modus operandi* was the same as that adopted to prepare the above, and we found them to be composed of,—

No. 1.

	Found.	Calculated.
4 equivalents of copper . .	56·25	56·45
3 ... zinc . .	43·75	43·55
	100·00	100·00

No. 2.

18 equivalents of copper .	87·05	86·29
1 ... zinc . .	5·07	4·93
1 ... tin . . .	7·88	8·78
	100·00	100·00

No. 3.

10 equivalents of copper .	77·45	77·77
3 ... zinc . .	14·39	14·23
1 ... tin . . .	8·16	8·00
	100·00	100·00

The first alloy of this series, or one much approaching to it, is already in commerce, and has been analysed by M. Rieffel.

The second alloy has been introduced into commerce of late, and is much valued by locomotive manufacturers for its extreme hardness.

The third alloy is one not yet in commerce, but we believe, from its physical properties, that it will replace the second alloy in many of its principal applications, and if so, it will prove an advantage to trade, as it is a much cheaper alloy.

We were induced to try the action of various acids on the above alloys; for if they were simple mixtures of metals, there was no reason why the metals composing them should not be attacked, though mixed together, as they would if they existed in a free state; whilst, if they were chemically combined, the action of acids would be modified. The results, as far as we have proceeded, perfectly corroborate these views, and we hope that when we have completed this new field of investigation, we shall have practical results to offer.

Action of Hydrochloric Acid, full strength 1·24, *for two hours.*

	Composition of alloys.	Loss per cent.	Action on the metals composing the alloy.
No. 1 :—			
Cu . .	18 eq. 86·29	0·18	Zinc, violent.
Zn . .	1 ... 4·93		Tin, violent.
Sn . .	1 ... 8·78		Copper, slight.
No. 2 :—			
Cu . .	10 eq. 77·77	0·12	
Zn . .	1 ... 14·23		
Sn . .	1 ... 8·00		
No. 3 :—			
Cu . .	4 eq. 56·45	0·2	
Zn . .	3 ... 43·55		

Therefore these alloys are much less attacked than the metals which compose them; and it is certainly interesting to find that the alloy No. 3, which contains nearly 50 per cent. of zinc, is attacked so slightly by the hydrochloric acid. This inertness of an acid, having a most powerful action on one or more of the metals composing an alloy, is most curious; and what increases its interest is, that it appears to be general, as seen in the following tables, which illustrate the action of sulphuric acid :—

	Composition of alloy.	Action of sulphuric acid, spec. grav. 1·50.	Action of sulphuric acid on the metals.
No. 1 :—			
Cu .	18 eq. 86·29	None.	Tin, slight.
Zn .	1 ... 4·93		Copper, slight.
Sn .	1 ... 8·78		Zinc, violent.
No. 2 :—			
Cu .	10 eq. 77·77	None.	
Zn .	1 ... 14·23		
Sn .	1 ... 8·00		
No. 3 :—			
Cu .	4 eq. 56·45	None.	
Zn .	3 ... 43·55		

Action of Nitric Acid on the above Alloys.

	Composition of alloy.	Nitric acid, sp. gr. 1·100. Loss.	Action of nitric acid on the metals.
No. 1 :—			
Cu .	18 eq. 86·29	·02	Copper, most violent.
Zn .	1 ... 4·93		Tin, do.
Sn .	1 ... 8·78		Zinc, do.

	Composition of alloy.	Nitric acid. sp. gr. 1·100. Loss.	Action of nitric acid on the metals.
No. 2:—			
Cu .	10 eq. 77·77	·06	Copper, most violent.
Zn .	1 ... 14·23		Tin, do.
Sn .	1 ... 8·00		Zinc, do.
No. 3:—			
Cu .	4 eq. 56·45	·03	
Zn .	3 ... 43·55		

The results contained in the last table illustrate our views completely, viz. that alloys having a definite composition offer a most extraordinary resistance to the action of acids, as we have alloys the metals of which are most violently attacked by nitric acid, and still we find but a comparatively small amount of alloy dissolved by this powerful acid.

We hope at a future time to publish the result of our labour, which will not only consist in examining the comparative tenacity, hardness, &c. of certain alloys, as compared with the metals composing them, but the influence which, say one, two, or three equivalents of a metal entering into the composition of an alloy, may have on the physical and chemical properties of the said alloy.

XXXI. *On the Application of Photography to Experiments on Diffraction.* By John Bridge, *M.A. University College, London**.

IN addition to the difficulty of giving an explanation at once accurate and popular of the principles of the interference of light, the want of some cheap and convenient means of exhibiting its phænomena has hitherto prevented the subject from being generally known. It is not my concern here to insist that the former difficulty may be removed, but to describe a means by which the phænomena may, at a trifling cost, be illustrated in as great variety as may be desired.

If we look at a line of light through a series of equidistant lines ruled on glass, lateral spectra are produced, whether a telescope or only the naked eye be employed. Here is then an experiment which may be performed in a simple manner; and to make it a popular experiment, it is only necessary to produce these lines cheaply. It occurred to me to do this by taking a collodion picture, as small as desired, of a series of lines ruled on a scale as large as may be necessary to ensure accurate equidistance. When I had succeeded (to a considerable extent) in this, it seemed to me that by bringing the circles, triangles, or other figures used in Sir John Herschel's experiments within the space of the pupil of the eye on a collodion plate, that series of beautiful phænomena might be produced without the employment of a telescope, or at any rate by the employment of a telescope of very low power.

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* Communicated by the Author.

Several circumstances tend to prevent perfect accuracy, and claim attention, either to be obviated as we best can, or to be dismissed as incapable of doing material harm.

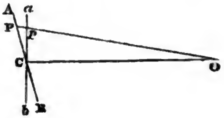
1. Inequality in the surface and in the substance of the glass which is to receive the collodion film.

2. Inequality in the collodion film; in its thickness from pouring off, or wrinkles on its surface left by the evaporation of the æther.

3. The difficulty of placing the glass to receive the image in a plane exactly parallel to that of the object.

With respect to the second difficulty, it is to be considered that the thickness of the collodion film is of an order quite comparable with the length of a wave of light; in fact, I found that the film of an average collodion from a hundred square inches, when dried and lightly pressed, occupied considerably less than $\frac{1}{64}$th of a cubic inch, so that the thickness was certainly not more (and probably very much less) than $\frac{1}{6400}$dth of an inch. The inequality arising from wrinkles is a small fraction of this. The inequality arising from pouring off must also, for the small surface I would use, be a very small fraction of the whole thickness; and such as it is, approximates to that of a very acute-angled prism, whose effect would be quite inappreciable. By cementing a piece of thin glass over the surface of the picture, it is probable that the effect of the wrinkles would be neutralized, at the same time that protection would be afforded to the film.

The third source of error is the most important, but is more in the power of care and delicacy than the others. To find the effect of a small error of this sort in displacing the points of the figure, let *ab* be the correct, and AB the actual position of the surface of the film, C the centre of the picture, O of the lens. Then the point which should be at the distance C*p* from C is at the distance CP.



Now

$$\mathrm{CP}-\mathrm{C}p=\frac{\mathrm{C}p^2}{\mathrm{CO}}\times\angle\mathrm{AC}a.$$

If we are to have, say 500 lines in $\mathrm{C}p=\frac{1}{2}$ of an inch, the error AC*a*, which would remove the 500th line to the place of the 501st, would be $\frac{1}{23}$, or rather more than 2° for a focal length of 10 inches.

The degree of success which I have obtained without the opportunity of satisfying these requirements, is sufficient to assure me that these beautiful experiments may thus be placed within the reach of all, and to justify the expectation that any one who possesses the necessary skill and other advantages may convert this, not only into a popular, but an accurate and scientific experiment.

Besides the lines giving lateral spectra, I have produced regular series of a large number of circles, triangles, &c. within the space of the pupil of the eye, so that the phænomena may be seen in a variety of forms by the unaided eye*. This is, I suppose, scarcely possible by any other means than photography.

XXXII. *Notes on Mineralogy.*—No. III. *On Serpentines and Soapstones. By the* Rev. Samuel Haughton, *Professor of Geology in the University of Dublin*†.

THE following additional analyses of serpentines and soapstones may be considered of interest by some of your readers, particularly those relating to the serpentine porphyry of the Lizard, respecting which there has been some difference of opinion among mineralogists.

The serpentine porphyry of Cornwall consists of greenish crystals imbedded in a reddish paste; the green mineral has been pronounced diallage by Dr. Boase and Sir H. De la Beche, and the reddish paste considered by the same authorities as a species of felspar: both these opinions seem to me to be erroneous, as the porphyritic serpentine of Landewednack and Kynance Cove must be considered as composed altogether of serpentine. I have not found alumina in sensible quantity in the Cornish porphyry, and therefore its presence in the veins of steatite which intersect the serpentine porphyry is highly interesting. During a visit which I paid to this interesting locality in 1854, I ascertained the cause of the presence of alumina in the veins of soapstone. Both at Kynance Cove and Gue Grease, the serpentine porphyry is intersected by dykes of granite, and the celebrated veins of soapstone lie spread out in sheets at the junction of the serpentine and granite; the soapstone must therefore be regarded as the result of the contact of these rocks at an elevated temperature; the serpentine supplying the magnesia; and the felspar of the granite, the alumina, necessary for the formation of the soapstone.

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* But much better with a common telescope.

† Communicated by the Author.

Analyses of Serpentine.

	No. 1.	No. 2.	No. 3.	No. 4.
Silica	38·29	40·12	42·88	41·24
Alumina....................	......	trace		
Protoxide of iron	13·50	3·47	3·80	7·41
Magnesia	34·24	40·04	40 52	36·28
Water......................	12·09	13·36	12·64	14·16
Carbonic acid............	......	2·00	......	
	98·12	98·99	99·84	99·09

No. 1. The red earthy, sometimes semi-crystalline base of the serpentine porphyry of Kynance Cove.

No. 2. Serpentine, carefully picked out from the verd antique of Ballinahinch, co. Galway. It is intimately mixed up with white crystalline marble, and is the result of metamorphic action. The carbonic acid present is due to small particles of limestone which could not be completely separated.

No. 3. Eruptive pale green (passing into gray) serpentine, containing abundance of magnetic iron oxide, from Zermat Thal, Switzerland.

No. 4. Dark green, glossy serpentine from Syria, precise locality unknown.

From the foregoing analyses we readily obtain the following atomic equivalents:—

Number of Atoms.

	Silica.	Protoxides.	Water.	Integers.
No. 1.	0·832	2·087	1·343	10 : 25·08 : 16·14
No. 2.	0·872	2·098	1·484	10 : 24·06 : 17·02
No. 3.	0·932	2·131	1·404	10 : 22·86 : 15·06
No. 4.	0·896	2·020	1·573	10 : 22·54 : 17·65

Notwithstanding the differences apparent in the foregoing results, they all approximate to the formula $5\mathrm{Si\,O^3}$, $12\mathrm{MgO}$, $8\mathrm{HO}$, which gives for rational formula,—

$$5\{2\mathrm{MgO},\ \mathrm{SiO^3} + \mathrm{HO}\} + \{2\mathrm{MgO},\ 3\mathrm{HO}\}.$$

The following Table contains the analyses of two specimens of soapstone; the first taken from the vein at Kynance Cove, the second from the vein at Gue Grease.

Analyses of Soapstones.

	Kynance.	Gue Grease.
Silica	42·47	42·10
Alumina	6·65	7·67
Magnesia	28·83	30·57
Water..................	19·37	18·46
	97·32	98·80

These analyses furnish us with the following :—

Number of Atoms.

	Silica.	Alumina.	Magnesia.	Water.	Integers.
Kynance......	0·923	0·129	1·441	2·152	6·4 : 0·9 : 10 : 14·9
Gue Grease ..	0·915	0·149	1·528	2·051	6· : 0·9 : 10 : 13·4

From this table we deduce the formula $6SiO^3, Al^2O^3, 10MgO, 14HO$, which gives as rational formula probably the following :—

$$5\{2MgO, SiO^3\} + \{Al^2O^3, SiO^3\} + 14HO.$$

XXXIII. *On Pressures of Saturated Vapours.*

By W. J. MACQUORN RANKINE, *C.E., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE perusal, this day, of a recent paper by Sir John Lubbock on Astronomical Refractions, has made me aware of the fact, that in 1840 that author published a general formula for the pressures of saturated vapours of the following form :—

$$\log P = \frac{1}{\beta}\left(\log E + \log\left(1 + \frac{F}{\tau}\right)\right); \quad . \quad . \quad . \quad (1)$$

P being the pressure, τ the absolute temperature, and β, E, F constants.

Sir John Lubbock points out the resemblance between the above formula and that which was published by me in the Edinburgh Philosophical Journal for July 1849, viz.

$$\log P = A - \frac{B}{\tau} - \frac{C}{\tau^2} - \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

That resemblance I freely admit; inasmuch as the development of the formula (1) produces a series in terms of the powers of $\frac{1}{\tau}$.

The *differences* between the two formulæ appear to me to be the following.

The formula (1) is deduced from certain reasonings as to relations between the pressure of vapour and its total heat of evaporation (which reasonings it would be premature for me to discuss after so brief an examination of them as I have yet been able to make), and it contains essentially but three independent coefficients; so that all the coefficients of its development must be functions of the first three.

The formula (2) was rather *suggested by*, than deduced from, certain hypothetical views as to the condition of a vapour at and near the surface of its liquid; with respect to which views I may refer to the Transactions of the Royal Society of Edinburgh, vol. xx., and the Philosophical Magazine for December 1851 and December 1854. In this formula the number of independent coefficients to be determined empirically is *indefinite*, although three coefficients have been found to give a sufficiently close agreement with such experiments as have yet been made. On this point see § 10 of a paper in the Philosophical Magazine for December 1854. The computation of the coefficients is a very easy process.

I may observe, that, from the following principle in the theory of thermo-dynamics (Phil. Trans. 1854, part 1),—

Latent heat of evaporation of unity of weight of a fluid

$$=\tau\,.\,\frac{d\mathrm{P}}{d\tau}\,(\mathrm{V}-v)$$

> (expressed in units of work at the rate of 772 foot-pounds per Fahrenheit-unit of heat), where v and V are the volumes of unity of weight of the fluid in the liquid and vaporous states respectively,—

it is easily deduced that *if the vapour be a perfect gas,*

$$\log_e \mathrm{P}=a-\frac{\tau_0}{\mathrm{P}_0\mathrm{V}_0}\left\{(k-\mathrm{K})\log_e\tau+\frac{c}{\tau}\right\}, \quad . \quad . \quad (3)$$

in which τ_0, P_0, V_0 refer to a fixed standard absolute temperature, K is the dynamical specific heat of the vapour under constant pressure (or the mechanical work required to heat unity of weight of it one degree under constant pressure by friction), k that of the liquid, and a and c are constants to be found empirically. But as few saturated vapours are even approximately in the perfectly gaseous state, the formula (3) refers to an ideal case, and I therefore refrain from enlarging upon it.

The ratio $\frac{K\tau_0}{P_0V_0}$ corresponds to $\frac{\gamma}{\gamma-1}$ in Poisson's notation.

I have the honour to be, Gentlemen,
Your most obedient Servant,
W. J. MACQUORN RANKINE.

Glasgow, September 21, 1855.
59 St. Vincent Street.

XXXIV. *On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; to which is prefixed, an Account of some Experiments on Molecular Influences.* By JOHN TYNDALL, *Ph.D., F.R.S. &c.*

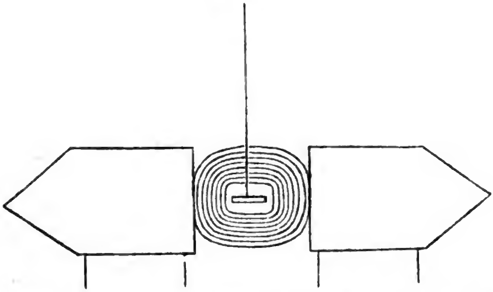
[Concluded from p. 179.]

3. *Separate and joint action of a Magnet and a Voltaic Current on Paramagnetic and Diamagnetic Bodies.*

IN operating upon bars of bismuth with the magnet, or the current, or both combined, it was soon found that the gravest mistakes might be committed if the question of structure was not attended to; that it is not more indefinite to speak of the volume of a gas without giving its temperature, than to speak of the deportment of bismuth without stating the relation of the form of the mass to the planes of crystallization. Cut in one direction, a bar of bismuth will set its length parallel to an electric current passing near it; cut in another direction, it will set its length perpendicular to the same current. It was necessary to study the deportment of both of these bars separately.

A helix was formed of covered copper wire one-twentieth of an inch thick: the space within the helix was rectangular, and was 1 inch long, 0·7 inch high, and 1 inch wide: the external diameter of the helix was 3 inches. Within the rectangular space

Fig. 6.



the body to be examined was suspended by a fibre which descended through a slit in the helix. The latter was placed between the two flat poles of an electro-magnet, and could thus be caused to act upon the bar within it, either alone or in combination with the magnet. The disposition will be at once understood from fig. 6, which gives a front view of the arrangement.

Action of Magnet alone: Division of bars into Normal and Abnormal.—A bar of soft iron suspended in the magnetic field will set its longest dimension from pole to pole: this is the normal deportment of paramagnetic bodies. A bar of bismuth, whose planes of principal cleavage are throughout parallel to its length, suspended in the magnetic field with the said planes vertical, will set its longest dimension at right angles to the line joining the poles: this is the normal deportment of diamagnetic bodies. We will therefore, for the sake of distinction, call the former a *normal paramagnetic bar,* and the latter a *normal diamagnetic bar.*

A bar of compressed carbonate of iron dust, whose shortest dimension coincides with the line of pressure, will, when suspended in the magnetic field with the said line horizontal, set its length equatorial. A bar of compressed bismuth dust, similarly suspended, or a bar of bismuth whose principal planes of crystallization are transverse to its length, will set its length axial in the magnetic field. We will call the former of these an *abnormal paramagnetic bar*, and the latter an *abnormal diamagnetic bar.*

Action of Current alone on normal and abnormal bars.—A normal paramagnetic bar was suspended in the helix above described; when a current was sent through the latter, the bar set its longest horizontal dimension parallel to the axis of the helix, and consequently perpendicular to the coils.

An *abnormal paramagnetic bar* was suspended in the same manner; when a current was sent through the helix, the bar set its longest dimension perpendicular to the axis of the helix, and consequently parallel to the coils.

A *normal diamagnetic bar* was delicately suspended in the same helix; on the passage of the current it acted precisely as the abnormal magnetic bar; setting its longest dimension perpendicular to the axis of the helix and parallel to the coils. When a fine fibre and sufficient power are made use of, this deportment is obtained without difficulty.

An *abnormal diamagnetic bar* was suspended as above; on the passage of the current it acted precisely as the normal magnetic bar: it set its length parallel to the axis of the helix and perpendicular to the coils. Here also, by fine manipulation, the result is obtained with ease and certainty.

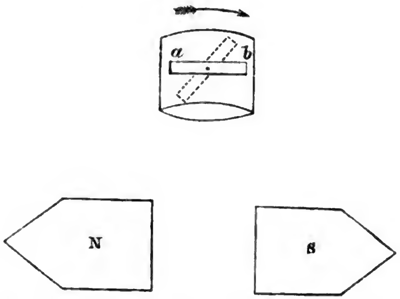
Action of Magnet and Current combined.—In examining this subject, eight experiments were made with each particular bar; it will be remembered that fig. 6 gives a general view of the arrangement.

1. Four experiments were made in which the *magnet* was excited first, and after the suspended bar had taken up its position of equilibrium, the deflection produced by the passage of a current through the surrounding helix was observed.

2. Four experiments were made in which the *helix* was excited first, and when the bar within it had taken up its position of equilibrium, the magnetism was developed and the consequent deflection observed.

Normal Paramagnetic Bar.—In experimenting with the soft iron it was necessary to place it at some distance from the magnet, otherwise the attraction of the entire mass by one or the other pole would completely mask the action sought. Fig. 7 represents the disposition of things in these experiments: N and S indicate the north and south poles of the magnet; *ab* is the bar of iron; the helix within which the bar was suspended is shown in outline around it; the arrow shows the direction of the current in the *upper half* of the helix; its direction in the under portion would, of course, be the reverse.

Fig. 7.

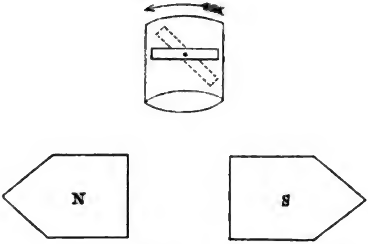


On exciting the magnet, the bar of soft iron set itself parallel to the line joining the poles, as shown by the unbroken line in fig. 7.

When the direction of the current in the helix was that indicated by the arrow, the bar was deflected towards the position dotted in the figure.

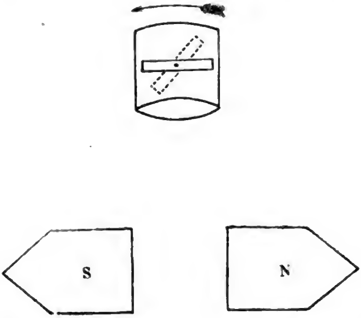
Interrupting the current in the helix, and permitting the magnet to remain excited, the bar returned to its former position: the current was now sent through the helix in the direction of the arrow, fig. 8; the consequent deflection was towards the dotted position.

Fig. 8.



Both the current which excited the magnet and that which passed through the helix were now interrupted, and the polarity of the magnet was reversed. On sending a current through the helix in the direction of the arrow, the deflection of the bar was from the position of the defined line to that of the dotted one, fig. 9.

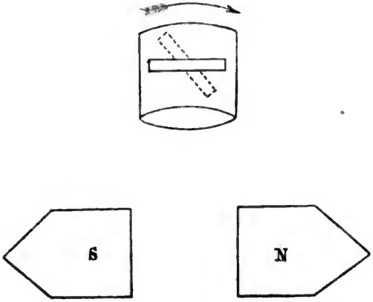
Fig. 9.



Interrupting the current through the helix, and permitting the bar to come to rest under the influence of the magnet alone, a current was sent through the helix in a direction opposed to its former one: the deflection produced was that shown in fig. 10.

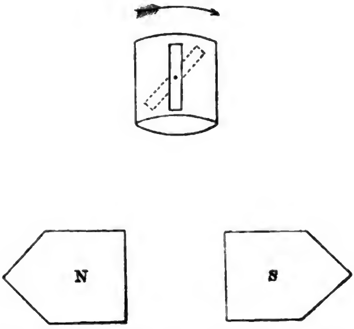
The position of equilibrium finally assumed by the bar depends, of course, upon the ratio of the forces acting upon it: in these experiments, the bar, in its final position, enclosed an angle of about 50 degrees with the axial line.

Fig. 10.



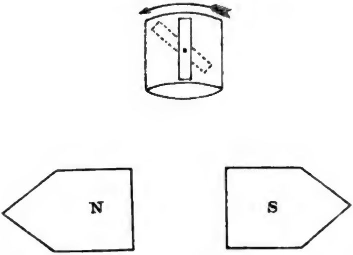
A series of experiments was next made, in which the bar was first acted on by the current passing through the helix, the magnet being brought to bear upon it afterwards. On the passage of the current through the helix in the direction shown in fig. 11, the bar set its length parallel to the axis of the latter. On exciting the magnet so that its polarity was that indicated by the letters N and S in the figure, the deflection was towards the dotted position.

Fig. 11.



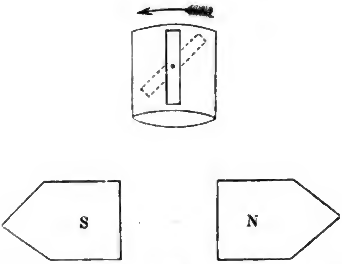
Interrupting the current through both magnet and helix, and reversing the current through the latter, the bar came to rest, as before, parallel to the axis: on exciting the magnet, as in the last case, the deflection was that shown in fig. 12.

Fig. 12.



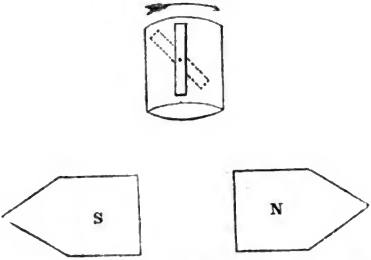
Preserving the same current in the helix, and reversing the polarity of the magnet, the deflection was that shown in fig. 13.

Fig. 13.



Preserving the magnet poles as in the last experiment, and reversing the current in the helix, the deflection was that shown in fig. 14.

Fig. 14.



In these cases, the bar, in its final position of equilibrium, enclosed an angle of about 40 degrees with the axial line.

Normal Diamagnetic Bar.—The above experiments exhibit to us the deportment of the normal paramagnetic body under a great variety of conditions, and our next step is to compare with it the deportment of the normal diamagnetic body under the same circumstances.

For the sake of increasing the force, the helix was removed from its lateral position and placed between the two poles, as in fig. 6, p. 257. The normal diamagnetic bar was suspended within the helix and submitted to the self-same mode of examination as that applied in the case of the paramagnetic body.

The polarity first excited was that shown in fig. 9, Plate III., and the position of rest, when the magnet alone acted, was at right angles to the line joining the poles; on sending a current through the helix in the direction of the arrow, the deflection was towards the dotted line.

Preserving the magnetic polarity as in the last experiment, the direction of the current through the helix was reversed, and the deflection was that shown in fig. 10.

Reversing the polarity of the magnet, and sending the current through the helix in the direction of the last experiment, the deflection was that shown in fig. 11.

Preserving the last magnetic poles, and sending the current through the helix in the opposite direction, the deflection was that shown in fig. 12.

In the following four experiments the helix was excited first.

Operated upon by the helix alone, the suspended bar set its length parallel to the convolutions, and perpendicular to the axis of the coil: the direction of the current was first that shown in fig. 13: when the magnet was excited, the bar was deflected towards the dotted position.

Interrupting both currents, and reversing the current in the helix; when the magnet was excited, as in the last experiment, the deflection was that shown in fig. 14.

Preserving the helix current as in the last experiment; when the polarity of the magnet was reversed, the deflection was that shown in fig. 15.

Interrupting both, and reversing the current in the helix; when the magnet was excited as in the last experiment, the deflection was that shown in fig. 16.

In a paper on the Polarity of Bismuth* published in the Philosophical Magazine, ser. 4. vol. ii., and in Poggendorff's *Annalen*, vol. lxxxvii., an experiment is recorded in which the deportment exhibited by fig. 11 of the present series was obtained. In a recent memoir on the same subject, M. v. Feilitsch* states that he has sought this result in vain. Sometimes he observed the deflection at the moment of closing the circuit, but conceived that it must be ascribed to the action of induced currents; for immediately afterwards a deflection in the opposite direction was observed, which deflection proved to be the permanent one.

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* From the notices of this paper which have appeared in the continental journals, I am obliged to infer that it is in some respects obscurely written. The conclusion I intended to express is that bismuth possesses a polarity opposed to that of iron.—J. T.

I have repeated the experiment here referred to with all possible care; and the result is that described in the remarks which refer to fig. 11. This result agrees in all respects with that described in my former paper. To enable myself, however, to appeal to quantitative measurement, a small graduated circle was constructed and placed underneath the bar of bismuth suspended within the helix. The effect, as will be seen, is not one regarding which a mistake could be made on account of its minuteness: operating delicately, and choosing a suitable relation between the strength of the magnet and that of the spiral†, on sending a current through the latter as in fig. 11, the bar was deflected so forcibly that the limit of its first impulsion reached 120° on the graduated circle underneath. The permanent deflection of the bar amounted to 60° in the same direction, and hence the deportment could in no wise be ascribed to the action of induced currents, which vanish immediately. Before sending the current through the helix, the bar was acted on by the magnet alone, and pointed to zero.

Though it was not likely that the shape of the poles could have any influence here, I repeated the experiment, using the hemispherical ends of two soft iron cores as poles: the result was the same.

A pair of poles with the right- and left-hand edges rounded off, showed the same deportment.

A pair of poles presenting chisel edges to the helix showed the same deportment.

Various other poles were made use of, some of which appeared to correspond exactly with those figured by M. v. Feilitsch; but no deviation from the described deportment was observed. To test the polarity of the magnet, a magnetic needle was always at hand: once or twice the polarity of the needle became reversed, which, had it not been noticed in time, would have introduced confusion into the experiments. Here is a source of error against which, however, M. v. Feilitsch has probably guarded himself. Some irregularity of crystalline structure may, perhaps, have influenced the result. With "chemically pure zinc" M. v. Feilitsch obtained the same deflection that I obtained with bismuth: now chemically pure zinc is *diamagnetic*[*], and hence its deportment is corroborative of that which I have observed. M. v. Feilitsch, however, appears to regard the zinc used by him as magnetic; but if this be the case, it cannot have been chemically pure. It is necessary to remark that I have called the north pole of the electro-magnet that which attracts the south, or unmarked end, of a magnetic needle; and I believe this is the custom throughout Germany.

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* Poggendorff's *Annalen*, vol. xcii. p. 395.

† In most of these experiments the spiral was excited by ten cells, the magnet by two.

Abnormal Paramagnetic Bar.—This bar consisted of compressed carbonate of iron dust, and was suspended within the helix with the line of compression, which was its shortest dimension, horizontal. As in the cases already described, it was first acted upon by the magnet alone; having attained its position of equilibrium, a current was sent through the helix, and the subsequent deflection was observed.

The magnet being excited as in fig. 17, Plate III., the bar set its length equatorial; on sending a current through the helix in the direction of the arrow, the bar was deflected to the dotted position.

Reversing the current in the helix, but permitting the magnet to remain as before, the deflection was that shown in fig. 18.

Interrupting all, and reversing the polarity of the magnet; on sending the current through as in the last case, the deflection was that shown in fig. 19.

Reversing the current, but preserving the last condition of the magnet, the deflection was that shown in fig. 20.

In the subsequent four experiments the helix was excited first. Whatever might be the direction of the current through the helix, the bar always set its length perpendicular to the axis of the latter, and parallel to the coils.

When the direction of the helix current, and the polarity of the magnet, were those shown in fig. 21, the deflection was to the dotted position.

Interrupting all, and reversing the current in the helix; on exciting the magnet the deflection was that shown in fig. 22.

Changing the polarity of the magnet, and preserving the helix current in its former direction, the deflection was that shown in fig. 23.

Interrupting all, and reversing the current through the helix; when the magnetism was developed the deflection was that shown in fig. 24.

Abnormal Diamagnetic Bar.—This bar consisted of a prism of bismuth whose principal planes of crystallization were perpendicular to its length: the mode of experiment was the same as that applied in the other cases.

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[*] Phil. Mag. vol. xxviii. p. 456.

Acted upon by the magnet alone, the bar set its length from pole to pole: the magnetic excitation being that denoted by fig. 29, a current was sent through the helix in the direction of the arrow; the bar was deflected to the dotted position.

Reversing the current through the helix, the deflection was that shown in fig. 30.

Interrupting both currents and reversing the magnetic poles; on sending a current through the helix as in the last experiment, the deflection was that shown in fig. 31.

Reversing the current through the helix, the deflection was that shown in fig. 32.

In the subsequent four experiments the helix was excited first.

Sending a current through the helix in the direction denoted by the arrow, the bar set its length at right angles to the convolutions, and parallel to the axis of the helix; when the magnetism was excited as in fig. 25, the deflection was to the dotted position.

When the current was sent through the helix in an opposite direction, the deflection was that shown in fig. 26.

Interrupting both currents, and reversing the poles of the magnet; on sending a current through the helix as in the last experiment, the deflection was that shown in fig. 27.

Reversing the current in the helix, the deflection was that shown in fig. 28.

In all these cases the position of equilibrium due to the first force was attained before the second force was permitted to act.

It will be observed, on comparing the deportment of the normal paramagnetic bar with that of the normal diamagnetic one, that the position of equilibrium taken up by the latter, when operated on by the helix alone, is the same as that taken up by the former when acted on by the magnet alone: in both cases the position is from pole to pole of the magnet. A similar remark applies to the abnormal para- and diamagnetic bars. It will render the distinction between the deportment of both classes of bodies more evident, if the position of the two bars, before the application of the second force, be one and the same. When both the bars, acted on by one of the forces, are axial, or both equatorial, the contrast or coincidence, as the case may be, of the deflections from this common position by the second force will be more strikingly evident.

To effect the comparison in the manner here indicated, the figures have been collected together and arranged upon Plate III. The first column represents the deportment of the normal paramagnetic bar under all the conditions described; the second column, that of the normal diamagnetic bar; the third shows the deportment of the abnormal paramagnetic bar, and the fourth that of the abnormal diamagnetic bar.

A comparison of the first two columns shows us that the deportment of the normal magnetic bar is perfectly antithetical to that of the normal diamagnetic one. When, on the application of the second force, an end of the former is deflected to the right, the same end of the latter is deflected to the left. When the position of equilibrium of the magnetic bar, under the joint action of the two forces, is from N.E. to S.W., then the position of equilibrium for the diamagnetic bar is invariably from N.W. to S.E. There is no exception to this antithesis, and I have been thus careful to vary the conditions of experiment in all possible ways, on account of the divergent results obtained by other inquirers. In his recent memoirs upon this subject, M. v. Feilitsch states that he has found the deflection of diamagnetic bodies, under the circumstances here described, to be precisely the same as that of paramagnetic bodies: this result is of course opposed to mine; but when it is remembered that the learned German worked confessedly with the "roughest apparatus," and possessed no means of eliminating the effects of structure, there seems little difficulty in referring the discrepancy between us to its proper cause.

The same perfect antithesis will be observed in the case of the abnormal bars, on a comparison of the third and fourth columns. In all cases then, whether we apply the magnet singly, or the current singly, or the magnet and current combined, the deportment of the normal diamagnetic bar is opposed to that of the normal paramagnetic one, and the deportment of the abnormal paramagnetic bar is opposed to that of the abnormal diamagnetic one. But if we compare the normal paramagnetic with the abnormal diamagnetic bar, we see that the deportment of the one is identical with that of the other*. The same identity of action is observed when the normal diamagnetic bar is compared with the abnormal paramagnetic one. The necessity of taking molecular structure into account in experiments of this nature could not, I think, be more strikingly exhibited.

For each of the bars, under the operation of the two forces, there is an oblique position of equilibrium: on the application of the second force, the bar swings like a pendulum beyond this position, oscillates round it, and finally comes to rest there. Hence, if before the application of the second force the bar occupy the axial position, the deflection, when the second force is applied, appears to be from the axis to the equator; but if it first occupy the equatorial position, the deflection appears to be from the equator to the axis.

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* Identical to the eye, but not to the mind. The notion appears to be entertained by some, that, by changing molecular structure, I had actually converted paramagnetic substances into diamagnetic ones, and *vice versâ*. No such change, however, can cause *the mass* of a diamagnetic body suspended by its centre of gravity to be *attracted*, or the mass of a paramagnetic body to be *repelled*. But by a change of molecular structure, one of the forces may be so caused to apply itself that it shall present to the eye all the *directive* phænomena exhibited by the other.—J. T., May 5, 1855.

We have already shown that the repulsion of diamagnetic bodies is to be referred to a state of excitement induced by the magnet which acts upon them: it has been long known that the attraction of paramagnetic bodies is due to the same cause. The experiments just described exhibit to us bars of both classes of bodies moving in the magnetic field: such motions occur in virtue of the induced state of the body, and the relation of that state to the forces which act upon the mass. We have seen that in all cases the antithesis between both classes of bodies is maintained. Whatever, therefore, the state of the paramagnetic bar under magnetic excitement may be, a precisely antithetical state would produce all the phænomena of the diamagnetic bar. If the bar of iron be polar, a reverse polarity on the part of bismuth would produce the effects observed. From this point of view all the movements of diamagnetic bodies become perfectly intelligible, and the experiments to be recorded in the next chapter are not calculated to diminish the probability of the conclusion that diamagnetic bodies possess a polarity opposed to that of magnetic ones.

The phænomena to which we have thus far referred consist in the rotations of elongated bars about their axes of suspension. The same antithesis, however, presents itself when we compare the *motion of translation* of a paramagnetic body, within the coil, with that of a diamagnetic one. A paramagnetic sphere was attached to the end of a horizontal beam and introduced into the coil: the magnet being excited, the sphere could be made to traverse the space within the coil in various directions, by properly varying the current through the coil. A diamagnetic sphere was submitted to the same examination, and it was found that the motions of both spheres, when operated on by the same forces, were always in opposite directions.

V. FURTHER COMPARISON OF PARAMAGNETIC AND DIAMAGNETIC PHÆNOMENA:—DIAMAGNETIC POLARITY.

When an iron bar is placed within a helix, it is well known that on sending a current through the latter the bar is converted into a magnet, one end of the bar thus excited being attracted, and the other end repelled by the same magnetic pole. In this *twoness* of action consists what is called the *polarity* of the bar: we will now consider whether a bar of bismuth exhibits similar effects.

Fig. 39 Plate IV. represents the disposition of the apparatus used in the examination of this question. AB is a helix of covered copper wire one-fifteenth of an inch in thickness: the length of the helix is 5 inches, external diameter 5 inches, and internal diameter 1·5 inch. Within this helix a bar of bismuth 6½ inches long and 0·4 of an inch thick was suspended. The suspension was effected by means of a light beam, from two points of which, sufficiently distant from each other, depended two silver wires each ending in a loop: into these loops, *ll'*, the bar of bismuth was introduced, and the whole was suspended by a number of fibres of unspun silk from a suitable point of support. Fig. 39 *a* is a side view of the arrangement used for the suspension of the bar. Before introducing the latter within the helix, it was first suspended in a receiver, which protected it from air currents, and in which it remained until the torsion of the fibre had exhausted itself: the bar was then removed, and the beam, without permitting the fibre to twist again, was placed over the helix so as to receive the bar introduced through the latter. From the ends of this helix two wires passed to a current reverser R, from which they proceeded further to the poles of a voltaic battery. CD and EF are two electro-magnetic spirals, each 12 inches long, 5½ inches external and 2 inches internal diameter. The wire composing them is one-tenth of an inch thick, and so coiled that the current could be sent through four wires simultaneously. Within these spirals were introduced two cores of soft iron 2 inches thick and 14 inches long: the ends of the cores appear at P and P'. The spirals were so connected together that the same current excited both, thus developing the same magnetic strength in the poles P P'. From the ends of the spirals proceeded wires to the current reverser R', and thence to a second battery of considerably less power than the former. By means of the reverser R' the polarity of the cores could be changed; P' could be converted from a south pole to a north pole, at the same time that P was converted from a north pole to a south pole. Lastly, by a change of the connexions between the two spirals, the cores could be so excited as to make the poles of the same quality, both north or both south.

The diameter of the cylindrical space, within which the bismuth bar was suspended, was such as to permit of a free play of the ends of the bar through the space of an inch and a half. Having seen that the bar swung without impediment, and that its axis coincided as nearly as possible with the axis of the helix, a current from the battery was sent through the latter. The magnetism of the cores P and P' was then excited, and the action upon the bismuth bar observed. M. v. Feilitsch has attempted a similar experiment to that here described, but without success: when, however, sufficient power is combined with sufficient delicacy, the success is complete, and the most perfect mastery is obtained over the motions of the bar.

The helix above described is the one which I have found most convenient for the experiments; various other helices, however, were tried with a result equally certain, if less energetic. The one first made use of was 4 inches long, 3 inches exterior diameter, and three-quarters of an inch interior diameter, with wire one-fifteenth of an inch in thickness, the bar being suspended by a fibre which passed through a slit in the helix: sending through this helix a current from a battery of 10 cells, and exciting the cores by a current from 1 cell, the phænomena of repulsion and *attraction* were exhibited with all desirable precision.

I shall now proceed to describe the results obtained by operating in the manner described. The bismuth bar being suitably suspended, a current was sent through the helix, so that the direction of the current *in the upper half* was that indicated by the arrow in fig. 40, Pl. IV. On exciting the magnet, so that the pole N was a north pole and the pole S a south pole, the ends of the bar of bismuth were *repelled*. The final position of the bar was against the side of the helix most remote from the magnets: it is shown by dots in the figure.

By means of the reverser R the current was now sent through the helix in the direction shown in fig. 41: the bar promptly left its position, crossed the space in which it could freely move, and came to rest as near the magnets as the side of the helix would permit it. *It was manifestly attracted by the magnets.*

Permitting the current in the helix to flow in the last direction, the polarity of the cores of soft iron was reversed: we had then the state of things sketched in fig. 42; the bismuth bar instantly loosed from the position it formerly occupied, receded from the magnet, and took up finally the position marked by the dots.

After this new position had been attained, the current through the helix was reversed: the bar promptly sailed across the field towards the magnets, and finally came to rest in the dotted position, fig. 43. In all these cases, when the bar was freely moving in any direction, under the operation of the forces acting upon it, the reversion either of the current in the helix or of the polarity of the cores arrested the motion; approach was converted into recession, and recession into approach.

The ends of the helix in these experiments were not far from the ends of the soft iron cores; and it might therefore be supposed that the action was due to some modification of the cores by the helix, or of the helix by the cores. It is manifest that the magnets can have no *permanent* effect upon the helix; the current through the latter, measured by a tangent galvanometer, is just as strong when the cores are excited as when they are unexcited. The helix may certainly have an effect upon the cores, and this effect is either to enfeeble the magnetism of the cores or to strengthen it; but if the former, and the bar were the simple bismuth which it is when no current operates on it, the action, though weakened, *would still be repulsive*; and if the latter, the increase would simply augment the repulsion. The fact, however, of the ends of the bar being *attracted*, proves that the bar has been thrown into a peculiar condition by the current circulating in the surrounding coil. Changing the direction of the current in the coil, we find that the self-same magnetic forces which were formerly attractive are now repulsive; to produce this effect the condition of the bar must have changed with the change of the current; or, in other words, the bar is capable of accepting *two different states* of excitement, which depend upon the direction of the current.

In order, however, to reduce as far as possible the action of the helix upon the cores, I repeated the experiments with the small helix referred to in fig. 6, page 27. It will be remembered that this helix is but an inch in length, and that the bismuth bar is 6½ inches long. I removed the magnets further apart, so that the centres of the cores were half an inch beyond the ends of the bismuth bar, while the helix encircled only an inch of its central portion: in this position, when the helix was excited, there was no appreciable magnetism excited by it in the dormant cores; at least, if such were excited, it was unable to attract the smallest soft iron nail. Here then we had cores and helix sensibly independent of each other, but the phænomena appeared as before. The bar could be held by the cores against the side of the helix, with its ends only a quarter of an inch distant from the ends of the cores; on reversing either current the ends instantly receded, but the recession could be stopped by again changing the direction of the current. With a tranquil atmosphere, and an arrangement for reversing the current without shock or motion, the bar obeyed in an admirable manner the will of the experimenter, and, under the operation of the same forces, exhibited all the deflections sketched in figs. 40, 41, 42 and 43.

The motion of the bar cannot be referred to the action of induced currents. The bar was brought into the centre of the hollow cylinder in which it swung, and held there; the forces were all in action, and therefore all phænomena of induction passed; the arrangement of the forces being that shown in fig. 40, on releasing the bar it was driven from the cores, whereas when the arrangement was that shown in fig. 41, it was drawn towards them.

But it does not sufficiently express the facts to say that the bar is capable of two different states of excitement; it must be added, that both states exist simultaneously in the excited bar. We have already proved that the state necessary for the action of one pole is not that which enables an opposite pole to produce the same action; hence, when the two ends of the bar are attracted or repelled, at the same time, by two opposite poles, it is a proof that these two ends are in different states. But if this be correct, we can test our conclusion by reversing one of the poles; the direction of its force being thereby changed, it ought to hold the other pole in check and prevent all motion in the bar. This is the case: if, in any one of the instances cited, the polarity of either of the cores be altered; if the south be converted into a north, or the north into a south pole, thus making both poles of the same quality, the repulsion of the one is so nearly balanced by the attraction of the other, that the bar remains without motion towards either of them.

To carry the argument a step further, let us fix our attention for an instant upon fig. 40. The end of the bar nearest to the reader is repelled by a south pole; the same end ought to be *attracted* by a north pole. In like manner, the end of the bar most distant from the reader is repelled by a north pole, and hence the state of that end ought to fit it for *attraction* by a south pole. If, therefore, our reasoning be correct, when we place a north pole opposite to the lower end of the bar, and on the same side of it as the upper north pole, and a south pole opposite the upper end of the bar and on the same side of it as the lower south pole, the simultaneous action of these four poles ought to be more prompt and energetic than when only two poles are used. This arrangement is shown in Plate V.: the two poles to the right of the bismuth bar must be of the same name, and the two to the left of the bar of the opposite quality. If those to the right be both north, those to the left must be both south, and *vice versâ*. The current reverser for the magnets appears in front, that for the helix is hidden by the figure. The above conclusion is perfectly verified by experiments with this apparatus, and the twofold deflection of the bismuth bar is exhibited with remarkable energy*.

The bar used in these cases is far heavier than those commonly made use of in experiments on diamagnetism, but the dimensions stated do not mark the practical limit of the size of the bar. A solid bismuth cylinder, 14 inches long and 1 inch in diameter, was suspended in a helix 5·7 inches long, 1·8 inch internal diameter, 4 inches external diameter, and composed of copper wire 0·1 of an inch in thickness: when a current of twenty cells was sent through the helix, and the magnets (only two of them were used) were excited by one cell, all the phænomena exhibited by figs. 40, 41, 42 and 43, were distinctly exhibited.

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* These experiments, and almost all the others mentioned in this memoir, may be exhibited in the lecture-room. By attaching indexes of wood to the bars of bismuth, and protecting the indexes from air currents by glass shades, the motions may be made visible to several hundreds at once. See a description of a Polymagnet, Phil. Mag. June 1855.—J. T.

A considerable difference is always necessary between the strength of the current passing through the helix and that which excites the cores, so as to prevent the induction of the cores, which of itself would be followed by repulsion, from neutralizing, or perhaps inverting, the induction of the helix. When *two* magnets were used and the helix was excited by ten cells, I found the magnetic excitement by one or two cells to be most advantageous; when the cores were excited by ten, or even five cells, the action was always repulsive*. When four magnets were applied and the helix was excited by a battery of ten or fifteen cells, a power of five cells for the magnets was found efficient.

The deportment of paramagnetic bodies is so well known, that it might be left to the reader to discern that in all the cases described it is perfectly antithetical to that of the diamagnetic body. I have nevertheless thought it worth while to make the corresponding experiments with an iron bar; to facilitate comparison, the results are placed side by side in Plate IV. with those obtained with the bar of bismuth. It must be left to the reader to decide whether throughout this inquiry the path of strict inductive reasoning has been adhered to: if this be the case, then the inference appears unavoidable, *that the diamagnetic force is a polar force, the polarity of diamagnetic bodies being opposed to that of paramagnetic ones under the same conditions of excitement*†.

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* The perfect similarity of this deportment to that of soft iron under the same circumstances is evident.

† I would gladly refer to M. Plücker's results in connexion with this subject had I been successful in obtaining them; I will here, however, introduce the description of his most decisive experiment in his own words. (See Scientific Memoirs, New Ser. p. 336.)

"From considerations of which we shall speak afterwards, it appeared to me probable that bismuth not only assumes polarity in the vicinity of a magnetic pole, but that it also retains the polarity for some time after the excitation has taken place; or, in other words, that bismuth retains a portion of its magnetism permanently, as steel, unlike soft iron, retains a portion of the magnetism excited in it by induction. My conjecture has been corroborated by experiment.

"I hung a bar of bismuth, 15 millims long and 5 millims. thick, between the pointed poles of the large electro-magnet; it was suspended horizon-

VI. Concluding Observations: on M. Weber's Theory of Diamagnetic Polarity[*], and on Ampère's Theory of Molecular Currents.

It is well known that a voltaic current exerts an attractive force upon a second current, flowing in the same direction; and that when the directions are opposed to each other the force exerted is a repulsive one. By coiling wires into spirals, Ampère was enabled to make them produce all the phænomena of attraction and repulsion exhibited by magnets, and from this it was but a step to his celebrated theory of molecular currents. He supposed the molecules of a magnetic body to be surrounded by such currents, which, however, in the natural state of the body

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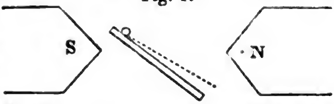
tally from a double cocoon-thread, fig. 1. The distance between the points was diminished until the bar could barely swing freely between them. A little rod of glass was brought near to one of the points, so that the bismuth bar, before the magnetism was excited, and in consequence of the torsion, leaned against the glass rod. On exciting the magnet by a current of three of Grove's elements, the bismuth, prevented from assuming the equatorial position, pressed more forcibly against the glass rod; when the current was interrupted, the bar remained still in contact with the rod, while its free end vibrated round its position of equilibrium. The current was closed anew and then reversed by a gyrotrope. In consequence of this reversion, the bar of bismuth, loosening from the glass rod, moved towards the axial position, but soon turned and pressed against the glass as before, or in some cases having passed quite through the axial position was driven round with the reversed ends into the equatorial. . . . . . This experiment, which was made with some care, proves that the bismuth requires time to reverse its polarity."

Fig. 1.



I have repeated this experiment with great care, and have obtained in part the effect described: it is perfectly easy to produce the rotation of the bar. The cause of this rotation, however, was in my case as follows:—When the magnet was unexcited, the position of equilibrium of the axis of the bar acted upon by the torsion of the fibre was that shown by the dotted line in the figure; when the magnetism was developed, the repulsive force acting on the free end of the bar necessarily pushed it beyond the dotted line—an action which was perfectly evident when the attention was directed towards it. On reversing the current, a little time was required to change the polarity of the iron masses; during this time the free end of the bismuth *fell* towards its former position, and the velocity acquired was sufficient to carry it quite beyond the pole points. The only difference between M. Plücker and myself is, that I obtained the same result by simply *intercepting* the current as by reversing it. I may remark that I have submitted ordinary bismuth to the most powerful and delicate tests, but as yet I have never been able to detect in it a trace of that retentive power ascribed to it by M. Plücker.

[*] Poggendorff's *Annalen*, vol. lxxxvii. p. 145, and Taylor's Scientific Memoirs, New Ser. p. 163.

mutually neutralized each other, on account of their confused grouping. The act of magnetization he supposed to consist in setting these molecular currents parallel to each other; and starting from this principle, he reduced all the phænomena of magnetism to the mutual action of electric currents.

If we reflect upon the experiments recorded in the foregoing pages from first to last; on the inversion of magnecrystallic phænomena by the substitution of a magnetic constituent for a diamagnetic; on the analogy of the effects produced in magnetic and diamagnetic bodies by compression; on the antithesis of the rotating actions described near the commencement; on the indubitable fact that diamagnetic bodies, like magnetic ones, owe their phænomena to an induced condition into which they are thrown by the influencing magnet, and the intensity of which is a function of the magnetic strength; on the circumstance that this excitation, like that of soft iron, is of a dual character; on the numerous additional experiments which have been recorded, all tending to show the perfect antithesis between the two classes of bodies;—we can hardly fail to be convinced that Mr. Faraday's first hypothesis of diamagnetic action is the true one—that diamagnetic bodies operated on by magnetic forces possess a polarity " the same in kind as, but the reverse in direction of that acquired by magnetic bodies." But if this be the case, how are we to conceive of the *physical mechanism* of this polarity? According to Coulomb's and Poisson's theory, the act of magnetization consists in the decomposition of a neutral magnetic fluid; the north pole of a magnet, for example, possesses an attraction for the south fluid of a piece of soft iron submitted to its influence, draws the said fluid towards it, and with it the material particles with which the fluid is associated. To account for diamagnetic phænomena this theory seems to fail altogether: according to it, indeed, the oft-used phrase, 'a north pole exciting a north pole, and a south pole a south pole,' involves a contradiction. For if the north fluid be supposed to be *attracted* towards the influencing north pole, it is absurd to suppose that its presence there could produce *repulsion*. The theory of Ampère is equally at a loss to explain diamagnetic action; for if we suppose the particles of bismuth surrounded by molecular currents, then according to all that is known of electro-dynamic laws, these currents would set themselves parallel to, and in the same direction as those of the magnet, and hence attraction, and not repulsion, would be the result. The fact, however, of this not being the case proves that these molecular currents are not the mechanism by which diamagnetic induction is effected. The consciousness of this, I doubt not, drove M. Weber to the assumption that the phænomena of diamagnetism are produced by molecular currents, not *directed*, but actually *excited* in the bismuth by the magnet. Such induced currents would, according to known laws, have a direction *opposed* to those of the inducing magnet, and hence would produce the phænomena of repulsion. To carry out the assumption here made, M. Weber is obliged to suppose that the molecules of diamagnetic bodies are surrounded by channels, in which the induced molecular currents, once excited, continue to flow without resistance.

This theory, notwithstanding its great beauty, is so extremely artificial, that I imagine the general conviction of its truth cannot be very strong; but there is one conclusion flowing from it which appears to me to be in direct opposition to experimental facts. The conclusion is, "*that the magnetism of two iron particles in the line of magnetization is increased by their reciprocal action; but that, on the contrary, the diamagnetism of two bismuth particles lying in this direction is diminished by their reciprocal action.*" The reciprocal action of the particles varies inversely as the cube of the distance between them: at a distance expressed by the number 1, for example, the enfeeblement is eight times what it would be at the distance 2.

The conclusion, as regards the iron, is undoubtedly correct; but I believe experiment proves that the mutual action of diamagnetic molecules, when caused to approach each other, *increases* their repulsive action. I have had massive iron moulds made and coated with copper by the voltaic current; into these fine bismuth powder has been introduced and submitted to powerful hydraulic pressure. No sensible fact can, I think, be more certain, than that the particles of this dust are brought into closer proximity along the line in which the pressure is exerted, and this is the line of *strongest diamagnetization*. If a portion of the compressed mass be placed upon the end of a torsion beam and the amount of repulsion measured, it will be found that the repulsion is a maximum when the line of magnetization coincides with the line of compression; or, in other words, with that line in which the particles are packed most closely together: if the bismuth were fixed, and the magnet moveable, the former would repel the latter with a maximum force with the line of compression parallel to the direction of magnetization: it is a stronger *diamagnet* in this direction than in any other. Cubes of bismuth, which, in virtue of their crystallization, possessed a line of minimum magnetization, have been placed in those moulds and pressed closely together in the direction of the said line: the approximation of the particles thus effected has converted the direction spoken of from one of minimum into one of maximum magnetization. It would be difficult for me to say how many diamagnetic bodies I have submitted to compression, some massive, some in a state of powder, but in no single instance have I discovered an exception to the law that the line of compression of purely diamagnetic bodies is the line of strongest diamagnetization. The approximation of diamagnetic particles is therefore accompanied by an augmentation of their power, instead of a diminution of it, as supposed by the theory of M. Weber.

Any hypothesis which involves the idea of the diminution of the diamagnetic action of a body by the approximation of its particles, is, I believe, opposed to facts. Such a hypothesis must, I imagine, form the basis of the following remark of Prof. W. Thomson:—referring to "a thin bar or needle of a diamagnetic substance," he says, "such a needle has no tendency to arrange itself across the lines of magnetic force; but, as will be shown in a future paper, if it be very small compared with the dimensions and distance of the magnet, the direction it will assume, when allowed to turn freely round its centre of gravity, will be that of the lines of force*." I have not found in any of the subsequent numbers of the Philosophical Magazine the proof here promised†. But I doubt not the conclusion involves the assumption that the mutual action of diamagnetic particles is to weaken each other, and hence to produce a more feeble magnetization *along* a thin diamagnetic bar than *across* it—an assumption which, as already shown, is contradicted by experiment.

It is scarcely possible to reflect upon the discovery of Faraday in all its bearings, without being deeply impressed with the feeling that we know absolutely nothing of the physical causes of magnetic action. We find the magnetic force producing, by processes which are evidently similar, two great classes of effects. We have a certain number of bodies which are attracted by the magnet, and a far greater number which are repelled by the same agent. Supposing these facts to have been known to Ampère, would he have satisfied his profound mind by founding a theory which accounts for only the smaller portion of them? This theory is admirable as far as it goes, but the generalization is yet to come which shall show the true relationship of phænomena, towards whose connexion the theory of Ampère furnishes at present no apparent clue.

Royal Institution, October 1854.

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* Phil. Mag. vol. xxxvii. p. 244.

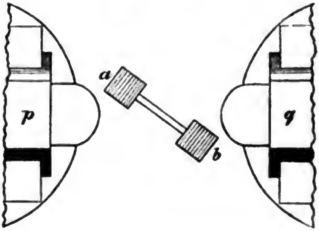
† This remark appears to have induced Mr. Thomson to publish the proof referred to in the last Number of the Philosophical Magazine. The arguments there brought forward have been long familiar to me, but I regret to say that I cannot attach much real value to them. At some future day I hope to be able to justify the scepticism which I here venture to express.—J. T., May 5, 1855.

Note on M. Matteucci's *Objections.*

The foregoing memoir was on the point of leaving my hands for the Royal Society, when accident, backed by the kindness of Mr. Faraday, placed the *Cours Spécial* of M. Matteucci, recently published in Paris, in my hands. An evening's perusal of this valuable work induces me to append the following remarks to the present paper.

M. Matteucci honours the researches which bear my name, and those which I published in connexion with M. Knoblauch, with a considerable share of his attention. He corroborates all the experimental facts, but at the conclusion states three objections to the manner in which these facts have been explained. "La faveur," writes the learned Italian, "avec laquelle les idées de MM. Tyndall et Knoblauch ont été accueillies m'imposent le devoir de ne pas vous laisser ignorer les objections qui s'élèvent contre elles. La première consiste dans la différence très-grande et constant dans la force qui fait osciller entre les poles un aiguille de bismuth cristallisé, suivant que ses clivages parallèles à sa longueur sont suspendus verticalement ou dans un plan horizontal : cette différence me parait inconciliable avec le résultat déjà rapporté de l'expérience de M. Tyndall, sur lequel se fonde l'explication des phénomènes magneto-cristallisés. Mais une objection encore plus grave est celle du mouvement d'*attraction** vers les poles qui se manifeste dans les prismes de bismuth cristallisé dont les clivages sont perpendiculaires à leur longueur. Pour rendre la conséquence de cette dernière expérience encore plus évidente, j'ai fixé deux cubes de bismuth, qui ont deux faces opposées naturelles et parallèles aux plans de clivage, aux extrémités d'un petit levier de verre, ou de sulphate de chaux, suspendu par un fil de cocon au milieu du champ magnétique entre les extrémités polaires d'un electro-aimant (fig. 27*a*) ; lorsque les deux cubes ont les clivages verticaux et perpendiculaires à la longueur à l'aiguille, au moment où le circuit est fermé, l'aiguille est attiré, *quelle que soit la position* qu'elle occupe dans le champ magnétique, et se fixe en equilibre dans la ligne polaire. . . . . . Il me semble impossible d'expli-

Fig. 27*a*.



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* This is in reality not a 'movement of *attraction*,'—see Appendix to the present paper.—J. T., May 1855.

quer ces mouvements du bismuth cristallisé, comme on a essayé de la faire, par la force repulsive de l'aimant, qui, suivant l'expérience de M. Tyndall*, s'exerce avec plus d'intensité parallèlement aux clivages que dans la direction perpendiculaire à ces plans.

"Remarquons encore qu'on ne trouve pas constamment l'accord qui devrait exister, selon les idées de MM. Tyndall et Knoblauch, entre les phénomènes magneto-cristallisés et les effets produits par la compression dans le bismuth, si l'on considère ces plans de clivages et la ligne suivant laquelle la compression a eu lieu comme jouissant des même propriétés†."

With regard to the first objection, I may say that it is extremely difficult to meet one so put; it is simply an opinion, and I can scarcely say more than that mine does not coincide with it. I would gladly enter upon the subject and endeavour to give the objection a scientific form were the necessary time at my disposal, but this, I regret to say, is not the case at present. I shall moreover be better pleased to deal with the objection after it has assumed a more definite form in the hands of its proposer, for I entertain no doubt that it is capable of a sufficient answer. The second objection M. Matteucci considers to be a more grave one. The facts are as follows:—the repulsion of a mass of crystallized bismuth depends upon the direction in which the mass is magnetized. When the magnetizing force acts in a certain direction, the intensity of magnetization, and the consequent repulsion of the mass, is a maximum. This is proved by placing the mass upon the end of a torsion beam and bringing its several directions successively into the line of the magnetic force. Poisson would have called such a direction through the mass a principal axis of magnetic induction, and I have elsewhere called it a line of elective polarity. When a sphere or cube of bismuth is freely suspended in the magnetic field, with the direction referred to horizontal, in all positions except two the forces acting on the mass tend to turn it; those positions are, when the line of maximum magnetization is axial and when it is equatorial, the former being a position of unstable, and the latter a position of stable equilibrium. When the above line is oblique to the direction of magnetization, the sphere or cube will turn round its axis of suspension until the direction referred to has set itself at right angles to the line joining the poles. Now if the direction of maximum magnetization be transverse to an elongated mass of bismuth, such a mass must, when the said direction recedes to the equator, set its length from pole to pole.

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* This was first proved by Mr. Faraday.—J. T.

† *Cours spécial sur l'induction, &c.*, p. 255.

The facts observed by M. Matteucci seem to me to be a simple corroboration of this deduction*.

The third objection is directed against an imaginary case, "si l'on considère les plans de clivage et la ligne de compression comme jouissant des même propriétés." It must be evident that a crystal like bismuth, possessing a number of cleavages of unequal values, cannot be compared in all respects with a body which has suffered pressure *in one direction* only. I have no doubt whatever, that, by a proper application of force in different directions, a compressed mass might be caused to imitate to perfection every one of the actions exhibited by crystallized bismuth. Indeed I would go further, and say, that I shall be happy to undertake to reproduce, with bismuth powder, the deportment of any diamagnetic crystal whatever that M. Matteucci may think proper to name.

In looking further over M. Matteucci's instructive book, I find another point alluded to in a manner which tempts me to make a few remarks in anticipation of a fuller examination of the subject. The point refers to the reciprocal action of the particles of magnetic and diamagnetic bodies. It is easy to see, that if the attraction of a bar of iron varies simply as the number of the particles attracted, then, inasmuch as the weight of the body varies in the same ratio, and the moment of inertia as the weight, the times of oscillation of two masses of the same length, but possessing different numbers of attracting particles, must be the same. Coulomb indeed mixed iron filings with wax, so as to remove the particles out of the sphere of their mutual inductive action, and proved that when needles of equal lengths, but of different diameters, were formed from the same mixture, the duration of an oscillation was the same for all. From this he inferred that the attractive force is simply proportional to the number of ferruginous particles; but this could not be the case if these particles exerted any sensible reciprocal action, either tending to augment or diminish the induction due to the direct action of the magnet. On account of such a mutual action, two bars of soft iron, of the same length, and of different diameters, have not the same time of oscillation.

In examining the question whether the particles of diamagnetic bodies exert a similar reciprocal action, M. Matteucci fills quills of the same length, and of different diameters, with powdered bismuth, and finds that there is no difference between the duration of an oscillation of the thick ones and the slender ones; from this he infers that there can be no reciprocal action among the particles of the bismuth.

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* For a more complete examination of this subject see the Appendix to this paper.—J. T., May 1855.

Now it is not to be imagined that even in Coulomb's experiments with the iron filings the molecular induction was absolutely nothing, but simply that it was so enfeebled by the separation of the particles that it was insensible in the experiments. This remark applies with still greater force to M. Matteucci's experiments with the bismuth powder; for the enfeeblement of a force already so weak, by the division of the diamagnetic mass into powder, must of course practically extinguish all reciprocal action of the particles, even supposing a weak action of the kind to exist when the mass is compact.

I will not here refer to my own experiments on compressed bismuth, but will take a result arrived at by M. Matteucci himself while repeating and corroborating these experiments. "I made," says M. Matteucci, "two cylinders of bismuth precisely of the same dimensions, the one compressed, the other in its natural state, and found that the compressed mass had a diamagnetic power *distinctly superior* to that of natural bismuth*." Now M. Matteucci, in his *Cours Spécial*, has made his own choice of a test of reciprocal molecular action; he assumes that if cylinders of the same length, but of different masses, have equal times of oscillation, it is a conclusive proof that there is no action of the kind referred to. This necessarily implies the assumption, that were the times of oscillation *different*, a reciprocal action would be demonstrated. Now, according to his experiments described in the Association Report, the times of oscillation *are* different; the diamagnetism of the compressed cylinder is "*distinctly superior*" to that of the uncompressed one: *the diamagnetic effect increases in a greater proportion than the quantity of matter*; and hence, on M. Matteucci's own principles, the result negatived by his experiments on powdered bismuth is fairly established by those which he has made with the compressed substance.

APPENDIX†.

Reflecting further on the subject of diamagnetic polarity, an experiment occurred to me which constitutes a kind of crucial test to which the conclusions arrived at in the foregoing memoir may be submitted.

Two square prisms of bismuth, 0·43 of an inch long and 0·2 of an inch wide, were laid across the ends of a thin plate of cedar wood, and fastened there by white wax. Another similar plate of wood was laid over the prisms, and also attached to them by wax; a kind of rectangular box was thus formed, 1 inch long and of the same width as the length of the prisms, the ends of the box being formed by the prisms, while its sides were open. Both plates of wood were pierced through at the centre, and in the aperture thus formed a wooden pin was fixed, which could readily be attached to a suspending fibre. Fig. 1 represents the arrangement both in plan and section.

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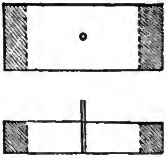
* Report of British Association for 1852, Transactions of Sections, p. 7.

† Received December 21, 1854.

Fig. 1.



The prisms first chosen were produced by the compression of fine bismuth powder, without the admixture of gum or any other foreign ingredient, the compressed mass being perfectly compact and presenting a surface of metallic brilliancy. If such a mass be placed on the end of a torsion balance and a magnetic pole is brought to bear upon it, I have proved the repulsion to be maximum when the direction in which the mass has been compressed is in the continuation of the axis of the magnet. A comparative view of the repulsion in this direction, and in another perpendicular to it, is given in the following Table:—

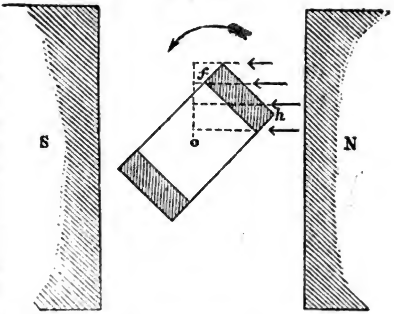
Compressed bismuth powder.

	Repulsion.	
Strength of magnet.	Line of pressure axial.	Line of pressure equatorial.
5·8	22	13
8·4	46	31
10·0	67	46
11·9	98	67

We see here that the repulsion, when the line of pressure is axial, exceeds what occurs when the same line is equatorial by fully one-half the amount of the latter. Now this can only be due to the more intense magnetization, or rather diamagnetization, of the bismuth along the line of pressure; and in the experiment now to be described I availed myself of this fact to render the effect more decided.

The prisms of bismuth were so constructed that the line of pressure was parallel to the length of each. The rectangular box before referred to was suspended from its centre of gravity O in the magnetic field, so that the two prisms were in the same horizontal plane. Let the position of the box thus suspended horizontally be that shown in fig. 2. For the sake of simplicity, we will confine our attention to the action of one of the poles N, which may be either flat or rounded, upon the prism *hf* adjacent to it, as indeed all the phænomena to be described can be produced before a single pole. The direction of the force emanating from N is represented by the arrows; and if this force be *purely repulsive,* the action upon every single particle of the diamagnetic mass furnishes a moment which, in the position here assumed, tends to turn the rectangular box in the direction marked by the arrow. It is perfectly impossible that such a system of forces could cause the box to turn in a direction opposed to the arrow; yet this is the precise direction in which the box turns when the magnetic force is developed.

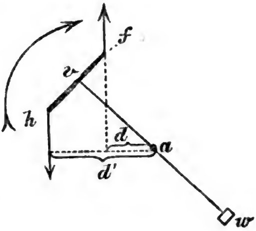
Fig. 2.



Here, then, we have a mechanical effect which is perfectly inexplicable on the supposition that the diamagnetic force is purely repulsive. But if the conclusions arrived at in the foregoing memoir be correct, if the diamagnetic force be a polar force, then we must assume that attraction and repulsion are developed simultaneously, as in the case of ordinary magnetic phænomena. Let us examine how this assumption will affect the analysis of the experiment before us.

The marked end of a magnetic needle is pulled towards the north magnetic pole of the earth; and yet, if the needle be caused to float upon a liquid, there is no motion of its mass towards the terrestrial pole referred to. The reason of this is known to be, that the south end of the needle is repelled by a force equal to that by which the north, or marked end, is attracted. These two equal and opposite forces destroy each other as regards *a motion of translation,* but they are effective in producing *a motion of rotation.* The magnetic needle, indeed, when in a position oblique to the plane of the magnetic meridian, is solicited towards that plane by a mechanical couple, and if free to move, will turn and find its position of equilibrium there.

Fig. 3.



Let such a needle, *fh,* be attached, as in fig. 3, to the end of a light wooden beam, *vw*; let the beam and needle be suspended horizontally from the point *a,* round which the whole system is free to turn, the weight of the needle being balanced by a suitable counterpoise, w; let the north pole of the earth be towards N. Supposing the beam to occupy a position oblique to the magnetic meridian, as in the figure, the end f, or the marked end, of the needle is solicited towards N by a force ϕ, and the tendency of this force to produce rotation in the direction of the arrow is expressed by the product of ϕ into the perpendicular drawn from the axis of rotation upon the direction of the force. Setting this distance $=d$, we have the moment of ϕ in the direction stated,

$$=\phi d.$$

The end h of the needle is repelled by the earth's magnetic pole with a force ϕ': calling the distance of the direction of this latter force from the axis of rotation d', we have the moment of ϕ' in a direction opposed to the arrow,

$$=\phi' d'.$$

Now as the length of the needle may be considered a vanishing quantity, as compared with its distance from the terrestrial pole, we have practically

$$\phi=\phi',$$

and consequently

$$\phi d<\phi' d'.$$

The tendency to turn the lever in a direction opposed to the arrow is therefore predominant; the lever will obey this tendency, and move until the needle finds itself in the magnetic meridian: when this position is attained, the predominance spoken of evidently ceases, and the system will be in equilibrium. Experiment perfectly corroborates this theoretic deduction.

In this case, the centre of gravity of the needle recedes from the north magnetic pole as if it were repelled by the latter; but it is evident that the recession is not due either to the attraction or repulsion of the needle considered as a whole, but simply to the mechanical advantage possessed by the force ϕ', on account of its greater distance from the axis of rotation. If the force acting upon every particle of the needle were purely *attractive*, it is evident that no such recession could take place. Supposing, then, that we were simply acquainted with the fact, that the end f of the needle is attracted by the terrestrial pole, and that we were wholly ignorant of the action of the said pole upon the end h, the experiment here described would lead us infallibly to the conclusion that the end h must be repelled. For if it were attracted, or even if it were neither attracted nor repelled, the motion of the bar must be *towards* the pole N instead of in the opposite direction.

Let us apply this reasoning to the experiment with the bismuth prisms already described. The motion of the magnetic needle in the case referred to is not more inexplicable, on the assumption of a purely attractive force, than is the motion of our rectangular box on the assumption of a purely repulsive one; and if the above experiment would lead to the conclusion that the end h of the magnetic needle is repelled, the experiment with the bismuth leads equally to the conclusion that the end f of the prism hf, fig. 2, must be *attracted* by the pole N. The assumption of such an attraction, or in other words, of diamagnetic polarity, is alone capable of explaining the effect, and the explanation which it offers is perfect.

On the hypothesis of diamagnetic polarity, the prism hf turns a hostile end h to the magnetic pole N, and a friendly pole f away from it. Let the repulsive force acting upon the former be ϕ, and the attractive force acting upon the latter ϕ'. It is manifest that if ϕ were equal to ϕ', as in the case of the earth's action, or in other words, if the field of force were perfectly uniform, then, owing to the greater distance of ϕ' from the axis of rotation, from the moment at which the rectangular box quits the equatorial position, which is one of unstable equilibrium, to the moment when its position is axial, the box would be incessantly drawn towards the position last referred to.

But it will be retorted that the field of forçe is not uniform, and that the end h, on account of its greater proximity to the magnet, is more forcibly repelled than the end f is attracted: to this I would reply, that it is only in "fields" which are approximately uniform that the effects can be produced; but to produce motion towards the pole, it is not necessary that the field should be perfectly uniform: setting, as before, the distance of the direction of the force ϕ from the axis of rotation $=d$, and that of the force $\phi'=d'$, a motion towards the pole N will always occur whenever

$$\frac{d'}{d} > \frac{\phi}{\phi'}.$$

To ascertain the diminution of the force on receding from a polar surface such as that here used, I suspended a prism of bismuth, similar to those contained in the rectangular box, at a distance of 0·9 of an inch from the surface of the pole. Here, under the action of the magnet excited by a current of ten cells, the number of oscillations accomplished in a second was 17; at 0·7 of an inch distant the number was 18; at 0·5 of an inch distant the number was 19; at 0·3 distant the number was 19·5; and at 0·2 distant the number was 20. The forces at these respective distances being so very little different from each other, it follows that a very slight deviation of the box from the equatorial position is sufficient to give the moment of ϕ' a preponderance over that of ϕ, and consequently to produce the exact effect observed in the experiment.

The consistency of this reasoning is still further shown when we operate in a field of force which diminishes speedily in intensity as we recede from the magnet. Such a field is the space immediately in front of pointed poles. Suspending our rectangular box between the points, and causing the latter to approach until the box has barely room to swing between them, it is impossible to produce the phænomena which we have just described. The intensity with which the nearest points of the bismuth bar are repelled so much exceeds the attraction of the more distant end, that the moment of attraction is not able to cope successfully with the moment of repulsion; the bars are consequently repelled *en masse,* and the length of the box takes up a position at right angles to the line which unites the poles.

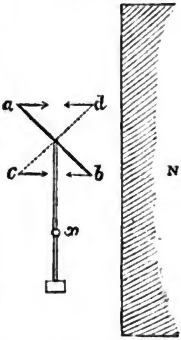
It is manifest, however, that by increasing the distance between the bismuth bar and the points acting upon it, we diminish the difference of action upon the two ends of the bar. When the distance is sufficient, we can produce, with the pointed poles, all the phænomena exhibited between flat or rounded ones.

All the effects which have been described are produced with great distinctness when, instead of compressed bismuth, two similar bars of the crystallized substance are used, in which the planes of principal cleavage are parallel to the length. Such bars are not difficult to procure, and they ought to hang in the magnetic field with the planes of cleavage vertical. It is unnecessary to describe the experiments made with such bars; they exhibit with promptness and decision all the effects observed with the compressed bismuth.

We have hitherto operated upon elongated masses of bismuth; but with the compressed substance, or with the substance crystallized uniformly in planes, as in the case last referred to, an elongation of the mass is not necessary to the production of the effects described. Previous, however, to the demonstration of this proposition, I shall introduce a kind of lemma, which will prepare the way for the complete proof.

Diamagnetic bodies, like paramagnetic ones, vary considerably in the intensity of their forces. Bismuth or antimony, for example, exhibits the diamagnetic force with greater energy than gold or silver, just as iron or nickel exhibits the magnetic force with greater energy than platinum or chromium. Let two thin bars, *ab, cd,* fig. 4, of two bodies of different diamagnetic powers be placed at right angles to each other, so as to form a cross; let the cross be attached to the end of a lever and suspended horizontally from the point x, before the flat or rounded pole N of a magnet. Let the continuous line *ab* represent the needle of the powerful diamagnetic body, and the broken line *cd* that of the feeble one. On the former a mechanical couple acts in the direction denoted by the arrows at its ends; and on the latter a couple operates in the direction of the arrows at *its* ends. These two couples are evidently opposed to each other; but the former being, by hypothesis, the more powerful of the two, it will overcome the latter. The mechanical advantage possessed by the *attracted* end *a* of the more powerful bar, on account of its greater distance from the axis of suspension x, will, in an approximately uniform field of force which we here assume, cause the centre of gravity of the cross to move towards the pole N.

Fig. 4.



In the formation of such a cross, however, it is not necessary to resort to two different substances in order to find two needles of different diamagnetic powers; for in crystallized bodies, or in bodies subjected to mechanical pressure, the diamagnetic force acts with very different energies in different directions. Let a mass of a diamagnetic body which has been forcibly compressed in one direction be imagined; let two needles be taken from such a mass, the one with its length parallel, and the other with its length perpendicular to the line of pressure. Two such needles, though composed of the same chemical substance, will behave exactly as the two bars of the cross in the experiment last described; that needle whose length coincides with the line of pressure will bear the same relation to the other that the needle of the powerfully diamagnetic substance bears to that of the feeble one. An inspection of the table at page 282 will show that this must be the case.

It is also shown in the following table, that in masses of crystallized bismuth the diamagnetic repulsion acts with very different energies in different directions. Cubes were taken from a mass of bismuth with the planes of principal cleavage parallel throughout to two opposite faces of each cube. The cubes were placed upon the ends of a torsion balance, and the diamagnetic repulsion was accurately measured when the force acted parallel to the planes of cleavage. The cubes were then turned 90° round, and the repulsion was measured when the force acted perpendicular to the planes referred to.

Cubes of crystallized bismuth.

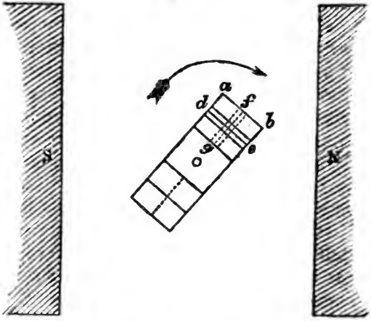
	Repulsion when the force was directed	
Strength of magnet.	along the cleavage.	across the cleavage.
3·6	11·7	8
5·7	34·8	23
8·4	78	53
10·0	111	76·5
11·9	153	110

It is manifest from this table that bismuth behaves as a body of considerably superior diamagnetic power when the force acts *along* the planes of cleavage.

Let two indefinitely thin needles be taken from such a mass, the one with its length parallel, and the other with its length perpendicular to the planes of cleavage; it is evident that if two such needles be formed into a cross and subjected to experiment in the manner above described, the former will act the part of the more powerfully diamagnetic needle, and produce similar effects in the magnetic field.

We now pass on to the demonstration of the proposition, that it is not necessary that the crystallized masses should be elongated to produce the effects exhibited by the prisms in the experiments already recorded. Let us suppose the ends of our rectangular box to be composed of cubes, instead of elongated masses, of crystallized bismuth, and let the planes of principal cleavage be supposed to be parallel to the face *ab*, fig. 5. Let the continuous line *de* represent an indefinitely thin slice of the cube passing through its centre, and the dotted line *gf* a similar slice in a perpendicular direction. These two slices manifestly represent the case of the cross in fig. 4; and were they alone active, the rectangular box, in a uniform field of magnetic force, must turn in the direction of the arrow. Comparing similar slices *in pairs* on each side of those two central slices, it is manifest that every pair parallel to the line *de* represents a stronger mechanical couple than every corresponding

Fig. 5.



pair parallel to *fg*. The consequence is, that a cube of crystallized bismuth suspended in the manner described, in a sufficiently uniform field of magnetic force, will move in the same direction as the cross in fig. 4: its centre of gravity will therefore *approach* the pole N, which was to be demonstrated.

This deduction is perfectly illustrated by experiment. It is manifest that the effect of the pole S upon the cube adjacent to it is to increase the moment of rotation of the rectangular box: the same reasoning applies to it as to the pole N.

Referring to fig. 27*a*, page 278, it will be seen that we have here dealt with the second and gravest objection of M. Matteucci, and converted the facts upon which the objection is based into a proof of diamagnetic polarity, so cogent that it alone would seem to be sufficient to decide this important question. Holding the opinion entertained by M. Matteucci regarding the nature of diamagnetic force*, his objection must have appeared to him to be absolutely unanswerable: I should be glad to believe that the remarks contained in this Appendix furnish, in the estimation of the distinguished philosopher referred to, a satisfactory explanation of the difficulty which he has disclosed.

Let me, in conclusion, briefly direct the reader's attention to the body of evidence laid before him in the foregoing pages. It has been proved that matter is repelled by the pole of a magnet in virtue of an induced condition into which the matter is thrown by such a pole. It is shown that the condition evoked by one pole is not that which is evoked by a pole of an opposite quality—that each pole excites a condition peculiar to itself. A perfect antithesis has been shown to exist between the deportment of paramagnetic and diamagnetic bodies when acted on by a magnet alone, by an electric current alone, or by a magnet and an electric current combined. The perplexing phænomena resulting from molecular structure have been laid open, and the antithesis between paramagnetic and diamagnetic action traced throughout. It is further shown, that whatever title to polarity the deportment of a bar of soft iron, surrounded by an electric current, and acted on by other magnets, gives to this substance, a bar of bismuth possesses precisely the same title: the disposition of forces, which in the former case produces attraction, produces in the latter case repulsion, while the repulsion of the iron finds its exact complement in the attraction of the bismuth. Finally, we have a case adduced by M. Matteucci which suggests a crucial experiment to which all our previous reasoning has been submitted, by which its accuracy has been proved, and the insufficiency of the assumption, that the diamagnetic force is not polar, is reduced to demonstration. When we remember that against all this no single experimental fact or theoretic argument* which can in any degree be considered as conclusive, has ever been brought forward, nor do I believe can be brought forward, the conclusion seems irresistible, that we have in the agency by which bodies are repelled from the poles of a magnet, a force of the same dual character as that by which bodies are attracted; that, in short, "diamagnetic bodies possess a polarity the same in kind but the opposite in direction to that possessed by magnetic ones."

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* "Il ne peut exister dans les corps diamagnétiques une polarité telle qu'on la concoit dans le fer doux."—*Cours Spécial*, p. 201.

XXXV. *Experiments on some of the Compounds of Tin with Arsenic.* By Ed. Haeffely *of Mulhouse*†.

By pouring an excess of nitric acid into a solution of stannate of soda and arseniate of soda, in which the arseniate predominates, and bringing this mixture to a state of ebullition, a white gelatinous precipitate is produced, composed of arsenic acid, peroxide of tin, and water; when washed and dried at the ordinary temperature, it forms transparent fragments.

To analyse the above compound, the delicate process described by M. Levol‡ was employed, viz. placing within a glass tube heated to redness 100 parts in an anhydrous state, and passing through it a current of hydrogen, when the tin is left in the metallic form, and the arsenic sublimed in the tube. Three analyses gave,—

Tin . . .	45	46·3	45·21
Arsenic . .	27·2	26·3	

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* I ought perhaps to except an argument of Professor W. Thomson's, which professes to prove that an absolute creation of force, and the setting up of a perpetual motion, would follow, if diamagnetic polarity were conceded. While expressing my admiration of the ingenuity of Mr. Thomson's reasoning, it appears to me to labour under the disadvantage of proving too much, his conclusion being equally fatal to polarity of all kinds. The argument, I believe, was first publicly urged against myself at the Belfast Meeting of the British Association; but at the Liverpool Meeting last year Professor Thomson himself admitted "that he had not perfect confidence in the truth of the conclusion, as one of the assumptions on which the reasoning was founded admitted of doubt."—See Athenæum, 1854, p. 1204. Indeed, from many of his published papers, it might be inferred that Mr. Thomson actually *assumed* what I, in the present memoir, have attempted to *prove*.

I refrain from alluding to the negative results obtained by Mr. Faraday in repeating M. Weber's experiments; for though admirably suited to the exhibition of certain effects of ordinary induction, Mr. Faraday himself has shown how unsuitable the apparatus employed would be for the investigation of the question of diamagnetic polarity. See Experimental Researches (2653, 2654), vol. iii. p. 143.—J. T., May 9, 1855.

† Communicated by the Author.

‡ *Annales de Chimie et de Physique*, 3 sér. vol. xvi.

By adding to these numbers the respective quantities of oxygen required for their maximum oxidation, we arrive at the formula—

$$AsO^5 + 2SnO^2,$$

which requires—

Tin . .	44·52	Arsenic . .	28·31

Dried in the stove at 120° C. the loss amounted to 25·3 per cent., corresponding to 10 equivalents.

The formation of an insoluble arseniate of tin is nothing new of itself, for it has long been known that metalloidic acids form insoluble precipitates with certain insoluble metallic acids. I only point out the favourable circumstance by which a constant compound is formed, namely, by taking care to operate with an excess of the metalloidic acid. Phosphoric acid in excess gives with peroxide of tin,—

$$PO^5\,2SnO^2 + 10HO.$$

M. Levol, in seeking for an easy method to ascertain the quantity of arsenic in the metals and their alloys, operated in all his experiments with an excess of the metallic oxide, by which he obtained but imperfect precipitates of tin with arsenic (or antimony with arsenic). This led him to abandon the hope of determining this element by precipitation; he says, "the increase of weight which arsenic acid obtains by its combination with hydrated peroxide of tin (or antimony) did not appear to me a sufficient warrant for determining the arsenic from it in a rigorous analysis."

M. Levol's combining proportion varies from 1 of arsenic to 15, and 20 of tin: but operating under different circumstances, I am able to combine 12·72 of the metalloid with 20 of the metal. The above result may be applied in quantitative analysis, when a mere calculation will replace the delicate and somewhat complicated operation of M. Levol. I turned this formation of the constant compound $AsO^5\,2SnO^2$ to account in the analysis of the commercial stannates of soda, containing large admixtures of arseniate of soda.

To a known weight of a commercial stannate I add a *known quantity of arseniate of soda*, boil with an excess of nitric acid, collect the insoluble arseniate of tin $AsO^5\,2SnO^2$, and from its weight calculate the amount of *arsenic acid* and peroxide of tin.

The liquid filtrates are treated with sulphuretted hydrogen, to throw down as sulphuret of arsenic the *excess of arsenic acid* added intentionally. The amount of arsenic acid found, and the amount added intentionally being known, it will be easy to calculate the original quantity which the stannate contained.

Decomposition of $AsO^5\,2SnO^2\,10HO$.

In presence of an excess of a fixed alkali (soda used) this arseniate of tin is split into two different soda salts, one of which contains all the arsenic, and crystallizes at first in silky needles, while the mother-liquors consist of ordinary stannate of soda: the reaction takes place according to the equation—

$$2(AsO^5\,2SnO^2)+9NaO=\underbrace{2(AsO^5,6NaO,SnO^2)}+3(SnO^2\,NaO).$$

This same salt is also obtained by boiling 2 equivalents of terbasic arseniate of soda with 1 equivalent of hydrated peroxide of tin. (When hydrate of alumina is taken, an arseniate of alumina and soda is obtained.) To analyse the tin salt, I determined each constituent by direct processes.

A current of hot air deprived of its carbonic acid passed over 100 parts of the salt, gave a loss of 48 per cent. of water.

100 parts treated with an excess of boiling nitric acid gave 13·95 parts of the insoluble arseniate of tin, $AsO^5\,2SnO^2$, corresponding to—

$$\begin{array}{ll} 7{\cdot}887 & SnO^2=2 \text{ equivs.} \\ \underline{6{\cdot}063} & AsO^5=1 \text{ equiv.} \\ 13{\cdot}95 & \end{array}$$

100 parts acidulated with hydrochloric acid, treated with a current of sulphuretted hydrogen, gave 41·5 parts of double sulphuret of tin and arsenic.

These two weights being ascertained, a mere calculation may be substituted for the tedious operation of M. Levol, for it remains only to calculate the corresponding amount of double sulphurets which 13·95 parts of arseniate of tin would form, to be deducted from the total amount of double sulphuret, 41·5, obtained; as follows:—

$$13{\cdot}95 \text{ of } AsO^5\,2SnO^2=15{\cdot}825=AsS^5\,2SnS^2;$$

and 15·825 $AsS^5\,2SnS^2$ deducted from 41·5 double sulphuret, leaves 25·675 of $AsS^5=19{\cdot}05\,AsO^5$.

By evaporating to dryness the hydrochloric acid filtrates, I get 37 of chloride of sodium $=19{\cdot}6$ soda.

Recapitulation of the Numbers.

	By experiment.	Calculated.	Equivalent numbers.	
AsO^5 . .	25·113	24·442	115	1 eq.
SnO^2 . .	7·887	7·970	93	3 ...
NaO . .	19·6	19·766	37·5	$\frac{1}{2}$ ...
HO . .	48	47·822	225	25
	100·6	100·000	470·5	

The formula of this salt is therefore—

$$2(AsO^5\ 6NaO\ SnO^2)\ 50HO.$$

Stannate of soda is used as a mordant in print and dye works. I have made numerous experiments on a large scale to decide whether in calico printing or dyeing, the hydrated peroxide of tin alone would be preferable to an arseniate of peroxide of tin as a mordant. The result of comparative trials leads me to give the preference to hydrated peroxide of tin alone, the various shades being more brilliant and less liable to unevenness than when arsenic acid is present. It would be desirable to substitute in commerce a purer stannate of soda for that at present sold, as well as for that containing arseniate of soda.

These experiments were made in the laboratory of Harrison Blair, Esq., near Manchester, who kindly afforded me every opportunity for carrying out the same. They have led at least to this practical fact, that the danger arising from the employment of arseniates in stannates of soda may be obviated by the use of a pure stannate of soda alone.

XXXVI. *Improvements on a Dew-point Hygrometer lately described by the Author*[*]. *By* Professor CONNELL[†].

A FEW simple alterations have greatly facilitated the employment of this instrument, and at the same time made it considerably less liable to injury. It consists, as will be recollected, of a little bottle of thin brass, into which the bulb of a thermometer is introduced so as to fit air-tight, and the neck of which is attached laterally to a small exhausting syringe. Æther having been previously introduced into the bottle, the temperature is by a gradual process of exhaustion reduced, until moisture begins to be condensed on the polished surface of the bottle, when the temperature of deposition is indicated by the thermometer. A little collar of ivory is introduced between the bottle and the syringe to prevent the heat produced by the friction of the piston and cylinder from being communicated to the bottle. The valves of the syringe are made of gold-beater's leaf, as the vapour of the æther was found to act on valves of oiled silk.

As washers of leather required to be introduced on both sides of the ivory collar to make the connexions air-tight, considerable difficulty was experienced, from the varying thickness of these

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[*] Edinburgh Philosophical Transactions for 1854, and Philosophical Magazine, vol. viii. p. 81.

[†] Communicated by the Author, having been read to the Physical Section of the British Association at Glasgow, September 17, 1855.

washers, in bringing the bottle with the inserted thermometer into the proper vertical position. This inconvenience has been completely remedied by connecting the neck of the bottle with the syringe by means of a coupling screw, instead of an ordinary screw. In this way the bottle and thermometer can be at once brought into any required position, without the use of any washer at all on that side; and as the screw projecting from the ivory collar on that side is now made of brass, instead of being of ivory as before, it is not subject to fracture when any little additional force is necessary in order to unscrew it. In this way both additional facility of manipulation and additional security from accident are attained. This alteration also creates much greater facility of attachment, according to the varying nature or position of the fixtures to which the clamp which secures the instrument when in operation is made fast. The form of the key employed in screwing and unscrewing the bottle and ivory collar has also been altered in a way which does not risk the fracture of the collar as the previous one did. It now grasps the collar externally, like that used for an air-pump or gas-holder. The new forms of screw, ivory valve-piece, and key, can easily be adapted to any instrument which has been constructed in the original manner.

It is also satisfactory to be able to state, that during this summer I have had ample experience of the instrument being in all its parts quite calculated to stand the ordinary concussions of travelling, having carried it with me in its proper case in my ordinary baggage, during a tour on the Continent, without injury to any part of it. On this occasion I have also found that it worked equally well as at home, in the various situations in which I employed it in Germany, Switzerland and France*.

It may be mentioned that the gold-beater's leaf valve is constructed by folding a piece of that material, 2 inches long by $\frac{1}{2}$ an inch wide, first longitudinally and then transversely. This fourfold portion is then placed across the valve-piece and secured in its place by two or three coils of silk thread, and the superfluous leaf and thread cut away by a sharp pen-knife. A drop of sperm oil on the point of the knife is then passed carefully under the leaf and the knife instantly withdrawn. Gentle pressure by the point of the finger causes the oil to pass over the whole surface under the leaf. The proper state of the valve is ascertained both by blowing and suction.

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* In order to meet the wishes of those who may desire to find their own bottles for æther and olive oil, Mr. Kemp of Edinburgh has fitted up all the essential parts of the instrument in a smaller and less expensive case, which he finds he can afford to sell at two-thirds of the cost of the more complete arrangement; those, however, who may wish to have everything necessary at their hand, can still obtain from him the complete collection in their proper case.

In the use of this hygrometer, the person of the experimenter can easily be kept at a sufficient distance from the condensing surface; and in the ordinary mode of operating at an open window whilst the little brass bottle is in the open air, the body of the experimenter is of course in the room. Were it thought necessary, a small telescope or opera-glass might be used to observe the surface, whilst the handle of the piston-rod might be lengthened so as to admit of still greater distance.

XXXVII. *Notices respecting New Books.*

Experimental Researches in Electricity. By MICHAEL FARADAY, *D.C.L., F.R.S. &c. &c.* 3 vols. 8vo. London: Taylor and Francis.

THREE volumes of Faraday's Experimental Researches are now before the public. They embrace his contributions to the Philosophical Transactions, the Philosophical Magazine, and other journals during a period of upwards of thirty years. The first of these volumes opens with the celebrated memoir on Voltaic and Magneto-electric Induction. In the year 1820 Arago and Humboldt were making experiments on terrestrial magnetism on the slope of Greenwich Hill: Arago observed that the magnetic needle came more quickly to a state of rest when in proximity with certain bodies, and following up this observation he founded upon it his so-called magnetism of rotation. The investigation of the force which produced the rotation of the magnetic needle occupied for years the attention of English and continental philosophers; exact experiments were made and explanations given, all of which, however, were overthrown by the subsequent experiments of Arago himself. The fact stood forth as a challenge to the natural philosophers of the day, until in the year 1831 Faraday crushed the difficulty, and embraced the discovery of Arago in that of magneto-electric induction—a discovery which rivals in importance that of the pile by Volta, and, indeed, already threatens to supersede the latter in its most important practical applications.

There are men, who, having made a great discovery—having passed the trying furnace of thought through which such a result is approached,—rest contented with their achievement, and evince no desire to renew the struggle by which their laurels were won. The great Italian whose name we have mentioned, suffers in this respect by a comparison with Faraday. The latter drew from his discovery courage and strength to go on to others, and the volumes now before us are the practical record of a life of scientific activity unexampled in result since the time of Newton. Faraday has not been disciplined in the accredited schools of science: he has figured the operations of nature in his own fashion before his mental eye, and the very richness of his imagination in this respect often places his readers in difficulty. We question much whether those who dwell most upon Faraday's simplicity of expression are really aware of the effort sometimes required to understand him. Such a man is not to be skimmed over superficially; he cannot make his thoughts an alphabet for children, and those who would accompany him to the depths from which he speaks must be prepared to encounter difficulties in the way. Often indeed he speaks in a language not generally known even among scientific men. As Kant in the 'Critic of Pure Reason' was almost necessitated to elaborate a new language for himself, so the incessant contact with new truths which it has been Faraday's fortune to experience has rendered a divergence from the accredited forms of expression necessary to him. He has seen theories fade before the stern realities which he has brought to light, and it is therefore impossible for him to make use of the language of those theories with the undoubting confidence of those who believe in them. An apparent vagueness may sometimes be the result of this scepticism, but this is inevitable with the man, who, working on the boundaries of science, discerns the inadequacies of received notions, and has to cross a chasm of doubt and darkness in his transition from the defective to the true.

The salient qualities of Faraday's scientific character manifest themselves from the beginning to the end of these volumes,—a union of ardour and patience, the former prompting the attack, the latter holding him to his subject until he has obtained complete possession of it. Every experiment is a wedge which opens a way before it to other results. He places his subject in all imaginable positions, looks round it, and observes all its outlying relations. Give him hold of a fact and the probability is that he will draw a hundred after it, and so master its dependencies as to leave his followers nothing to find. How strikingly is he in this respect contrasted with Oersted, who, having made a great discovery, seemed to have a wall of adamant built round his intellect to prevent him from pursuing it to its consequences. Fructified by this man's intellect, each fact becomes a seedling which ever sprouts and blossoms into new results.

The memoir to which we have referred as commencing the first volume is followed by others of great importance;—on the Identity of Electricity, &c.,—on the Laws of Electric Conduction, wherein the remarkable influence of fusion upon conducting power is made manifest,—on Electro-Chemical Decomposition,—on the Power of Platinum to induce Combination,—on the Definite Nature and Extent of the Chemical Forces—a memoir of incalculable value, which excited the thoughts and admiration of even metaphysicians at the time of its publication, and at the present moment stirs profoundly the minds of scientific men. Faraday has introduced many new terms into science, and the utility of some may not be manifest to superficial thought. But scientific terms, arising as they often do out of theoretic contemplations, often introduce something more than, or different from, the fact into the mind, and hence the possible injury arising from their use. Faraday's constant effort has been to displace those terms which carry with them foregone conclusions by others of a character so neutral that the thought shall be uninfluenced by the term. Passing over many minor papers we arrive at the important memoirs on Static Induction, on Conduction, and on Induction in Curved Lines. The latter idea reveals perhaps as clearly as any other the profound and subtle character of Faraday's mind. The question he proposes to himself is: "How is the electric power propagated? Does it act like the force of gravity at a distance without the help of intervening matter, or does it act in virtue of such intervention?" He reflects thus: If it be a force acting at a distance, this force will act in the straight line which unites each pair of active points, but if it be a force which propagates itself by the agency of material particles, there is every reason to suppose that it may act in curved lines, as sound, for example, which depends on the atmosphere for its propagation, can turn round a corner, or waves of water round a rock. Reasoning in the inverse way, if it can be established that the electric force propagates itself in curved lines, it may certainly be inferred from this that it requires a material medium for its propagation. Faraday believes that his experiments establish the latter point; others doubt it; but however this may be, the profound and philosophic nature of the thought which guided him is manifest. We may not be able to make Faraday's mode of symbolizing the operations of nature our own, but when we reflect upon the success which has attended his efforts, on the fact that his peculiar symbols enabled him to foresee results of which no other electrician dreamed, and which after a lapse of twenty years accident has brought to light, we are warned against being dogmatic on theoretic matters, and learn the salutary lesson, that the competent and earnest searcher after truth has other avenues than our licensed scientific turnpikes into nature.

The second volume of the Experimental Researches contains valuable papers on the Electricity of the Gymnotus, on the Source of Power in the Voltaic Pile, on the Electricity evolved by the Friction of Water and Steam, in which the principles of the hydro-electric machine of Armstrong are fully developed, a paper on Magnetic Rotations, dating as far back as 1821, some speculative observations, and the writer's letters in the only controversy which has ever disturbed his peaceful life. The memoir on the Source of Power in the Voltaic Pile is of peculiar importance. The arguments which it contains appear to us to overthrow wholly the hypothesis of contact as maintained by Volta. The doctrine of the conservation of force has received formal expression at the hands of many recent investigators, but the idea appears to have been a guiding light to Faraday throughout his entire scientific career, and the explicit statement of this principle at page 103 of the present volume places a difficulty in the way of the theory of contact which has never yet been overcome.

The third volume opens with an account of one of the most remarkable discoveries of modern times, entitled by the author, "the Magnetization of Light and the Illumination of Magnetic Lines of Force," and expressed in the theoretic language usual in our day, "the Rotation of the Plane of Polarization of a Luminous Ray. Up to the present time the value of this discovery has been principally suggestive; no new fact has been added to the subject since it left the hands of Faraday. A connexion between light and magnetism is here indicated which none of our theories would have enabled us to predict. None of our hypotheses would have prompted us to seek this result; they would rather have dissuaded us from the attempt, but this man sees the generally fleeting nature of those images by which the operations of nature are rendered tangible to the human mind, and is not to be warped from his path by a symbol. The method of true scientific investigation is probably incommunicable; it belongs to the individual rather than to the system, and our contemporaries, we think, miss their mark when they point at Faraday's researches as simply illustrative of the power of inductive philosophy. Faraday's researches are illustrative of the power of a strong and independent soul, expressing itself after its own methods, and acknowledging no mediator between it and Nature. You may fill your brain with inductive philosophy, but without the God-given force which this man possesses, and which signalizes him from all others at the present day, you will never accomplish what he has accomplished. Hotly following the discovery of the rotation of the plane of polarization is that of diamagnetism. At the end of the last century Bruggmanns had actually shown that a magnetic needle was repelled by a mass of bismuth: here he stopped. Le Bailliff repeated the experiment with bismuth and antimony: here *he* stopped. Seebeck and others have touched upon the same subject; the cherry struck against their lips, but they failed to take it in. These fragmentary glances excited a momentary curiosity and were almost forgotten, until Faraday, without knowing anything of the experiments alluded to, rediscovered the same facts, but instead of stopping, he made them the inlet to a new and vast scientific domain, in which philosophers of all countries now find occupation. The value of Faraday's discoveries consists in a great degree in the amount of intellectual power which they call into action. It has been his good fortune to alight, not upon the isolated nuggets of science, which, while enriching himself might have left others without a share, but to strike the golden lodes at which the highest scientific spirits of the present age have worked, and at which thousands yet to come will work with incalculable benefit to mankind.

The present volume contains numerous offshoots from the great discovery just alluded to,—the author's experiments and speculations on Magne-crystallic Action,—the Magnetism of the Atmosphere, and its possible Influence upon the Diurnal Variation of the Magnetic Needle,—on the Nature of the Lines of Magnetic Force,—on Electric Induction in Subterranean Telegraph Wires, and papers on other kindred subjects. The mention of the telegraph reminds us of the application of the author's own discovery to telegraphic purposes, whereby the grave difficulties presented to the transmission of ordinary voltaic electricity through subterranean wires are practically overcome. The public never hear Faraday's name mentioned in connexion with the electric telegraph—scarcely that of Volta; the public contemplate the flower, but see not the roots from which its petals are fed; the men who have given the telegraph its present practical form are deserving of public esteem, and have obtained both it and more substantial marks of public appreciation; and yet the merits of Faraday, in comparison with such men, are about in the ratio of Prometheus to Dr. Kahn. Down deep in the mysteries of Nature, he and his great *confrère* Volta found the forces which pulse through the telegraphic wire. Honour to those who by their mechanical skill have brought these great discoveries home to human hearths, and made them the ministers of society; but a higher and nobler meed be his who brought us the fire from heaven which imparts life and activity to the telegraphic mechanism. This is the position of Faraday, this the position of Volta, although neither of them even derive the poor breath of popular applause in return for their transcendent services.

This leads us into sober reflections. The public never can know and appreciate the national value of such a man as Faraday. He does not work to please the public, nor to win its guineas; and the said public, if asked its opinion as to the practical value of his researches, can see no possible practical issue there. The public does not know that we need prophets more than mechanics in science,—inspired men, who, by patient self-denial and the exercise of the high intellectual gifts of the Creator, bring us intelligence of His doings in Nature. To them their pursuits are good in themselves. Their chief reward is the delight of being admitted into communion with Nature, the pleasure of tracing out and proclaiming her laws, wholly forgetful whether those laws will ever augment our banker's account or improve our knowledge of cookery. Such men, though not honoured by the title of 'practical,' are they which make practical men possible. They bring us the tamed forces of Nature, and leave it to others to contrive the machinery to which they may be yoked. The labour of the prophet, as we have called him, is unpurchaseable. Nevertheless he is a man, has the needs of a man, and, as age advances, feels the infirmities of a man. Has such a man no claim upon his country? We know not what provision has been made for him whose labours are now before us: we only know his services to science and to the public. If we are rightly informed, it was Faradaic electricity which shot the glad tidings of the fall of Sebastopol from Balaklava to Varna. Had this man converted his talent to commercial purposes, as so many do, we should not like to set a limit to his professional income. As before stated, we do not know the equivalent provided by Government for such sacrifices and such services as his,—where he is to rest his noble head when years have showered their snows upon it. The quality of his services cannot be expressed by pounds, but that brave body which for forty years has been the instrument of that great soul, is a fit object for a nation's care, as the achievements of the man are, or will one day be, the object of a nation's pride and gratitude.

XXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 226.]

June 14, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"Remarks on the Rev. H. Moseley's Theory of the Descent of Glaciers." By James D. Forbes, D.C.L., F.R.S.

In a paper "On the Descent of Glaciers," communicated to the Royal Society on the 19th of April, 1855, and printed in their Proceedings, the Rev. Henry Moseley has proposed an explanation of that phenomenon.

The first part of his paper contains a lucid description of the gradual motion of a sheet of lead covering the roof of Bristol Cathedral, which he ascribes (I have no doubt justly) to the successive expansions and contractions of the lead by atmospheric temperature. He explains the influence of the slope of the roof and of the measure of friction upon the motion with his customary precision and clearness. He also finds for the probable measure of the effect or creeping motion of the lead, a quantity which, considering the imperfect nature of the data with regard to temperature, agrees sufficiently well with observation.

In the latter and shorter part of the paper is a transition to the case of glaciers, whose motion over their beds may, he thinks, be accounted for in the same way, namely, by the alternate contraction and expansion of the ice by diurnal changes of temperature, and he then enters into certain calculations founded principally on data contained in my 'Travels in the Alps of Savoy' in confirmation of this view.

Entertaining as I do the highest respect for Mr. Moseley's eminent attainments as a theoretical mechanician, it is with extreme regret that I find it necessary, in maintenance of the views regarding glacier motion which I have elsewhere advanced, and in the interest of scientific truth, to show (as I believe I can) that Mr. Moseley has been led, apparently by a sudden inadvertency, to uphold an opinion completely indefensible.

I must first object to Mr. Moseley's description or definition of a glacier, as calculated to mislead the inquirer: he says (p. 339), "glaciers are, on an increased scale" [compared to the sheet lead covering of a roof], "sheets of ice placed upon the slopes of mountains." There are certainly some inconsiderable glaciers of the second order to which this description might possibly apply, with the exception of the small thickness inferred by the word "sheet;" but the true glaciers, whose theory has been so often discussed (which theory must evidently likewise include that of glaciers of the second order), cannot fairly be called either *sheets* of ice or be accurately described as lying *on* the slopes of mountains. They are vast icy accumulations whose depth bears a considerable proportion to their breadth, and which fill mountain ravines or valleys, Glaciers are very generally hemmed in by precipitous rocks which determine their *contour* or ground plan; they have often to make their way through contracted gorges where the ice occupies (as in the case of the Mer de Glace of Chamouni), within a short distance, a channel but half as wide as it did before. Yet the glacier, preserving its continuity as a whole, expands or contracts in conformity with the irregularities, not only of its lateral walls, but of its bed, forcing itself over obstacles, or even occasionally allowing itself to be cleft into two branches by them, and closing again into a united mass after the insular obstruction has been past. To speak of such resistances of the channel to the progress of the ice as mere *friction*, or of a glacier considered as a solid body and in its whole extent (or in any considerable part of it) as having an *angle of repose*, as in the case of a substance with a flat base resting on an inclined plane, is evidently inadmissible and tends to mislead. The valley of the Mer de Glace might have almost any possible inclination before the ice would tend to slide out of it *en masse*, for it is moulded to every sinuosity or protuberance of the bed, whether vertical or horizontal. Let Mr. Moseley imagine a sheet of lead having the ground plan of the *Mer de Glace* and confined by margins of wood accurately adapted to it, and he will see that unless lead were so ductile as to be entitled to the appellation of a semifluid, no motion could possibly result, however great might be the slope on which it lay.

I am sorry to find that Mr. Moseley denies entirely (p. 341) the viscous or plastic structure of a glacier as "not consistent with the fact that no viscosity can be traced in its parts when separated." The answer to this objection seems to be merely this; that the viscosity, though it cannot be "traced" in the parts, *if very minute*, nevertheless *exists* there, as unequivocally proved by experiment on the large scale, or even on spaces several yards or fathoms in extent*. The plastic condition of a glacier is, as I have repeatedly stated, no longer an hypothesis, but a *fact*, since I have in many places demonstrated that, account for it as we may, different portions of the same continuous mass of ice are moving at the same moment with different velocities. That a small piece of ice is not sensibly plastic, is not more strange than that the fine blue colour so perceptible in the glacier totally vanishes in its constituent fragments. That *ductility* and *fragility* are not incompatible qualities, is shown by the fact, that sealing-wax at moderate atmospheric temperatures will mould itself (*with time*) to the most delicate inequalities of the surface on which it rests, under a pressure of not more than half an inch of its substance, but may at the same time be shivered to atoms by a blow with a hammer.

The question of plasticity, however, affects only *mediately* Mr. Moseley's theory of the primary cause of motion by dilatation and contraction. According to the views I support, the dilatation and contraction of the ice of glaciers (assuming it to exist) would be inefficient to move the mass unless it moved plastically; and if it moves plastically, the supposition of its thermal expansion is, at all events, superfluous, since gravity is in that case a sufficient moving force.

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* See Phil. Trans. 1846, p. 162, and Phil. Magazine (1845), xxvi. p. 414.

But, it will be argued, if the ice be really acted on by heat and cold as Mr. Moseley supposes, it is a *vera causa* of motion and cannot be neglected. And here we join issue respecting the physical theory proposed.

Mr. Moseley's explanation of the descent of the lead on a roof at an angle much below that at which motion could take place by gravity, friction being allowed for (the *angle of repose*), amounts to this, that every increase of temperature of the mass by the heat of the day expanding it, pushes the lower end downwards *more* than it pushes the upper end upwards; whilst the cold of the night retracts a little the lower end, but (being favoured by the slope) it pulls down the upper end more than it had been pushed up during the heat of the day, and thus by a species of vermicular motion impels the body down the inclined plane. The motion is calculated from a formula including the absolute expansibility of lead, the slope of the roof, the angle of repose, and the *diurnal range* of temperature. Taking then corresponding data for the Mer de Glace of Chamouni, assuming 30° to be the angle of repose of a glacier upon its bed, taking the expansion of ice to be nearly double that of lead (according to experiments made at St. Petersburgh), and the daily range of temperature *of the ice* to be the same as that *of the air* observed by De Saussure on the Col du Géant in the month of July, Mr. Moseley calculates the daily descent of the glacier opposite the Montanvert and compares it with my observations.

Waiving for the moment all other objections, *can we possibly attribute to the ice of the entire mass of this vast glacier an average daily range of temperature of 4¼° of Reaumur or 9½° of Fahrenheit?* The idea seems to me to be perfectly untenable.

The expansion and contraction of ice by heat and cold can of course only take place *below* the freezing-point, or 32°. Let it be percolated by water as it may, it cannot rise *above* that temperature nor expand in the smallest degree. But it is a matter perfectly notorious, that, at least in summer, and throughout the whole extent of the Glacier Proper, and even far into the region of the *névé*, the glacier is charged with percolating water derived from superficial fusion. Mr. Moseley admits this, and even attributes the diurnal oscillation of temperature which he assumes, to the action of water, as in the following passage: "Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses by variations in the quantity and the temperature of the water, which flowing from the surface everywhere percolates them" (p. 339). This action therefore clearly brings the temperature of the ice up to 32° during the day. But how is the cold of the night to operate in reducing the temperature of a mass of ice certainly from 300 to 600 or more feet in thickness through the enormous average depression of 9½ degrees? The water so efficient by its percolation in *raising* the temperature (if necessary) to 32°, being frozen, is now powerless. Cold can be conveyed downwards, or to speak more correctly, heat can be transmitted upwards through the ice only by the slow process of conduction, and this on the supposition that the depression of superficial temperature is all that the theory might require. But how stands the fact? Mr. Moseley quotes from De Saussure the following *daily ranges* of the temperature of the air in the month of July at the Col du Géant and at Chamouni, between which points the glacier lies.

At the Col du Géant	4°·257 Reaumur.
At Chamouni	10°·092 Reaumur.

And he assumes "the same mean daily variation of temperature to obtain throughout the length" [and depth?] "of the Glacier du Géant which De Saussure observed in July at the Col du Géant." But between what limits does the temperature of the air oscillate? We find, by referring to the third volume of De Saussure's Travels, that the mean temperature of the coldest hour* (4 A.M.) during his stay at the Col du Géant was 0°·457 Reaumur, or 33°·03 Fahrenheit, and of the warmest (2 P.M.) 4°·714 Reaumur, or 42°·61 Fahrenheit†. So that even upon that exposed ridge, between 2000 and 3000 feet above where the glacier can be properly said to commence, the air does not, on an average of the month of July, reach the freezing-point at any hour of the night. Consequently the *range of temperature attributed to the glacier is between limits absolutely incapable of effecting the expansion of the ice in the smallest degree.* This would of course be still more applicable if we take the mean of the temperatures at Chamouni and the Col du Géant to present the general atmospheric conditions to which the glacier is exposed.

It is in summer that the glacier moves fastest: it is with my observations of motion *in July* that Mr. Moseley compares the results of his theory: and therefore it is of no avail to say that there are periods of the year when congelation penetrates at night some inches, or even it may be some feet into the ice, and when therefore the sensible heat of the glacier may be considered to vary, though, if regard be had to its vast thickness, it must be on an average and in the most extreme circumstances to an absolutely inappreciable degree.

Lastly, Mr. Moseley, whilst condemning in the following passage the theory of glacier motion by the dilatation of water in the interstices of the ice, clearly passes sentence on his own, which could not come into action until the other had already produced its effects: "The theory of Charpentier, which attributes the descent of the glacier to the daily congelation of the water which percolates it, and the expansion of its mass consequent thereon, whilst it assigns a cause which, so far as it operates, cannot, as I have shown, but cause a glacier to descend, appears to me to assign one inadequate to the result; for the congelation of the water which percolates the glacier does not, according to the observations of Professor Forbes, take place at all in summer more than a few inches from the surface. Nevertheless it is in summer that the daily motion of the glacier is greatest." (Moseley, Proc. R.S. vol. vii. p. 341.)

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* The observations were made every two hours day and night.

† The corresponding extremes at Chamouni are 53°·25 and 75°·96 Fahr.

"Researches on the Foraminifera.—Part I. General Introduction, and Monograph of the Genus *Orbitolites*." By William B. Carpenter, M.D., F.R.S., F.G.S. &c.

The group of *Foraminifera* being one as to the structure and physiology of which our knowledge is confessedly very imperfect, and for the natural classification of which there is consequently no safe basis, the author has undertaken a careful study of some of its chief typical forms, in order to elucidate (so far as may be possible) their history as living beings, and to determine the value of the characters which they present to the systematist. In the present memoir, he details the structure of one of the lowest of these types, *Orbitolites*, with great minuteness; his object having been, not merely to present the *results* of his investigations, but also to exhibit the *method* by which they have been attained; that method essentially consisting in the minute examination and comparison of a *large number* of specimens.

The *Orbitolite* has been chiefly known, until recently, through the abundance of its *fossil* remains in the Eocene beds of the Paris basin; but the author, having been fortunate enough to obtain an extensive series of *recent* specimens, chiefly from the coast of Australia, has applied himself rather to these as his sources of information; especially as the *animals* of some of them have been sufficiently well preserved by immersion in spirits, to permit their characters to be well made out.

As might have been anticipated from our knowledge of their congeners, these animals belong to the *Rhizopodous* type; the soft body consisting of *sarcode*, without digestive cavity or organs of any kind; and being made up of a number of segments, equal and similar to each other, which are arranged in concentric zones round a central nucleus. This body is invested by a calcareous shell, in the substance of which no minute structure can be discerned, but which has the form of a circular disk, marked on the surface by concentric zones of closed cells, and having minute pores at the margin. Starting from the central nucleus,—which consists of a pear-shaped mass of sarcode, nearly surrounded by a larger mass connected with it by a peduncle,—the development of the Orbitolite may take place either upon a *simple*, or upon a *complex* type. In the former (which is indicated by the *circular* or *oval* form of the cells which show themselves at the surfaces of the disk, and by the *singleness* of the row of marginal pores), each zone consists of but a single layer of segments, connected together by a single annular stolon of sarcode; and the nucleus is connected with the first zone, and each zone with that which surrounds it, by radiating peduncles proceeding from this annulus, which, when issuing from the peripheral zone, will pass outwards through the marginal pores, probably in the form of *pseudopodia*. In the *complex* type, on the other hand (which is indicated by the *narrow* and *straight-sided* form of the superficial cells, and by the *multiplication* of the horizontal rows of marginal pores), the segments of the concentric zones are elongated into vertical columns with imperfect constrictions at intervals; instead of a single annular stolon, there are two, one at either end of these columns, between which, moreover, there are usually other lateral communications; whilst the radiating peduncles, which connect one zone with another, are also multiplied, so as to lie in several planes. Moreover, between each annular stolon and the neighbouring surface of the disk, there is a layer of superficial segments, distinct from the vertical columns, but connected with the annular stolons; these occupy the narrow elongated cells just mentioned, which constitute two *superficial* layers in the disks of this type, between which is the *intermediate* layer occupied by the columnar segments.

These two types seem to be so completely dissimilar, that they could scarcely have been supposed to belong to the same species; but the examination of a large number of specimens shows, that although one is often developed to a considerable size upon the simple type, whilst another commences even from the centre upon the complex type, yet that many individuals which begin life, and form an indefinite number of annuli, upon the simple type, then take on the more complex mode of development.

The author then points out what may be gathered from observation and from deduction respecting the *Nutrition* and mode of *Growth* of these creatures. He shows that the former is probably accomplished, as in other Rhizopods, by the entanglement and drawing in of minute vegetable particles, through the instrumentality of the pseudopodia; and that the addition of new zones probably takes place by the extension of the sarcode through the marginal pores, so as to form a complete annulus, thickened at intervals into segments, and narrowed between these into connecting stolons, the shell being probably produced by the calcification of their outer portions. And this view he supports by the results of the examination of a number of specimens, in which *reparation of injuries* has taken place. Regarding the *Reproduction* of Orbitolites, he is only able to suggest that certain minute spherical masses of sarcode, with which some of the cells are filled, may be *gemmules*; and that other bodies, enclosed in firm envelopes, which he has more rarely met with, but which seem to break their way out of the superficial cells, may be *ova*. But on this part of the inquiry, nothing save observation of the animals in their living state can give satisfactory results.

The regular type of structure just described is subject to numerous *variations*, into a minute description of which the author next enters; the general results being, that neither the shape nor dimensions of the entire disk, the size of the nucleus or of the cells forming the concentric zones, the surface-markings indicating the shape of the superficial cells, nor the early mode of growth (which, though typically *cyclical*, sometimes approximates to a *spiral*), can serve as distinctive characters of *species*; since, whilst they are all found to present most remarkable differences, these differences, being strictly gradational, can only be considered as distinguishing *individuals.* It thus follows that a very wide *range of variation* exists in this type; so that numerous forms which would be unhesitatingly accounted specifically different, if only *the most divergent examples* were brought into comparison, are found, by the discovery of those *intermediate links* which a large collection can alone supply, to belong to one and the same specific type.

After noticing some curious *monstrosities*, resulting from an unusual outgrowth of the central nucleus, the author proceeds to inquire into the *essential character* of the Orbitolite, and its relations to other types of structure. He places it among the very lowest forms of Foraminifera; and considers that it approximates closely to sponges, some of which have skeletons not very unlike the calcareous net-work which intervenes between its fleshy segments. Of the *species* which the genus has been reputed to include, he states that a large proportion really belong to the genus *Orbitoides*, whilst others are but varieties of the ordinary type. This last is the light in which he would regard the *Orbitolites complanata* of the Paris basin; which differs from the fully-developed Orbitolite of the Australian coast in some very peculiar features (marking a less complete evolution), which are occasionally met with among recent forms, and which are sometimes distinctly transitional towards the perfect type.

The author concludes by calling attention to some general principles, which arise out of the present inquiry, but which are applicable to all departments of Natural History, regarding the *kind* and *extent* of comparison on which alone specific distinctions can be securely based.

June 21.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On the Magnetism of Iron Ships, and its accordance with Theory, as determined externally, in recent Experiments." By the Rev. W. Scoresby, D.D., F.R.S.

The magnetic condition of iron ships is a subject of so much importance, practically and scientifically, that I have been induced to submit to the Society a few characteristic facts (hastily indeed brought together) derived from recent experiments.

In a work in the Society's library, entitled 'Magnetical Investigations,' it was shown, by deductions from an elaborate series of experiments on plates and bars of malleable iron, that the magnetic condition of iron ships should, theoretically, be conformable to the direction of terrestrial induction whilst on the stocks; and the *retentive* quality, which is so highly developed by virtue of the hammering and other mechanical action during the building, should be so far fixed in the same direction, as to remain after the ships might be launched, until disturbed by fresh mechanical action in new positions of their head or keel. In this view, taking, for instance, the condition of the middle, or the main-breadth section of a ship on the stocks, the magnetic polar axis should assume the direction of the dipping-needle (with an equatorial plane, or plane of no-attraction at right angles to it), passing through or proximate to the centre of gravity of the iron material in such section. Thus every ship should have a characteristic magnetic distribution, primarily, dependent on her position whilst on the stocks; so that, being built with the head north or south, the *equatorial plane* should appear externally on the same horizontal level, the *polar axis* only being inclined from the vertical, in correspondence with the direction of the axis of terrestrial magnetism (Magnetical Investigations, vol. ii. pp. 331, 332).

It was also inferred, that whilst such individuality of the magnetic distribution would be rendered *retentive* on the same principles as this quality of magnetism is developed in bars or plates of iron by mechanical action, so the axial direction of the ship's magnetism would be *liable to change*, under mechanical action, in new positions of the ship's head, or under new relations of terrestrial magnetism, just as the retentive magnetism is liable to change in bars or plates of iron if hammered, vibrated, or bent whilst held in new positions.

And it was further inferred, that the analogy with plates and bars might be expected to hold, notwithstanding the numerous pieces of which the ship's hull might be composed, because, in experiments on combining short magnets into long series, or submitting piles of short bars of iron to terrestrial induction, it was found that no material difference in the effects occurred betwixt a *single* steel magnet of a given length, or a *single* bar of iron, and the same substance and dimensions combined in short or small pieces in contact. Hence it was considered that an iron ship should exhibit in its general fabric the characteristic phenomena of one undivided mass, or a unity as a magnetic body.

These anticipations, it will be seen (published betwixt three and four years ago), have, so far as we have now time to elucidate them, had verifications, in actual experiments on iron ships, as beautiful as they are conclusive.

In the case of the 'Elba,' an iron ship of 200 feet in length, built recently on the Tyne, the magnetic condition before launching, which I was invited to investigate by Mr. Robert Newall, the owner, was found satisfactorily accordant with theory. Her head pointed south a few degrees westerly, and her lines of no-deviation (indicative of the position of the magnetic equatorial plane) were at a small distance in elevation different on the two sides, that of the starboard side being the highest. Proceeding in a direction at right angles to the dip, and passing through, or near to, the ship's general centre of gravity, the lines of no-attraction (descending forward) came out near the junction of the stem with the keel. And *there*, it was remarkable, as I had suggested as probable to Mr. James Napier of Glasgow, before making any experiment, there was found a departure from the ordinary regularity of the lines of no-attraction in a considerable downward bend.

Towards the stern, the equatorial lines rose out of and came above the iron plating of the *top-sides*, about 40 feet from the tafrail; thus giving to the *after-part* of the ship a uniform northern polarity. The ship, consequently, had become a huge simple magnet—the north pole at the stern and the south at the head. The attractive power, as was expected, was highly energetic. At the distance of 50 feet, a compass on the level of the keel, at right angles to it abreast of the stern, was deviated to an extent of above 10°; at 100 feet distance the ship's magnetism caused a deflection of about half a point; and at 150 and even 200 feet there was a very sensible disturbance!

In the case of the 'Fiery Cross,' built at Glasgow and launched in January last (a case which I have elsewhere referred to), the lines of no-deviation, as taken for me by Mr. James Napier, were still more rigidly in accordance with theory,—the difference of elevation of the observed lines of no-deviation at the main-breadth section agreeing with calculation, theoretically, to within an inch or two. In the other case, that of the 'Elba,' a slight discrepancy as to the comparative level of the lines of no-attraction on the two sides, might, perhaps, be satisfactorily explained by the proximity and somewhat disturbing influence of another iron ship (advanced only to the frames or angle irons) on the port side of her.

In the case of the 'Elizabeth Harrison,' a large iron ship built at Liverpool, the first I had carefully examined, the correspondence of the magnetic polar axis and equatorial plane with those of terrestrial action was equally characteristic and conclusive.

Hence we may perceive a sufficient reason for some of the peculiar phenomena in iron ships of the compass disturbances and their changes. We may see why a ship built with her head easterly or westerly, and having the polar axis inclined over 18° or 20° to the starboard or port side, should be *particularly liable to compass changes*, if severely strained or struck by the sea with her head in an opposite direction. We see why the compasses of the 'Tayleur' should have been exposed to such a change as appears to have taken place in her lamentable case. We see in the case of the 'Ottawa*' (one I have elsewhere referred to), why a heavy blow of the sea, with the ship heeling and her head pointing eastward, would be likely to produce a change in her magnetism, when her previous magnetic distribution was solicited by terrestrial action in an angle of 30° or 40° of difference. And we see why the *deviations* of the compass in iron ships should, differently from those of wooden ships, be sometimes *westerly* and sometimes *easterly* in ships built and trading in home latitudes; for here, whilst in wooden ships, where the iron work is in detached masses, the ship can have but little, externally, of the character of a true magnet, and can possess but small comparative differences from the position of her head whilst building; in iron ships, on the converse, where the ship is rendered by percussive action a powerful and, *retentively*, true magnet, her deviating action must be expected to be different, as the polarity of the head or stern may differ in denomination, or as the ship's magnetic polar axis may happen to lie over to starboard or port.

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* Where the compass suddenly changed two points.

As an objection might be made to deductions from experiments on simple individual bars or plates of iron being applied to the case of iron ships built up of thousands of pieces, I have repeated the experiments, substituting for an entire plate or bar of iron a plate about 18 inches long and 3 broad, made up of numerous separate plates, and combined in the manner of the plating of iron ships. The compound or combined plate of some eighteen or twenty pieces yielded, under percussion, vibration or bending, results precisely similar to those obtained by the use of single plates or bars.

XXXIX. *Intelligence and Miscellaneous Articles.*

EXAMINATION OF THE GREEN MATTER CONTAINED IN GREEN INFUSORIA. BY THE PRINCE OF SALM-HORSTMAR.

If the green Infusoria of large ponds be collected upon a filter, then extracted with alcohol whilst fresh, and the green extract evaporated to dryness at a temperature of about 104° F., the body obtained has the following properties:—

1. When slowly heated upon platinum, it does not fuse, but soon evolves vapours (not smoke) which possess the peculiar odour of ponds, whilst the greater part of the mass is volatilized. The production of vapour then ceases, and a small brownish residue is left, which, however, is also slowly volatilized at a temperature below that at which the platinum becomes red-hot. No carbonaceous residue is left.
2. It dissolves readily both in alcohol and acetic æther.
3. It dissolves in hot water with a yellowish-olive colour; the water first becomes milky.
4. It dissolves in ammonia with a yellow colour.
5. It is soluble with a greenish-yellow colour in solution of potash, and the solution shows a black stripe in the red of the spectrum.
6. The alcoholic solution has no reaction upon litmus.

It is consequently neither a wax-like body nor chlorophyll; for chlorophyll extracted by alcohol from freshly-dried leaves of *Lolium perenne* and evaporated at 104° F. until no more fluid is present, forms a sticky mass which does not dissolve in water, ammonia, or solution of potash. Its behaviour when heated in a platinum crucible is the same as that of the green of the Infusoria.

The two alcoholic solutions are very different in colour when freshly prepared; that of chlorophyll from grass is emerald-green, that from the Infusoria olive or bottle-green.

Angström's exact optical investigations of the two substances had also presupposed their distinctness.—Poggendorff's *Annalen*, vol. xciv. p. 466.

ON THE RED DISPERSED LIGHT IN A SOLUTION OF CHLOROPHYLL. BY THE PRINCE OF SALM-HORSTMAR.

From the statements of Stokes it appears as though the red dispersion was only to be seen in directions perpendicular to the incident ray; this, however, applies only partially to the phænomenon. I have observed that the red light can be distinctly seen, even when the test-glass containing the solution of chlorophyll is held so that the flame of a lamp is placed nearly between the glass and the eye of the observer. In order to avoid mistake, I put a piece of black velvet into the fluid so as to prevent the hinder surface of the glass from giving any reflexion; but the red dispersion remained as before. The red light consequently radiates also in the opposite direction to that of incidence.

It is remarkable, however, that this fluid should also show the red light in the opposite direction; that is to say, in the direction of the incident ray, as soon as the solution is so concentrated that the directly visible light becomes more obscured.

As the so-called dispersed light promises to do much to elucidate the phænomena of light, the smallest circumstance deserves to be kept in mind.

From the well-known fact that the red dispersion is only seen with the help of a burning-glass in the centre of the solution of chlorophyll, it would seem that those invisible rays of sun-light which are rendered visible by the chlorophyll are separated in the sun-light by heterogeneous spaces, just as though they passed through a lattice, which only causes those atoms lying close to the surface to become luminous, where it is permitted by the lattice, and complete their course at that point. Thus in the parallel state the red dispersion is visible at the surface; in the converging state, also, in the centre. The question then arises, whether this might not be employed in measuring these parallel heterogeneous intervals?—Poggendorff's *Annalen*, vol. xciv. p. 467.

MOLECULAR CONSTITUTION OF CRYSTALS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Dublin, August 4, 1855.

IN my paper on the Molecular Constitution of Crystals, which appeared in the Philosophical Magazine for August, I represented Sir David Brewster as having said that the cleavage of crystals arises from the unequal cohesion of the different poles of each molecule.

Quoting his words from memory, I erroneously considered them to apply to the first system of crystals of which I was then speaking;

I have fortunately since discovered my error, and hasten to make the only reparation in my power—that of acknowledging it.

I can assure Sir David Brewster that there is nothing I regret more than having made, even inadvertently, any misrepresentation, particularly of one whom I must ever look on as one of the fathers of science, and whose opinions, even though they differ from my own, I will ever regard with respect.

Your obedient Servant,

ROBERT T. FORSTER.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1855.

Chiswick.—August 1. Very fine. 2. Very fine: heavy rain. 3—5. Very fine. 6. Overcast. 7. Very fine. 8. Rain: thunder: heavy rain. 9. Cloudy and fine: clear at night. 10. Foggy, with heavy dew: very fine: slight haze: very fine. 11. Slight haze: very fine. 12. Overcast: fine: clear. 13. Very fine. 14. Cloudy and fine. 15, 16. Very fine. 17. Slight fog: very fine. 18. Very fine. 19. Very fine: slight rain at night. 20. Slight rain: cloudy and boisterous. 21. Fine: clear and windy. 22. Very fine. 23. Very fine: much sheet and forked lightning at night. 24—26. Very fine. 27. Very fine: cloudy at night. 28, 29. Very fine. 30. Very fine: clear at night. 31. Very fine.

Mean temperature of the month	61°·63
Mean temperature of Aug. 1854	60 ·55
Mean temperature of Aug. for the last twenty-nine years	61 ·98
Average amount of rain in Aug.	2·446 inches.

Boston.—Aug. 1. Fine: rain A.M. 2—4. Fine. 5, 6. Cloudy. 7. Cloudy: rain A.M. and P.M., with thunder. 8. Cloudy: rain A.M and P.M. 9. Rain A.M. 10. Fine. 11. Cloudy. 12. Cloudy: rain A.M. 13. Fine. 14. Cloudy. 15—18. Fine. 19. Fine: rain A.M. 20. Cloudy: rain A.M. 21, 22. Cloudy. 23. Cloudy: rain A.M. and P.M. 24. Fine. 25. Fine: rain P.M. 26—28. Fine. 29. Cloudy. 30, 31. Fine.

Sandwick Manse, Orkney.—Aug. 1. Cloudy A.M.: fine, fog P.M. 2. Rain A.M.: fine, cloudy P.M. 3. Clear A.M.: fine, drops, thunder and lightning P.M. 4. Rain A.M.: drizzle, showery P.M. 5. Bright A.M.: cloudy P.M. 6. Clear A.M. and P.M. 7. Rain A.M.: showers P.M. 8. Rain A.M.: drizzle P.M. 9. Drizzle A.M.: clear, vapour P.M. 10. Hazy A.M.: damp P.M. 11. Cloudy A.M.: showers P.M. 12. Cloudy A.M.: damp P.M. 13. Showers A.M.: cloudy P.M. 14. Bright A.M.: cloudy P.M. 15. Drizzle A.M.: cloudy P.M. 16. Cloudy A.M.: rain P.M. 17. Cloudy A.M. and P.M. 18, 19. Cloudy A.M.: cloudy, thunder and lightning P.M. 20. Showers A.M.: rain P.M. 21. Bright A.M.: cloudy P.M. 22. Showers A.M. and P.M. 23. Showers, bright A.M.: clear P.M. 24. Cloudy A.M.: rain P.M. 25. Clear A.M. and P.M. 26, 27. Rain A.M.: cloudy P.M. 28. Clear A.M.: showers P.M. 29. Bright A.M.: clear P.M. 30. Drops A.M.: clear, drops P.M. 31. Cloudy A.M. and P.M.

Mean temperature of Aug. for twenty-eight previous years	54°·99
Mean temperature of this month	56 ·10
Mean temperature of Aug. 1854	55 ·06
Average quantity of rain in Aug. for fifteen previous years	2·95 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston, 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston, 8¾ a.m.	Orkney, Sandwick.		Chiswick, 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. Aug.	Max.	Min.		9¼ a.m.	8¾ p.m.	Max.	Min.		9¼ a.m.	8¾ p.m.						
1.	29·853	29·773	29·32	29·65	29·58	74	54	61·5	61	56	sw.	sw.	e.	·02	·04	....
2.	29·824	29·728	29·27	29·52	29·49	76	50	63	59	58	sw.	sw.	e.	·17	·02	·11
3.	29·780	29·639	29·23	29·43	29·44	74	52	63	65	57	sw.	sw.	ne.	....	....	·29
☾ 4.	29·713	29·664	29·08	29·48	29·69	72	53	64·5	55	50½	sw.	sw.	n.	·01	....	·64
5.	30·022	29·929	29·43	29·90	30·05	73	41	61	53½	50½	sw.	nw.	n.	....	....	·05
6.	30·062	29·866	29·58	30·06	30·01	73	55	65	54½	50	s.	nw.	e.	·11	....	....
7.	29·755	29·664	29·28	29·82	29·74	75	56	66	52½	52½	sw.	s.	ne.	·44	·04	·10
8.	29·650	29·626	29·12	29·63	29·65	72	54	63	58	57	sw.	nw.	ese.	·50	·37	·20
9.	29·918	29·828	29·30	29·71	29·85	71	38	59	56½	54	w.	wnw.	calm	....	·35	·23
10.	30·151	30·043	29·57	29·89	29·99	76	49	57	61	58½	w.	wnw.	s.	....	....	....
11.	30·201	30·145	29·68	30·02	30·07	80	56	66	65	54½	w.	wnw.	w.	....	....	·05
● 12.	30·205	30·181	29·65	30·07	30·03	70	42	65	59	55	w.	n.	sw.	·01	·20	·44
13.	30·223	30·213	29·73	29·96	29·99	72	41	60	57	55	n.	wnw.	w.	....	....	·04
14.	30·189	30·174	29·66	30·06	30·12	73	57	63	57	54	nw.	w.	nw.	....	....	·01
15.	30·231	30·224	29·70	30·01	30·10	76	47	65	58	56	n.	nw.	w.	....	....	....
16.	30·297	30·274	29·75	30·14	30·18	79	45	63	58	56	nw.	nw.	sw.	....	....	·08
17.	30·282	30·244	29·78	30·14	30·09	83	45	61	59	58	n.	wnw.	wsw.	....	....	·06
18.	30·232	29·760	29·60	29·94	29·74	79	53	65	68	60	se.	s.	sse.	....	....	....
19.	29·791	29·689	29·10	29·40	29·35	76	56	70	61½	54	w.	ssw.	wsw.	·02	·02	....
☽ 20.	29·929	29·809	29·34	29·42	29·38	66	58	65	54½	52	sw.	w.	ese.	·05	·08	·07
21.	29·878	29·817	29·23	29·38	29·54	74	42	64	54	52	w.	sw.	w.	....	....	·14
22.	29·994	29·961	29·46	29·55	29·62	76	52	63	54½	51½	w.	w.	w.	....	....	·15
23.	29·898	29·691	29·44	29·74	29·83	81	56	63	55	48	se.	s.	calm	·12	....	·11
24.	29·876	29·725	29·20	29·76	29·47	74	40	68	53	56	sw.	ssw.	se.	....	·12	....
25.	30·006	29·969	29·47	29·53	29·50	73	49	59	57	55½	sw.	ssw.	s.	....	....	·61
26.	30·047	29·775	29·52	29·50	29·68	74	39	58·5	55½	53	n.	sw.	w.	....	·06	·30
○ 27.	29·983	29·878	29·43	29·44	29·46	77	49	63	55	56½	sw.	s.	wsw.	....	....	·06
28.	29·858	29·777	29·24	29·48	29·53	81	44	64	61	55	sw.	ssw.	calm	....	....	·08
29.	30·025	29·991	29·46	29·78	29·97	75	42	63	56	52½	w.	sw.	wsw.	....	....	·13
30.	30·257	30·250	29·58	30·13	30·08	72	40	60	56	55	ne.	s.	sse.	....	....	·03
31.	30·210	30·181	29·75	29·95	30·18	78	41	59	62	53½	e.	ssw.	sw.	....	....	....
Mean.	30·011	29·919	29·45	29·757	29·787	75·00	48·26	63·0	57·80	54·41				1·45	1·30	3·98

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[FOURTH SERIES.]

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XL. *On the Passage of Electric Currents through Rarefied Air.*

By P. Riess*.

AN interesting observation has lately been made by M. Gaugain in Paris. He allowed a magneto-induction current to pass between metallic knobs enclosed in a glass receiver containing rarefied air, one of which knobs, with the exception of a very small portion, was coated with an insulating substance†. Besides the glass receiver, or electric egg, a galvanometer was introduced into the circuit, the needle of which was strongly deflected when the naked knob was negative, *i. e.* when the current passed from the covered to the naked knob, but suffered no deflection when the current was reversed. Thus to the interruption current‡ of the induction apparatus was ascribed the property of passing through the electric egg in one direction only, that is, from the coated to the naked knob, and on this account the egg was called an *electric valve.* The following investigation will show that this conclusion is unfounded, and that the name, at least in the above sense, is inappropriate.

In order to repeat Gaugain's experiment, I provided myself with a short glass cylinder closed at each end with metallic plates; the lower plate, as well as its attached rod and knob, with the exception of a small portion of the latter about the size of a pin's head, were coated with melted sealing-wax: about three-quarters of an inch above the coated knob was a similar one, naked, at the end of a rod attached to the upper plate. To generate the magneto-induction current, I used a very efficient apparatus constructed by Siemens and Halske, and formerly described by me*. The induction coil of this apparatus was 2½ inches long and 2¼ inches broad; the voltaic current in the primary coil, in which lay a bundle of loose iron wires, was closed and broken by means of the tongue invented by Wagner, and a long time ago introduced by Neef. The circuit of the induction coil was closed after introducing into it a galvanometer with an astatic system of needles, as well as the before-described glass cylinder, in which the air was rarefied to one line of mercury pressure. By means of a commutator, the coated knob in the cylinder could be made positive or negative by connecting it immediately with the one or the other pole of the induction coil. That pole of the induction coil will be called *positive* which developes iodine when both poles are connected by a strip of paper moistened with a solution of iodide of potassium, and when the primary current is broken. This pole can be easily detected by first fixing two pointed elastic strips of metal with their points opposite one another, so that the distance between the points can be changed, and then fastening a strip of thin writing-paper between the points. When the points are connected with the poles of the induction coil, and made to approach one another until a continuous series of sparks ensues between the two, the sparks will be seen to pass on one side of the paper only, provided the latter is not perceptibly displaced. It follows from a known property of positive electricity, that the point in contact with this side of the paper is connected with the positive pole of the induction coil. To set the induction apparatus in action, I used one of Grove's elements; but this current being too strong, I found it necessary to pull the iron wire half-way out of the primary coil. When the coated knob in the cylinder was connected with the positive pole of the induction coil, the needle of the galvanometer was instantly deflected towards a certain side, and remained oscillating about 30°. On the other hand, when the naked knob was positive, the needle remained for some moments motionless, and then moved to a few degrees on one or on the other side of the zero of the scale. Hence it really appeared as if the interruption current passed easily and completely from the covered to the naked knob; but not at all, or only partially and with difficulty, from the naked to the covered knob. This explanation of the experiment, however, was contradicted by the light in the cylinder, which could be seen in both cases, and, in full day with shaded cylinder, appeared to have the same form; further, in the case where the needle was deflected, the light appeared more quiet and uniform than in the other case. Afterwards I examined the luminous phænomena more minutely. Before having some experience with respect to the above phænomena in the case of the simplest electric current, such as that of a Leyden battery, it appeared to me useless to experiment further upon that most complicated of all electric currents which the induction apparatus furnishes.

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* From the Report of the Academy of Science at Berlin, June 18, 1855.

† *Comptes Rendus*, vol. xl. p. 640. Phil. Mag. vol. x. p. 207.

‡ The induced currents caused by interrupting and closing the circuit of the primary or inducing current, are for the sake of brevity called interruption current and contact current, respectively.

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* Poggendorff's *Annalen*, vol. xci. p. 290.

Experiments on the Leyden Battery.

I had the following simple and convenient apparatus prepared. A cylinder of very thick glass, $3\frac{3}{4}$ inches high and 1·46 inch in interior diameter, was ground perfectly plane at one end, and closed at the other by a brass mounting, which carried a cock and a screw for the purpose of being screwed on to the air-pump. Projecting into the glass vessel and attached to this mounting is a slight brass cylinder, in which a brass rod, carrying a knob at one end, can move up and down. The cylinder can be closed and made air-tight by placing on the top a glass plate smeared with lard. The glass plate has a small hole in its centre, and upon it an ivory rod an inch long is cemented, which carries a clamp for fixing the conducting wire. A piece of platinum wire half a millim. thick descends from the clamp through the ivory and is cemented into the glass plate, the latter and the end of the wire being ground into one plane. In the plane of the lower surface of the glass, therefore, there is a disc of platinum half a millim. in diameter, between which and the brass knob beneath it electricity can pass. After placing the knob ($\frac{3}{8}$ths of an inch in diameter) at a distance of $\frac{5}{6}$ths of an inch from the platinum surface, the air in the cylinder was rarefied to about one line of mercury pressure, and the apparatus, together with a sensitive electric thermometer, brought into the circuit of an electric battery consisting of three jars. The apparatus was so connected with the discharging wire, that during the discharge the knob in the cylinder could be made either the positive or the negative electrode. In order that the manner of charging the battery may be seen, the part of the cylinder which was in connexion with the interior of the battery is given in the following table. The words "surface +" show, therefore, that the battery was charged with positive electricity; "knob −" that it was charged with negative; with respect to the direction of the current, it will be seen that it was the same in both these cases.

The following single observations show the degree to which the thermometer was heated with different charges of the battery; the knobs of the instrument for measuring the quantity of electricity in the battery were half a line distant from one another.

Heat in the discharging circuit when interrupted by air of 1 line of mercury pressure.

Quantity of electricity.	Surface +	Knob +	Surface +	Knob +	Knob −	Surface −
4	·6	10·5	5·3	10·7	6	10·2
5	8·5	16	8·4	15	8	14·3
6	13·5	21·5	14	21	13·6	21·3
7	18·7	29·3	19·7	30	17·5	28·8
Unit of charge ...	1·10	1·87	1·11	1·85	1·07	1·78

The following is the result of all the above observations, showing the relation between the heat developed and the direction of the discharge current:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 108.

In another series of experiments, the knob in the glass cylinder was 2·7 inches distant from the platinum surface in the glass plate.

Quantity of electricity.	Surface −	Knob −	Surface −	Knob −
5	9·5	6·7	9·2	7
6	15	10·3	15	10
7	17·7	13·7	17	13
8	26·3	18·5	27	18·2
Unit of charge ...	1·18	0·84	1·16	0·83

Here is the mean result:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 140.

In order to employ stronger charges of the battery (up to a quantity of electricity equal to 16), I replaced the thin platinum wire in the thermometer by a thicker, and found the following to be the mean result of four observations:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 130.

The knob in the glass cylinder was replaced by a horizontal brass disc, $\frac{1}{12}$ths of an inch in diameter, and 1 line distant from the platinum surface in the glass plate. The results were:—

Current from the surface to the disc: heat 100.
Current from the disc to the surface: heat 122.

I removed the disc so that the brass rod alone remained in the glass cylinder; it was $2\frac{1}{12}$ inches long, and its point was $\frac{5}{6}$ths of an inch distant from the platinum surface. The following were the results:—

Current from the surface to the rod: heat 100.
Current from the rod to the surface: heat 122.

Lastly, the discharge circuit was elongated by a platinum wire, 16 feet long and 0·057 line thick; the knob was again placed in the cylinder at a distance of $\frac{4}{6}$ths of an inch from the platinum surface, and the charge of the battery was increased to a quantity of electricity equal to 16. The mean results were:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 105.

In all the numerous experiments which were instituted, and in which the discharge current of the battery passed through air rarefied to 1 or 2 lines of pressure between a small and a comparatively great, but arbitrarily-shaped metallic surface, a difference was observed in the heating effect upon the discharging wire according to the direction of the current; this difference, however, although sometimes greater and sometimes less, was always in favour of one and the same direction. From the above experiments, it will be seen that the greatest heat was produced when the direction of the current was the same as that which, in the first-mentioned experiments with a magneto-induction current, produced a very small, or no deflection of the magnetic needle. *When the discharge current of a Leyden battery passes through very rarefied air between a very small and a comparatively large metallic surface, the heat produced in the remaining part of the discharge circuit is greatest when the current passes from the large to the small surface.* The meaning of a very small surface will be hereafter explained.

This remarkable and hitherto unknown change in the action of an electric discharge is easily explained by known facts. I satisfied myself that the battery was equally well discharged whatever might have been the direction of the current. As the discharge circuit had the same constitution in both cases, the difference in the heating power of the current could only be occasioned by the method of discharge. It is well known that a change in the heating effect, occasioned by a change in the method of discharge, can be shown in the clearest possible manner. When the discharge circuit is interrupted by a thin stratum of distilled water placed between metallic plates, and when the strongest charge of the battery which will pass noiselessly through the water is employed, a delicate electric thermometer is necessary in order to detect the small amount of heat which is thereby produced in the discharge wire. If the plates immersed in water, however, are brought inappreciably nearer to one another, the discharge passes through with a spark, and at the same time the heating effect in the discharge wire is so great that the thermometer no longer suffices for its measurement. Again, when the discharge takes place in free air between a knob and a very fine point, whose distance asunder is so regulated that no spark ensues, the heating effect in the wire is very small; whereas it is very great if the fine point is replaced by a blunt one, so as to cause a spark between the two electrodes. In both cases where the spark appeared, the time during which the same quantity of electricity was discharged was greatly diminished; hence, according to a well-known law, the heating effect was greatly increased. Exactly in the same manner as we are able to change the mode of discharging a battery through water and through free air by bringing the electrodes nearer to one another, and by changing the form of one of them, we can, as the new experiments show, attain the same end in very rare air by diminishing the negative electrode. The battery charge can pass through rarefied air, between arbitrarily-shaped electrodes, in two essentially different ways. In the first, which might be called the glimmering manner, a reddish luminous cone of air takes part in the discharge, its summit touches the positive electrode, and its base lies always at some distance from the negative electrode. The stratum of air in contact with a great part of the negative electrode takes part in the discharge in a less degree, and glows with a peculiar blue light. The second manner of discharge is that which I have called discontinuous. A narrow cylinder of air touching both electrodes takes part in the discharge, is rendered white hot, and burst asunder in the same manner as a metallic wire is which connects both electrodes, and whose diameter does not exceed a certain limit dependent upon the charge of the battery. The glimmering discharge takes place so slowly, that the surfaces of both electrodes remain uninjured, and only a small amount of heat is generated in the whole discharge circuit, whereas the discontinuous discharge tears away parts of the electrodes, scatters them red-hot on all sides, and heats the discharge circuit. Whatever may be the distance between the electrodes, the glimmering can be converted into the discontinuous discharge by increasing the density of the discharged quantity of electricity, by the admission of air, or, as our late experiments justify us in concluding, by diminishing the magnitude of the negative electrode. That is to say, if the surface of the negative electrode be very small in comparison to the surface which, with the employed density of the discharged quantity of electricity, would have been covered with a glimmering light, the discharge instead of being glimmering will be discontinuous. The recorded experiments are in complete harmony with this remarkable conclusion. When the direction of the discharge current was from the small platinum surface to the naked knob, the brass rod, and the inferior plate, that is to say, when the negative electrode had a sufficiently large superficial area, of the many partial discharges, which, with decreasing density, constitute the discharge of the battery, a part of the last, owing to their small tension, must have passed as glimmering discharges, and thus have exercised no heating effect. When, on the contrary, the direction of the current was from the knob to the small platinum surface, the negative electrode was limited in magnitude, and a part of the glimmering must have been converted into discontinuous discharges, and thus become effective in heating the circuit. Hence, in accordance with experiment, when the discharge passes from the surface to the knob, the heat in the remaining part of the discharge circuit must be smaller than when the discharge has an opposite direction. It is evident that the number of the glimmering discharges which become effective by reversing the current differs according to the form and distance of the electrodes. Cases may present themselves where the difference in the heating effect is inappreciable; but wherever it does occur, it must be in favour of the stated direction; and this, without exception, I have found experiment to corroborate. The explanation above given is also supported by ocular evidence. If the cylinder is placed in the shade, and observed during the time that the discharge passes from the knob to the surface, a brilliant light is noticed in the space between the electrodes, whilst the knob and rod are but feebly luminous. It may here be remarked, that this brilliant light between the electrodes consists of two dazzling colours; at the negative electrode it is reddish-violet, at the positive greenish-blue. When, on the contrary, the discharge passes from the surface to the knob, a broad reddish light is observable, not only between the electrodes, but throughout the length of the glass cylinder, and stretching over the knob and rod. In the last case, the explanation supposes that a great part of the discharges are glimmering.

In free air, when the discharge spark passes between a very small surface and a knob, no difference between the heating effects in the discharge circuit is observable when the direction of the current is changed. We have yet to examine, therefore, to what degree the rarefaction of the air must be carried in order that this difference may be observable. The glass cylinder was screwed on to the air-pump and kept in communication with the barometer test, the foot and clamp of the cylinder were connected with the discharge circuit of the battery, by means of long copper wires, after the thermometer had also been introduced into it. The knob in the cylinder stood at a distance of $\frac{5}{6}$ths of an inch from the small surface. In the following table the heating effects corresponding to the unit of charge are given, each of which was calculated from four thermometric observations; in the fourth line the heating effect with a positive knob is given, that with a positive surface and equal pressure of air being supposed equal to 100.

Heating effects in the discharge circuit when the latter is interrupted by rarefied air.

Pressure of air in inches.	$\frac{1}{12}$.	$\frac{5}{12}$.	$\frac{5}{6}$.	$1\frac{2}{3}$.	$2\frac{1}{2}$.	$3\frac{1}{3}$.
A. Surface positive.	0·85	1·02	0·93	0·94	1·09	1·18
B. Knob positive...	1·28	1·13	0·96	0·96	1·08	1·19
Ratio of A to B ...	150	111	103	102	100	100

The increase of heat by reversing the discharge in the glass cylinder, and thus making the knob the positive instead of the negative electrode, decreases quickly with increasing pressure of air, and is no longer perceptible at a pressure of $2\frac{1}{2}$ inches. An interesting corroboration of the theorem, that in these experiments two essentially different kinds of electric discharge are involved, is presented by the circumstance, that, in the line A, the greatest heat occurs when the air is most dense ($3\frac{1}{3}$ inches pressure), and, in the line B, when the air is least dense ($1\frac{1}{12}$th of an inch pressure).

In fact, when the greater part of the partial discharges were glimmering, the admission of air must have prevented the glimmering, and thereby rendered these discharges effective in the generation of heat. When, on the contrary, the majority of the partial discharges were discontinuous, the increased quantity of air had the effect of increasing the interval between the several partial discharges, and, consequently, of diminishing the heating effect. Insomuch as, in reference to this heating effect, these opposite actions simultaneously took place in every experiment, it cannot appear surprising that sometimes the one, sometimes the other action was in excess. Thus, in order to explain the observation in the line A with $\frac{5}{12}$ths of an inch pressure, we must assume that by changing the pressure of air from $\frac{1}{12}$ to $\frac{5}{12}$, the effect due to the prevention of the glimmering is in excess, whilst at $\frac{5}{6}$ths of an inch pressure the retardation of the discontinuous discharges is perceptible in the observation. From $3\frac{1}{3}$ inches pressure upwards only one kind of discharge, the discontinuous, appears to exist, whatever may be the condition of the electrodes; and, in fact, at $2\frac{1}{2}$ inches pressure it was no longer possible to detect any difference in the luminous phænomena in the cylinder when the knob was positive and when it was negative. In both cases a bright, broad expanse of light was alone visible in the space between the electrodes.

As might have been predicted, the discharge of the battery, after its passage through air rarefied to $\frac{1}{12}$th of an inch pressure, produced no deflection of the galvanometer needle. When one end of the galvanometer coil was connected with the conductor of an electric machine by means of a wire and the cylinder of rarefied air, the other end being in connexion with the earth, a considerable deflection of the needle was caused by turning the machine; no constant change in this deflection, however, was observable when the position of the cylinder in the circuit was changed, a result which will not surprise when the uncertainty of the experiment is considered.

Experiments on the Induction Apparatus.

The small induction apparatus, described at the commencement, was excited by means of one of Daniell's elements, and the bundle of iron wires half withdrawn from the primary coil. The coil of a delicate galvanometer was introduced into the circuit of the induction current as well as the glass cylinder used with the Leyden battery, containing air rarefied to $\frac{1}{12}$th or $\frac{1}{8}$th of an inch of pressure; lastly, a commutator was introduced so as to change the direction of the current in the glass cylinder without altering anything in the remaining part of the circuit. This position of the commutator was chosen in order to simplify the experiment itself, as well as its description; if the commutator is applied so as to reverse the direction of the induction current throughout the whole of its circuit, or, indeed, that of the voltaic current in the primary coil, an audible change in the play of the tongue connected with the induction apparatus ensues, which has the effect of still further complicating the experiment. Within the cylinder was a brass knob $\frac{3}{8}$ths of an inch thick, which was at first placed at a distance of $\frac{5}{6}$ths of an inch from the platinum surface. When the small surface was the positive electrode, that is to say, when it was connected with what was at first defined to be the positive pole of the induction apparatus, the needle of the galvanometer suffered a *normal* and maximum deflection of 70°; when the knob was the positive electrode, an *abnormal* deflection of 10°. By a *normal* or an *abnormal* deflection is to be understood one which does, or does not, correspond with the direction of the current. On repeating the experiment, a normal deflection of 10° was obtained when the knob was positive. The knob was withdrawn to a distance of $1\frac{5}{12}$ inch from the platinum surface. The knob being negative, I obtained a normal deflection of 40° or 50°; but when the knob was positive, a small abnormal deflection, which afterwards changed to a small normal one. When the knob and surface were $2\frac{2}{3}$ inches asunder, it was necessary to increase the intensity of the induction current, by pushing the bundle of iron wires further into the primary coil, in order that the current might traverse the space between the electrodes. In both positions of the electrodes the deflection was normal, but it was greater with the knob negative than positive. A smaller knob, $\frac{1}{24}$th of an inch in diameter, was placed at a distance of $\frac{17}{18}$ths of an inch from the small surface in the cover. When the knob was negative, the deflection amounted to 60° and was normal; when positive, the deflection was zero at the moment of closing the circuit, but passed into a normal or an abnormal one of 10°. The knob was replaced by a horizontal brass disc, $\frac{11}{12}$ths of an inch in diameter and $1\frac{1}{6}$ inch distant from the platinum surface. The disc being negative, the deflection was normal, and attained the maximum; when positive, it was abnormal, and amounted to 20°. I varied these experiments in numerous ways, but the common result of all was, that when the small platinum surface was positive, the deflection of the needle was always normal, and always occurred immediately after closing the induction circuit; when, on the contrary, that surface was negative, the deflection was sluggish and retrograde. That in the last case the deflection is sometimes abnormal, has been observed also by M. Gaugain, who, had he taken the observation sufficiently into consideration, would not have been led to the erroneous conclusion which he has drawn from his experiments. For there are undoubtedly two oppositely directed induction currents acting in these experiments: the interruption current, in reference to which the poles of the induction coil have been defined; and the contact current, in reference to which the names of the poles of the induction coil must be changed. The action of the two opposite currents can be demonstrated by means of a decomposing apparatus still more successfully than by the galvanometer. Besides the galvanometer and the cylinder of rarefied air, in which a knob was placed $\frac{5}{6}$ths of an inch from the small surface, two platinum points were introduced into the induction circuit; they were about one inch asunder, and stood on a piece of paper moistened with iodide of potassium. As before, the commutator changed the direction of the induction current in the glass cylinder alone. When the small surface in the cylinder was connected with the positive pole of the induction coil, the stain of iodine in seven successive experiments appeared only under that platinum point which was positive in reference to the interruption current; when, on the contrary, the surface was in communication with the negative pole, the stain of iodine in thirteen experiments appeared eight times under the other platinum point alone, and five times under both points. The knob in the cylinder was next placed $1\frac{5}{12}$ inch distant from the small surface: when the latter was positive, the iodine stain appeared five times under the above-defined positive platinum point; when the surface was negative, four times under the other point. *When the whole current of a magneto-induction apparatus is transmitted through very rarefied air, between a very small and a comparatively great surface, the interruption current alone traverses the space between the surfaces when the small one, in reference to that current, is positive; when, on the contrary, the small surface is negative with respect to the interruption current, the contact current is also transmitted.*

It has long been known, that, on account of the small tension possessed by the contact current, the interruption current is the only one which can traverse a space of air. According to Poggendorff*, this small tension of the contact current results from the production of a closed conducting circuit in the neighbourhood of the induction coil when the primary circuit is closed. Thus, although M. Gaugain concluded from his experiments, that, with a certain form of the electrodes, the interruption current did *not* pass through very rarefied air, it follows from the present investigation, that in reality both the contact current and the interruption current are transmitted in this case. Consequently, if the term valve is still applied to Gaugain's apparatus, it must be considered as a valve for the contact current, and not for the interruption current. A glance at the luminous phænomenon furnishes a corroboration of the above theorem, if, indeed, any were required. In order that the light might be more sharply defined, I had the brass knob in the cylinder, as well as the disc, stained black so as not essentially to injure the conduction at their surfaces. When the small surface was positive, the luminous phænomenon was the same as that which takes place between two equal knobs, and which has often been described. A small, well-defined cone of reddish light extended from the small surface to the negative electrode, and ended at some distance therefrom, so that between the base of the cone and the negative electrode a broad dark space remained. The whole surface of the negative electrode, whether its form was that of a knob or a disc, was surrounded by a beautiful blue light which stretched also over the rod. The light was throughout equally bright and quiescent. When, on the contrary, the small surface was negative, the light began immediately to flicker and change its intensity. When the current had been previously regulated so as to produce a very small deflection of the magnetic needle, or none at all, the form of the light was much the same as before. The reddish light of the positive electrode only was less distinctly defined: it extended itself until it reached the negative electrode without leaving a free dark space. The negative electrode itself was only partially surrounded by a blue light, that is to say, it extended only over the anterior hemisphere of the knob or the anterior surface of the disc. When the current was increased, the blue light was confined to a still smaller space, and was momentarily hidden by the broad reddish sheaf of light which extended between the electrodes. When these luminous phænomena have been once observed in the dark, it is easy to recognize them in daylight by merely shading the cylinder, whatever connexion the electrodes may have with the induction coil.

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* Phil. Mag. vol. x. p. 124.

The experiments with the Leyden battery serve to explain the phænomena of the induction apparatus. It has been shown that the electricity in the battery generates a less amount of heat in the discharge circuit when it is transmitted through rarefied air from the small to the large surface. The same takes place with the interruption current of the induction apparatus. A brass disc was placed in a glass cylinder at a distance of 1 line from the small surface in the cover, and the cylinder, together with the delicate electric thermometer, introduced into the circuit of the induction coil. The heat produced was in general small, but considerably smaller when the direction of the interruption current was from the small to the large surface. When the large disc was negative, the liquid in the thermometer sunk from 1 to 3 lines; when that disc was positive, from 5 to 6 lines. I obtained a constant result by allowing the current passing from the great to the small surface to continue for some time, until the liquid in the thermometer attained its lowest position; the sudden reversion of the current immediately caused the liquid to rise. By simultaneously introducing a galvanometer into the circuit, it was found that the greatest heat was produced when the deflection of the needle was smallest, and the smallest heat when the deflection was greatest. With the Leyden battery no doubt can be entertained that the change in the heating effect is occasioned by a change in the method of discharge, that an increased heating effect is produced by converting the slow glimmering discharge into the much more quickly completed discontinuous discharge. If a similar change in the discharge of the interruption current of the induction apparatus be assumed, the cause of the changed action will be the same, and that of the passage of the contact current will be found. From experiments with the Leyden battery, it is known that the conversion of the glimmering into the discontinuous discharge occasions a less deflection in a galvanometer introduced into the circuit, as well as a diminution in the quantity of any substance decomposed in the circuit. Whilst the glimmering discharge does not injure the electrodes, the discontinuous discharge tears parts of them away, and scatters the incandescent particles in the space between the electrodes. This space will consequently conduct electricity better, and the contact current can pass as a glimmering discharge. That for the transmission of this current a perfect conductor is not necessary, is proved by the fact that the same can be transmitted through a long strip of tolerably moistened paper. These premises being granted, the observed actions of the induction current can be deduced without difficulty. When the whole current of the induction apparatus is passed through well-rarefied air, between a small and a large surface, and the direction of the interruption current is from the small to the large surface, this current alone is transmitted, and that as a glimmering discharge: in the discharge circuit, therefore, only a small amount of heat will be generated, a galvanometer will be strongly and constantly deflected in one direction, and an electrolyzable substance will be decomposed in the ordinary manner, so that a certain constituent of the same will be separated at a certain electrode. If the direction of the current be reversed, so that the interruption current pass through the rarefied air from the great to the small surface, this current will suffer partial discontinuous discharge; hence a greater heat will be produced in the discharge circuit, the deflection in the galvanometer will be less, and a smaller quantity of the electrolyte will be decomposed. These actions will be modified by the glimmering discharge of the contact current, which can now pass in consequence of the greater conductibility of the space, its heating action will be weak, the deflection and decomposition which it causes will both be strong, and in direction opposed to the corresponding actions of the interruption current. The deflection of the needle, therefore, can be still further diminished, or destroyed, or even reversed; the separation of a particular constituent at the former electrode may be prevented, or it may even be disengaged at the opposite electrode; or, lastly, the effects of both currents may remain perceptible: the constituent in question may be separated at both electrodes, and the needle may be deflected sometimes on one side, sometimes on the other. In fact, this changeability in the result takes place, not only in different experiments, but frequently in the same experiment; nor can it surprise one who reflects that it is produced by the changeable successive action of two opposite currents. On the contrary, with the former direction of the induction current, the result, being influenced by one current only, is always essentially the same.

Though not immediately connected with the subject in hand, I may here remind the reader of a paradoxical fact upon which some light may, perhaps, be thrown by the above-proved influence of the interruption current upon the passage of the contact current. When the current of a powerful voltaic battery is allowed to pass in a luminous manner between two wires in free air, the wire which forms the *positive* electrode is more heated than the other, and is the first to become incandescent and to fuse. When, on the contrary, the current of an induction apparatus is allowed to pass with sparks between two equal wire points in air, that wire only becomes incandescent and fuses which forms the negative electrode of the interruption current, and which, as I have long ago shown, can be easily recognized by the covering of blue glimmering light upon it. Hitherto this paradox has not been solved. Is it not possible that the interruption current, part of which apparently passes here as a glimmering discharge, has but little heating effect, but on the other hand increases the conductibility of the space, and facilitates the passage of the contact current, which manifests itself principally by the incandescence of the wire end? It would then be the *positive* electrode of the latter current which produced the greatest heat, so that between the action of a voltaic and that of an induction current no difference would exist. This would be a very simple and acceptable solution of the paradox.

The above research by Prof. Riess was communicated to the Academy of Berlin on the 18th of June, and shortly afterwards, on the 30th of July, M. Gaugain made another communication to the Academy of Paris "On the Electric Conductibility of Air*." Having shown in a preceding communication†, that, within certain limits, the conductibility of air appeared to decrease with its density, he deemed the fact of sufficient theoretical importance to demand re-examination, with a view of more completely substantiating the fact. In his last research, the apparatus described under the name of an electric valve was sometimes employed, and sometimes simple tubes with metallic wires for electrodes. The general result obtained with all the apparatus was the same. When the induced currents of Ruhmkorff's apparatus were passed through air whose density was gradually diminished, the intensity of the electricity, as shown by a galvanometer, began at first to increase (departing from the atmospheric pressure), attained a maximum, afterwards began to decrease, and finally attained a minimum value when the vacuum was as perfect as possible. The density of air corresponding to the maximum intensity of current varied with the magnitude and disposition of the apparatus, with the distance between the electrodes, and with the superficial magnitude of the latter. At one time this density was as low as 2 millims., at another as high as 50 millims. In general, when the negative electrode presented a large surface, and the thickness of the air traversed by the current was considerable, the period during which the intensities increased was of great extent, and that during which they decreased scarcely appreciable; on the other hand, when the surface of the negative electrode was very small, the period during which the intensities increased embraced only a small number of degrees, that during which they decreased being greatly extended. When the electric valve was employed, the intensity of current, corresponding to the most perfect vacuum an ordinary machine is capable of producing, was sensibly equal to zero; with tubes containing electrodes of metallic wire, the intensity of current corresponding to the most perfect vacuum was always a maximum, but never zero, and in these cases the period during which the intensities diminished was not well defined so long as the distance between the electrodes was small.

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* *Comptes Rendus*, vol. xli. p. 152.

† Ibid. vol. xl. p. 640, and Phil. Mag. vol. x. p. 207.

The manner in which Prof. Riess would view these results is already known; the following explanation is proposed by M. Gaugain:—"When an electric current traverses a space containing air, the resistance with which it meets ought to vary, as in the case of liquids, with the mean section of the gaseous space which serves to conduct the same. The dimensions of this space cannot, it is true, be immediately ascertained, though it is natural to suppose that they are closely related to those of the sheaf of light which is observable. This hypothesis being admitted, we must conclude that when the negative electrode presents a large surface, the mean section of the current continually increases with the increased rarefaction of the air, for experiment proves that in this case the sheaf of light continually expands. The result is, that the increased section compensates, in a certain measure, for the diminution of conductibility, which is a consequence of the diminution of the elastic force of the air. When, on the contrary, the surface of the negative electrode is very limited, the conducting space formed by the reunion of the elementary currents cannot dilate, nothing compensates for the diminished conductibility, and consequently this diminution manifests itself."

After observing that in the above explanation it is assumed that the same laws which govern the transmission of currents through liquids also apply to gases, M. Gaugain cites an experiment as an additional justification of the assumption. An air-pump receiver was procured, in the upper part of which was a copper rod sliding, air-tight, in a leather box; after exhausting the air under the receiver, the plate of the air-pump was connected with one pole of Ruhmkorff's apparatus, and the rod with the other; the usual sheaf of light was seen between the copper rod and the plate of the air-pump. An insulated thin disc of tin was now placed between the electrodes, at a distance of 5 or 6 centimetres from each of them; the luminous appearance within the receiver was completely changed. When the current traversed the cylinder from top to bottom, a cone of red light descended from the copper rod, and the upper surface of the disc was covered with a blue light; a second cone of red light descended from the lower surface of the disc, and a second covering of blue light surrounded the exhausting tube of the air-pump. Thus the tin disc constituted a double pole, in exactly the same manner as when a metallic disc is placed between the electrodes in a liquid electrolyte, its surfaces become contrary poles, and the resistance in the circuit, instead of being diminished, is increased. This fact being established, the copper rod was next depressed so that its lower extremity approached the tin disc; the latter was then pierced as a sheet of paper would have been, and the current passed unbroken through the tube which was formed; only one cone of red light, only one dark space, and only one blue covering was then seen. M. Gaugain considers this a decisive proof that electricity suffers resistance in its passage from a solid to a gaseous, or from a gaseous to a solid body; "for it is clear," says he, "that the obstacle which compels the current to pierce the tin disc does not proceed from the resistance due to the metal itself, inasmuch as this resistance is much more feeble than that of the air displaced by the tin."

The conductibility of the partial vacuum obtained by means of an air-pump being so small, it was natural to expect that the more perfect one in a barometer tube would be still less conductible. M. Gaugain proved this by connecting the poles of Ruhmkorff's apparatus with the mercury, and with a platinum wire fused into the top of an ordinary barometer tube. He found it impossible to obtain the least appearance of light in the barometer chamber when the distance from the summit of the mercurial column to the platinum wire exceeded 3 or 4 millims., although he employed six of Bunsen's cells.

This result is in opposition to one obtained by M. Masson, and communicated to the Academy on the 7th of February, 1853; but M. Gaugain accounts for this divergence satisfactorily. In fact, the greatest possible care is necessary to expel the air and moisture in the barometer tube; indeed, when all precautions have been taken and a vacuum obtained which will not allow the induced currents to pass, the application of a spirit-lamp to the top of the tube for a few moments is sufficient, although it may have become cool afterwards, to make the vacuum conduct the currents again. It appears that the tubes used by Masson were heated by a lamp after the introduction of the mercury, and hence his results.

XLI. *On the Molecular Constitution of Crystals.*
By James D. Dana.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN a communication to your Journal for August last, by Mr. R. T. Forster, the author makes a statement which I beg the privilege of correcting.

Mr. Forster says, speaking of my views on twin-crystals, published in the American Journal of Science for 1836, that the ideas "were directly borrowed from Sir David Brewster," and that the paper by Sir David, containing his views, "was *quoted by Dana* in the American Journal of Science, and formed the basis of his investigations, without, however, being acknowledged."

At the time of this "quoting by Dana" in 1832, I was still in my teens, and for fourteen years afterwards I had no connexion with the American Journal, and was not in expectation of any such connexion. Instead of Sir David Brewster's paper forming the basis of my investigations, I had not at that time, culpable though it may appear, perused his paper. The conclusions were mainly arrived at during a cruise at sea in the years 1833 and 1834, when I had by me only a copy of Phillips's 'Mineralogy,' from the measurements of crystals in which excellent work, not knowing at that time in my solitude what had been done by others, I devised also the Haüyan method of crystallographic calculation. In my paper, published in 1836, I cite the opinion of Voltz as to the existence of three axes of attraction in molecules, and obviously do not claim originality on that point, the main one in which the theories agree. In a later paper in the American Journal of Science, 1847, vol. iv. p. 169, on "Cohesion," I have alluded to the observations of Sir David Brewster, and made a citation from his paper. The views I have brought forward are different from his in my regarding the axial dimensions of the molecule to be those of the crystal, and not a resultant between the pressure from the combining attraction and the dimensions: and on the subject of twin-crystals, I am not yet aware that anything has been written by Sir David.

I have the highest admiration for the labours of Sir David Brewster, and on the points of difference, while sustaining my own opinions, would not venture to affirm my own correctness, knowing that the subject of molecules is the last that science will clear up. I write, not to make claims of originality, but simply to correct an erroneous statement, and disavow all intentional injustice.

Very respectfully, your obedient Servant,
James D. Dana.

New Haven, Conn.,
September 15, 1855.

XLII. *On certain curious Motions observable at the Surfaces of Wine and other Alcoholic Liquors.* By James Thomson, *A.M., C.E., Belfast*[*].

THE phænomena of capillary attraction in liquids are accounted for, according to the generally received theory of Dr. Young, by the existence of forces equivalent to a tension of the surface of the liquid, uniform in all directions, and independent of the form of the surface. The tensile force is not the same in different liquids. Thus it is found to be much less in alcohol than in water. This fact affords an explanation of several very curious motions observable, under various circumstances, at the surfaces of alcoholic liquors. One part of these phænomena is, that if, in the middle of the surface of a glass of water, a small quantity of alcohol or strong spirituous liquor be gently introduced, a rapid rushing of the surface is found to occur outwards from the place where the spirit is introduced. It is made more apparent if fine powder be dusted on the surface of the water. Another part of the phænomena is, that if the sides of the vessel be wet with water above the general level surface of the water, and if the spirit be introduced in sufficient quantity in the middle of the vessel, or if it be introduced near the side, the fluid is even seen to ascend the inside of the glass until it accumulates in some places to such an extent, that its weight preponderates and it falls down again. The manner in which I explain these two parts of the phænomena is, that the more watery portions of the entire surface, having more tension than those which are more alcoholic, drag the latter briskly away, sometimes even so as to form a horizontal ring of liquid high up round the interior of the vessel, and thicker than that by which the interior of the vessel was wet. Then the tendency is for the various parts of this ring or line to run together to those parts which happen to be most watery, and so there is no stable equilibrium, for the parts to which the various portions of the liquid aggregate themselves soon become too heavy to be sustained, and so they fall down.

The same mode of explanation, when carried a step further, shows the reason of the curious motions commonly observed in the film of wine adhering to the inside of a wine-glass, when the glass, having been partially filled with wine, has been shaken so as to wet the inside above the general level of the surface of the liquid; for, to explain these motions, it is only necessary further to bring under consideration that the thin film adhering to the inside of the glass must very quickly become more watery than the rest, on account of the evaporation of the alcohol contained in it being more rapid than the evaporation of the water.

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[*] Communicated by the Author, having been read at the Meeting of the British Association at Glasgow.

That this part of the explanation is correct, or that these motions of the film in the wine-glass are really due to evaporation, may be shown by a very decisive experiment. If a vial be partly filled with wine and shaken, and then allowed to rest, no motion of the kind described will be found to occur in the thin film wetting the inside, provided that the vial be kept corked. On the cork being removed, however, and the air contained in the vial, and saturated with the vapour of wine, being withdrawn by a tube, so as to be replaced by fresh air capable of producing evaporation, a liquid film is instantly to be seen creeping up the interior of the vial with thick or viscid-looking pendent streams descending from it like a fringe from a curtain. These appearances are quite of the same kind as those met with in the open wine-glass.

Another experiment may be made to show, in a very striking way, the phænomenon of the more watery portion of the surface of a mixed liquid drawing itself away from the more alcoholic portion as follows:—If water be poured to the depth of about a tenth of an inch or less on a flat silver tray or marble slab, previously cleaned from any film which could hinder the water from thoroughly wetting its surface; and if then a little alcohol or wine be laid on the middle of that water, immediately the water will rush away from the middle, leaving a deep hollow there, and indeed leaving the tray bare of all liquid except an exceedingly thin film of the spirit, which continues always thinnest close to the margin of the water, because the water draws out to itself every portion of the spirit which approaches close to its margin.

The experiment alluded to near the commencement of the present paper, in which spirit was to be introduced into the middle of a surface of water previously dusted over with fine powder, may be well conducted as follows:—A tube for supplying the spirit should be provided*, which may be three or four inches long, half an inch or three-quarters in diameter, and terminating at bottom in a small open point, which, if found too wide, may be partially stopped by the insertion of a piece of thick soft thread, such as a strand from the wick of a spirit-lamp. A knot on the thread inside of the tube will serve as a valve to curtail or stop the flow of the spirit when required. The surface of the water should be clean and free from any kind of pellicle, such as is often met with, and is sometimes not easily avoided. It should then be lightly dusted over with some fine powder not apt to be quickly wet: Lycopodium powder will serve the purpose. Then the tube filled with spirit is to be dipped with its open point into the surface of the water, and instantly a nearly circular patch round the point of the tube will be seen occupied with liquid rushing outwards and completely divested of the covering of powder, while on the part outside of that patch there will be seen, by the motions of the powder, one, two, three, or many radial streams flowing outwards from the middle, and other return streams or eddies flowing backwards to the margin of the patch, on arriving at which each particle seems suddenly as if driven outwards with a rapid impulse. The margin of the central patch is usually to be seen formed like as of leaves of a plant growing out all round, and some superimposed on others, and all in rapid motion. The nature and causes of these forms of the margin, and of the eddies outside of the margin, I have not as yet been able satisfactorily to explain.

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* The tube of a small glass syringe as sold by apothecaries will serve the purpose well.

Another experiment may be made which is quite in accordance with the explanations already given, and which, being due to condensation of alcohol on a surface of water, is interesting when viewed in comparison with that in which the motions were shown to be produced by evaporation:—If a silver spoon, perfectly wetted with water so that a thin film adheres to it, be held over an open cup or vessel containing strong alcohol, the surface of the liquid will become greatly agitated with numerous motions, which are to be attributed to the unequal and varying condensation of the vapour of the alcohol at different parts of the surface of the film, according as the vapour is wafted about in fumes by the air.

While engaged in the investigation of the phænomena which I have now described, my attention has been turned to some other very interesting phænomena previously observed by Mr. Cornelius Varley, and described by him in the fiftieth volume of the Transactions of the Society of Arts. He observed with the aid of the microscope numerous motions of extremely curious and wonderful characters in fluids undergoing evaporation. Although I have not yet had it in my power to examine into all the phænomena he has discovered relative to these motions, yet I think that many of them, or all, are to be explained according to the principles I have now proposed.

I have not had access to the Transactions of the Society of Arts to read Mr. Varley's paper in full, but I quote the following abstract of his results from Queckett's 'Treatise on the Microscope,' 1st ed. p. 413:—"The plan recommended is as follows: take an animalcule-cage of moderate size, and upon the tablet place a drop of turpentine or spirits of wine, &c., then slide over it the thin glass cover, but do not compress the fluid very much; the microscope being placed in the vertical position, and provided with a magnifying power from 40 to 100 diameters, the contents of the cage are to be examined in the same way as if animalcules were contained in it. As the evaporation of either of these fluids takes place, numerous currents and vortices will be seen, especially if a small quantity of finely-powdered coal be ground into them; the particles of coal being very light, are held in suspension whilst the evaporation is going on, and are whirled about by the currents in different directions." The following fluids Mr. Varley has given as the best for the illustration of the currents:—

"1. A drop of spirits of wine, or of naphtha, exhibits two, three, or four vortices or centres of circulation, according to the size of the drop; and if these vortices are viewed laterally, the lines of particles will be seen forming oblique curves from top to bottom of the drop.

"2. Oil of turpentine shows a rapid circulation in two continuous spirals, one to the right, the other to the left, around the drop. These meet in the opposite diameter, from which the particles are carried slowly across the diameter to the place of starting, and this continues while there is fluid enough to let it be seen.

"3. If, however, the drop does not exceed one-tenth of an inch in diameter, it presents the appearance of particles continually rising up in the middle, and radiating in gentle curves to the circumference.

"4. If the liquid be put into a very small vial, similar motions are perceived, the particles when they have reached the side of the vial going down to rise up afterwards in the centre or axis.

"5. If a bubble of air be enclosed in the liquid, motions similar to those described in No. 2 are observed in the part immediately in contact with the bubble.

"6. In a flat drop of new wine laid on the tablet or disc of the aquatic live-box, but not compressed by the cover, the motion was a regular uniform circulation, the particles rising from below at one end of the drop, then passing straight across on the surface, and descending at the other end."

XLIII. *On Pressures of Saturated Vapours.*
By W. J. MACQUORN RANKINE, *C.E., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN consequence of some remarks which have been addressed to me respecting my letter on the above subject in the Number of your Magazine for October, I think it desirable to explain, that the formula

$$\log \mathrm{P} = \mathrm{A} - \frac{\mathrm{B}}{\tau} - \frac{\mathrm{C}}{\tau^2} - \ldots \qquad (2)$$

is in no way deduced from the formula

$$\log \mathrm{P} = a - b \cdot \log \tau - \frac{c}{\tau}, \qquad (3)$$

and that the coefficients A, B, C are not functions of *a, b, c*. In fact, the formula (2) was *suggested as probable* by the hypothesis of an attraction between the liquid and its vapour, as explained in the Numbers of your Magazine for December 1851 and December 1854; and A, B, C are part of an indefinite series of constants determined empirically, so as to agree with M. Regnault's experiments; while (3) is the result of an independent hypothesis as to a condition of saturated vapour, which may, as I have already stated, prove to be purely ideal, and respecting which, therefore, I refrain from entering into detailed investigations until we possess exact data as to the densities of vapours.

The formula of Professor Holtzmann,

$$\log \mathrm{P} = a - \frac{b}{\tau},$$

appears to be nearly the same with that of Roche, and may be held to constitute the *first approximation* towards the law of the maximum pressure of vapours.

I have the honour to be, Gentlemen,
Your most obedient Servant,
W. J. MACQUORN RANKINE.

Glasgow, October 1, 1855.

P.S. Since the above remarks were written, Sir John Lubbock has done me the favour to send me a table showing the differences between the results of his formula (1) and those of the experiments of M. Regnault on steam; the three constants in the formula having been determined from the same data with those in the formula published by me in 1849, viz. the pressures of steam at 26°·86, 100°, and 220° Centigrade.

These differences are, on the whole, greater than those resulting from my formula (2); but still they are so small as to lead to the following conclusion:—that to Sir John Lubbock belongs the merit of having in 1840 first proposed a formula containing *three* constants only, which is capable of giving the pressures of vapours with a near approximation to accuracy throughout the whole range of existing experiments.

W. J. MACQUORN RANKINE.

Glasgow, October 6, 1855.

XLIV. *On the Magnetic Variation in the Vicinity of the Cape of Good Hope. By* Colonel EDWARD SABINE, *Treas. & V.P.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, 13 Ashley Place, Oct. 16, 1855.

IF you should have room for the accompanying communication in your November Number, you will oblige me by its insertion.

I remain,
Your obedient Servant,
EDWARD SABINE.

In a letter which has been printed and circulated by the Board of Trade, addressed to Mr. Hartnup of the Liverpool Observatory, by Captain Klein of the Dutch Bark 'Magdaléna,' who had been supplied at Liverpool, in April 1854, with a set of the marine meteorological instruments which had been made under the superintendence of the Kew Committee of the British Association, and verified at the Kew Observatory, it is satisfactory to read the testimony of an obviously experienced navigator in regard to the efficiency of the Kew marine barometer. Captain Klein remarks, "I never observed with such a nice and beautiful working instrument as the Kew model barometer. With the greatest motion of the vessel it makes no greater jump than half a tenth; while at the same time it is so very sensitive, that the series will give a correct idea of the atmospheric tides." Captain Klein's testimony cannot but be very satisfactory to the gentlemen of the Kew Committee, and yet more so to Messrs. Welch and Adie, by whose ingenuity and pains the prevailing faults in the ordinary construction of the marine barometer have been greatly surmounted, whilst the price of the instrument has been reduced to about two-thirds of its usual previous amount.

The object of the Board of Trade in printing and circulating Captain Klein's letter, was doubtless a desire to make his experience of the good performance of these barometers known amongst the owners and masters of our merchant ships, with the view of introducing the use of these instruments more extensively in our mercantile marine,—an object which may be further promoted by making Captain Klein's opinion known to the readers of the Philosophical Magazine.

But the particular object of this communication is not so much to give additional currency to the Captain's commendation of the Kew barometer, as to notice a remark of his which occurs subsequently in the same printed letter, regarding maps of the magnetic variation. The remark is as follows:—"It was surprising to see how incorrect the variation charts are: my results differed often greatly from Raper's approximations, and I should frequently have wavered had I not invariably obtained the same amount as was indicated by the last observations made on land." With respect to one of the authorities to which Captain Klein refers, viz. "Raper's approximations," I have of course nothing to say; I am not myself acquainted with the work; but as being myself the author of a recent Map of the Variation over the North and South Atlantic Oceans, I could not but be solicitous, on reading Captain Klein's remark, to obtain a knowledge of any inaccuracies within the limits of that map which might appear to be substantiated by his observations; and having understood that the observations had been placed in the hands of Dr. Buys Ballot, Director of the Marine Meteorological Department in Holland, I wrote to that gentleman requesting him to communicate to me any particulars contained in them which might appear to be at variance with the variation lines in my map, to which I knew Dr. Buys Ballot had the means of referring.

I may here notice, that as the accuracy of my map rests wholly on the accuracy of the observations which were coordinated in its construction, I can have no motive whatsoever for not being glad to receive corrections, if more accurate observations are produced than those which were at my command when the map was constructed; and that for the express purpose of obtaining such corrections (should any appear to be required) by means of the officers of the Dutch Navy, who have the general reputation of being careful and skilful navigators, I presented several copies of my map a short time after it was published to Chevalier Swart, Hydrographer of the Dutch Navy, for distribution among Dutch officers. The chief peculiarities in this map which might be considered to add to its practical value, were, that it was constructed in all its parts for one and the same epoch, viz. 1840; that no other observations than those of contemporaneous, or very nearly contemporaneous date, were employed in its construction; that all the observations on which it was based were engraved on the face of the map; and that a memoir was attached to it in which these observations were discussed, and their corrections, either for local attraction if made on board ship, or for the difference of epoch if any, were carefully stated; and a table was also given referring to different latitudes and longitudes, by which the variation in any particular locality within the limits of the map might be approximately corrected for any other epoch (not very distant from 1840), conformably to the secular change which had taken place in the previous half century, or more exactly between 1787 and 1840.

Dr. Buys Ballot has kindly engaged to furnish me with a full account, both of Captain Klein's observations and of others made in the Dutch frigate 'De Ruyter,' in a similar voyage between Batavia and Amsterdam. It appears that a large portion of these observations apply to other parts of the ocean than those comprised in my map; but *the vicinity of the Cape of Good Hope*, which is included in that map, is specially pointed out by Dr. Buys Ballot as a locality in which the Dutch observations are at variance with the maps. Dr. Buys Ballot writes as follows:—"It is true that the corrections for local attraction are not applied (in the Dutch observations) in strictly due manner; but I have persuaded myself that since the local attraction was very small in those two vessels, and did not exceed 4° in any direction, this error could not affect the conclusions in any appreciable manner: therefore I think that really the maps give in the vicinity of the Cape a too great westerly variation."

If I could entertain the slightest doubt about the accuracy of the variation as given in my map for the vicinity of the Cape of Good Hope, I should most readily wait for the particulars which Dr. Buys Ballot is good enough to promise to furnish me; but I think that I am able to show that the variation in that locality as given in the map, whether it be taken for 1840 or for the present time, rests on too solid a foundation to be shaken by observations, however carefully made on shipboard, but which are uncorrected for the ship's attraction.

On reference to the memoir accompanying the map (Phil. Trans. 1849, part 2. art. xii.), it will be seen that the first in importance amongst the authorities for the variation at the Cape of Good Hope are the observations at the magnetic observatory at that station, giving as a mean result 29° 7′. The only doubt that could attach to a determination made with every care and precaution, and with the excellent instrumental means of a fixed magnetic observatory, resting also in this case on years of observation, would be the possibility of the existence of station error, viz. possible local deviation occasioned by rocks of particular character in the vicinity of the observatory affecting the needle; but this is a matter which has been long since set at rest by the strict accordance which has been found between the results obtained at the observatory itself, and at stations distant from it a few miles in different directions. Amongst these may be named the variations most carefully observed by Admiral Du Petit Thouars in False Bay in April 1839 (29° 9′), and by Sir James Clark Ross in Simon's Bay in April 1840 (29° 23′). The general result is also confirmed by nearly cotemporaneous observations (1837 to 1845), made at sea in the vicinity of the Cape by Admirals Vaillant and Du Petit Thouars, and Captains Sir James Clark Ross, Crozier, Moore and Clark, which are detailed in the memoir referred to, and are inserted in the map; the mean of all the sea observations, viz. 28° 51′, being shown in the map to correspond to the intersection of lat. −35° and long. 17° 30′ E., which, with a proper allowance for latitude and longitude, accords almost precisely with 29° 07′ at the observatory in lat. −33° 56′ and long. 18° 29′ E.

So far in regard to the accuracy of the map in relation to the period for which it was constructed, viz. 1840; but it may be asked, may not the magnetic variation in the vicinity of the Cape have altered since 1840 by the operation of secular change, so as to make the determinations on which the map was based inapplicable to the present epoch? It is well known, that generally in the Southern Atlantic the westerly variation increases at a nearly uniform rate of secular change of about 8′ in the year; and it is also known that the same rate of secular increase, or more exactly 7′·56, did obtain at the Cape of Good Hope from the earliest observations of Davis and Keeling in 1605 and 1609, to those of Captain FitzRoy and of Sir James Ross in 1836 and 1840. (Cape Magnetic Observations, vol. i. p. lx.) Had this rate of secular change (which had lasted so long) continued to 1854 (when Captain Klein's observations were made), the variation at the Cape would have been nearer 31° than 29° 07′; that is to say, the westerly variation in my map would have been *in defect* in 1854, instead of *in excess* as Dr. Buys Ballot supposes. The Cape, however, was selected as a station for a magnetic observatory, amongst other reasons for this one in particular, that the form of the variation lines in its vicinity, and their known progress from east to west in the temperate zone of the southern hemisphere, gave reason to believe that the secular increase of the variation which had taken place for so many years was approaching the period of its termination, when it would be succeeded by a nearly stationary variation continuing for several years. The probability of this alteration in the secular change is noticed in the memoir accompanying the map. (Phil. Trans. 1849, p. 200.) That it has been realized, has been shown by the records of the Cape Observatory. Between January 1841 and August 1850, the westerly variation had increased from 29° 7′ only to 29° 14′·6; or at something less than an average annual rate of 1′. (Cape Magnetic Observations, vol. i. p. lxv.) Supposing this rate to have continued to 1854, when Captain Klein observed, the variation at the Cape should have been 29° 18′, which is still 11′ *greater* than that which is shown by the map (instead of *less*).

But we are not without data from the vicinity of the Cape still nearer the epoch at which Captain Klein observed, from which we may obtain a corroboration of the correctness of the variation as given in the map, and its applicability to the present time. Lieutenant Dayman, R.N., formerly of the Magnetic Observatory of Van Diemen's Land, whose skill and accuracy as a nautical surveyor are justly held in the highest estimation, has transmitted to the Admiralty the results of determinations in 1853 at eighteen different stations, partly observed on land, and partly at sea on board H.M. Surveying Ship Hydra, between Capes Hangklip and Aguilhas. By the kindness of the Hydrographer, these were sent to me as soon as they reached the Admiralty. The mean of the eighteen determinations is 29° 24′. It would be difficult to say what result could be more in accordance with the map, allowing for the small difference of latitude and longitude. Therefore, had Captain Klein's observations agreed with the latest observations made on land (*i. e.* at the Cape Observatory and on the coast in its vicinity), they would also have substantially agreed with my map. If they differ from it, they differ also from the observations obtained by many observers of high reputation both on shore and at sea.

In conclusion, I wish to express the satisfaction with which I view the evidence afforded by Captain Klein's communication of the increasing interest which is taken in these questions, since the attention of the mercantile marine of different nations has been called to their importance; and I also see with pleasure the freedom with which Captain Klein's remarks and criticisms are given. We may confidently expect that one result of such increased attention will be improvement in maps of the magnetic variation in localities where they need improvement: and it may not be amiss to notice, that since, by the increased employment of iron in ships, the compass has been rendered much more subject to deviation than it formerly was, maps of the magnetic variation which can be relied upon, both as to epoch and amount of variation, have, besides their primary use in navigation, this additional value, that a single observation taken of the variation with the ship's head on the course which she may happen to be steering, furnishes, by comparison with the map, the deviation of the compass with the ship's head on that particular point in that part of her voyage; and thus such a map facilitates the task, which has now become an essential part of correct navigation, of the continual examination, and correction when required, of the tables of the deviation with the ship's head on different points. The inverse proceeding, or the endeavour to correct maps formed on a sufficiently extended basis of coordinated land and sea observations by observations at sea which have not been corrected for deviation, is hardly to be recommended: and doubtless as variation maps become more deserving in themselves, and their value is better known, will be less likely to be practised.

XLV. *On the Curvature of the Indian Arc; and the great Geological Law, that the various parts of the Solid Crust of the Earth are perpetually undergoing a Change of Level.* By Archdeacon Pratt.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Of the existence of the great law brought to light by the science of geology, that the various parts of the earth's surface are perpetually undergoing a change of level, there can be now no reasonable doubt. The observations made on landmarks on the coast of the Baltic,—the singular phænomenon of the columns of the temple of Serapis near Naples, although now above the water-level, as they must have been when the temple was built, and yet showing marks of having been below the surface, in the perforations of a kind of sea shell-fish at the present day flourishing in the neighbouring waters,—and the original researches and bold generalizations of Mr. Darwin among the ancient and recent coral formations of the South Seas,—combine to establish the law in the most satisfactory manner.

2. To assign a cause of this law may be more difficult than to recognize its existence. Mr. Babbage, and others after him, have attributed it to the expansion and contraction of masses of rock beneath, caused by accessions and withdrawals of heat through chemical agencies. Obscure as the origin of the law may be, the importance of its discovery cannot be over-estimated, as it supplies so clear an explanation of the wonderful alternations of level which must have taken place to produce the phænomena of fresh- and salt-water formations which geology brings to light.

3. Satisfactorily as a law of nature may have been made out, it can never be out of place to bring forward new illustrations, especially in one of such importance as this law possesses. I shall therefore make no apology for asking you to admit into your Magazine a proof drawn from an entirely new province, that of mathematical physics. The circumstances and arguments on which it is based I will now relate.

4. The mean ellipticity of the earth has been investigated by both the Astronomer Royal and the late M. Bessel with great care, by a comparison of arcs of meridian measured in various parts of the earth; and although the methods were somewhat different, they arrived at the same conclusion, one making the ellipticity $\frac{1}{300\cdot7}$, the other $\frac{1}{300\cdot8}$, the difference being inappreciable. Now this is the ellipticity to which theory leads us in supposing the earth to be in a fluid state, the density to increase gradually towards the centre by the law $\frac{\sin qr}{r}$ (r being the distance from the centre, and q a constant), the density of the superficial stratum to be that of granite, and the mean density of the earth to be what the late Mr. F. Baily made it. This coincidence has been generally considered to be a conclusive argument in favour of the hypothesis that the earth was once fluid, and acquired its present form (at any rate its present *mean* form) at that time, and from hydrostatic principles.

5. These investigations, however, do not show that the whole meridian through any place is exactly elliptical, nor that the meridians through places in different longitudes are all alike. They show merely that the *mean* curvature of the different parts of the earth is that which corresponds with the fluid condition of its mass, which is, as I have said, a very strong argument in favour of the hypothesis of original fluidity.

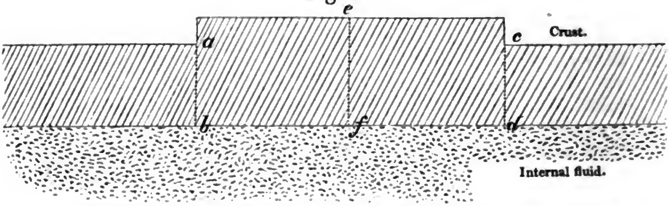
6. That the curvature of every meridian does not equal the mean curvature can, I think, be most satisfactorily proved in the case of the great Indian arc, about 800 miles long, and lying between Kaliana (about 50 miles from the Himalaya Mountains) and Damargida. This arc has been divided into two nearly equal parts, and the lengths and astronomical amplitudes of both have been determined with great precision. The astronomical amplitudes, however, need correction for the attraction of the Himalayas, which are found to have a sensible effect upon the plumb-line. In a paper published in the Transactions of the Royal Society for the present year, I have given a method for calculating the effect of this attraction, and have reduced the formulæ to numbers. On calculating the curvature of the Indian arc from the estimated lengths of its two portions, and the amplitudes corrected for attraction, I find that it is greater than the mean curvature; in fact, that the middle point of the whole 800 miles is raised a few feet higher above the cord joining its extremities, than if it possessed the mean curvature.

7. An hypothesis has been thrown out, since the publication of my result, to show that the effect of attraction from the enormous mass rising above the mean surface in the Himalayas,

would be counteracted by a counter or negative attraction arising from a *deficiency* of matter below*. It is assumed that the interior of the earth is fluid, and that the crust is sufficiently thin to allow the principles of hydrostatics to regulate the external form of the surface, or that this was the state of things when the surface took its present form. It is further assumed that the solid crust of the earth is light compared with the fluid mass on which it floats.

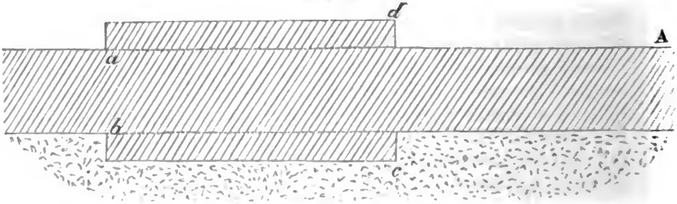
The case is then taken (fig. 1) of a table-land 2 miles high, 100 miles wide, and of indefinite length,—suggested, no doubt, by the plateau from which the Indus and Brahmapootra flow, more than 10,000 feet above the sea-level, and forming the most important part of the attracting mass in my paper. The mean crust is taken to be 10 miles thick, and therefore 12 at the table-land. It is then conceived that the crust is broken through in the dotted lines *ab*, *cd*, *ef*, where lies the greatest tendency to crack, and the force of cohesion necessary to prevent the two portions from sinking into the fluid below is estimated. The force is found to be equal to a weight of 20 miles' length of the rock; and the conclusion is justly drawn, that as no such power of cohesion can exist in the crust, the state of things represented in fig. 1 cannot be that of nature.

Fig. 1.



8. The hypothesis, as represented in fig. 2, is then suggested,

Fig. 2.



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* See a paper in the Transactions of the Royal Society for 1855, "On the Computation of the Effect of the Attraction of Mountain Masses," &c., by G. B. Airy, Esq., Astronomer Royal.

that the table-land is supported by the buoyancy of a mass of crust projecting downwards into the heavy fluid. This representing the state of things, a mass *ad* rising above the surface and attracting a distant station A, necessarily implies a corresponding *deficiency* of mass in the locality *bc*, causing an almost equal *negative* attraction at A counteracting the former positive attraction.

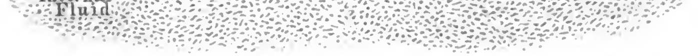
9. There are some difficulties in the way of this ingenious hypothesis which I wish now to state.

1st. On what physical principle can it be assumed that the crust of the earth is lighter than the fluid immediately below it? It might rather be imagined, that by the process of consolidation in using heat it would become contracted and therefore heavier.

2nd. If the result at which Mr. Hopkins has arrived in his calculations on precession be correct, that the solid part of the crust is at least 1000 miles thick, an enormous interval of time must have elapsed since the crust was sufficiently thin for the form of its surface to be affected by the fluid below. During this immense interval, there has been abundance of time for the geological law, of which I have spoken at the beginning of this paper, to have altered the form of the surface, originally given by hydrostatic principles, to such a degree as to have obliterated all traces of it; indeed, the whole table-land and all the mountain range may have been heaved up during this time.

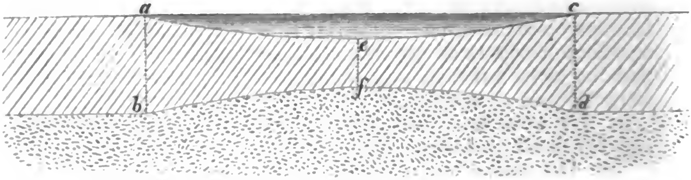
10. But I have another, a third difficulty. If the crust be so thin, or was so when the earth assumed its present contour, what must be the state of the crust beneath deep and wide oceans?

Fig. 3.



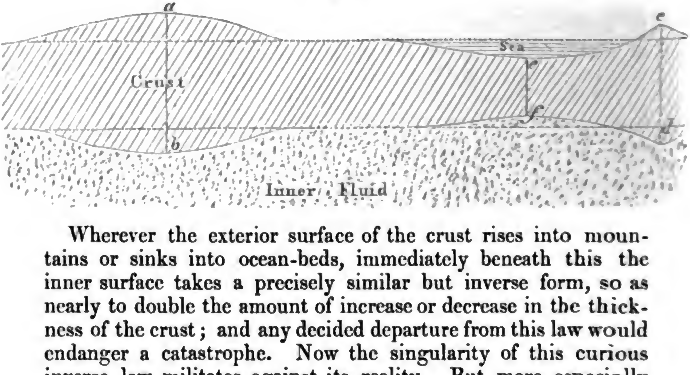
The state of things in fig. 3 could no more exist than that of fig 1. If we take the width of the ocean to be only 100 miles, and average depth 2 miles, and density half that of the crust, the cohesive force necessary to prevent the upward pressure of the internal fluid from breaking up the crust and letting in the ocean, is a force equal to the weight of about 14 miles of rock. This state of things cannot exist, and we must substitute fig. 4, in

Fig. 4.



which there is a hollow in the under part of the crust, allowing the heavy fluid to rise to such a height above its mean level that the deficiency of weight of the ocean may exactly equal the excess of weight of the heavy fluid below. Any departure from this form would endanger a catastrophe. By introducing, then, the principles of floatation to explain the cause of the exterior form of the earth's surface, we come to the following conclusion regarding the inner surface of the crust.

Fig. 5.



Wherever the exterior surface of the crust rises into mountains or sinks into ocean-beds, immediately beneath this the inner surface takes a precisely similar but inverse form, so as nearly to double the amount of increase or decrease in the thickness of the crust; and any decided departure from this law would endanger a catastrophe. Now the singularity of this curious inverse law militates against its reality. But more especially does it seem to contradict all notions of the process of solidification by cooling down. We can conceive no reason why the crust should have thickened so much less at *ef* than at *ab* and *cd*, but quite the contrary. It might have been expected that at *f* the crust would have thickened downwards faster than at *b* and *d*, till a pretty uniform thickness had been attained and kept up.

11. I feel disposed, therefore, to hesitate about the existence of a deficiency of matter below mountains, and rather to suppose that the variations of the present external form of the earth have

not been brought about by hydrostatical principles, although the *mean form* has most probably been so.

12. If this be a correct conclusion, it follows (see art. 6) that the country about the middle of the Indian arc is more elevated by some feet than its extremities. No doubt if this long arc were further subdivided into many portions, and their lengths estimated and their amplitudes corrected for attraction, they would all be found to have different ellipticities, the mean of which would be the ellipticity of the Indian arc. For it is hardly to be supposed, that if the form of the whole arc has been made to deviate from the fluid form by upheaval from below, that the curve of 800 miles has perfectly preserved its symmetry north and south of the middle point. Different portions are no doubt bent differently, and this process of calculation would show it. The calculation which I have made (and for the details of which I must refer to the paper in the Philosophical Transactions) exhibits this principle only for the whole arc, and not for its parts separately.

13. But this, I think, is sufficient to establish my point, and I gather from it the two following conclusions:—

I. The crust of the earth was too thick at the time the surface assumed its present form, and therefore is now too thick, to suffer the fluid below to regulate that form; for it has been proved that a large portion of it, the Indian arc of 800 miles, has not the form which the fluid theory requires.

II. Since the epoch when the crust became too thick for the fluid below to regulate the form of the surface, other forces—of upheaval, or of depression, or both—must have been in action; for the middle of the Indian arc is some feet higher than its two extremities when measured from the fluid level.

The forces thus brought to light by an investigation in mathematical physics must be of the nature of those which exhibit the great geological law I proposed to illustrate.

J. H. Pratt.

Lausanne, October 6, 1855.

XLVI. *Examination of an Organic Matter found in an ancient Egyptian Bottle.* By Philip B. Ayres, *M.D. Lond.*[*]

A SHORT time since I was requested by my friend B. Nightingale, Esq., to submit to analysis a substance he had shaken out of an ancient Egyptian bottle, supposed by him to have contained wine. The bottle was of coarse earthenware, formed of clay mixed with a considerable proportion of sand; it was globular, with a very short neck; a mouth so narrow that the little finger could not be inserted into it; with two ears through which a strap or cord might be passed so as to suspend it; and of the capacity of about a pint.

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[*] Communicated by the Author.

The substance shaken from this bottle had evidently formed a crust on its inner surface, produced by evaporation of, or subsidence from, its former contents, mixed with sand derived either from the coarse internal surface or from without. As presented to me, it was a mixture of sand with flattened masses of a deep brownish-black colour, earthy fracture, porous structure, and a saline, somewhat styptic taste.

When heated on platinum foil over the spirit-lamp, a fragment burned with a clear flame, without either fusion or intumescence, leaving a charred mass, from which the charcoal was burned off with difficulty.

As the substance was thus proved to be organic, I picked out the masses from the sand; but as much of organic matter still remained mixed with the sand, this was employed for the qualitative, reserving the purer masses for the quantitative analysis.

A. The sand mixed with the organic matter was boiled repeatedly with distilled water, yielding a deep reddish-brown solution, approaching very closely to that of port wine, which after filtration remained perfectly transparent, but of so deep a colour that when in bulk it was black and opake.

The solution reddened litmus distinctly, but not strongly.

Hydrochloric, nitric, and sulphuric acids threw down a light, flocculent, deep reddish-brown precipitate, leaving the supernatant liquid of a pale straw-colour. Solution of caustic potash caused no precipitate or change of colour. Solution of gelatine caused no change. Tincture of galls also produced no change. Lime-water deepened the colour, and produced a copious light-brown flocculent precipitate. Oxalate of ammonia rendered the fluid turbid without deepening its colour. Diacetate of lead caused so copious a brown precipitate as to render the liquid gelatinous. Acetate of lead, a moderately bulky, brown, flocculent precipitate, but not nearly so copious as that with the diacetate. Sulphate of copper, a brown precipitate. Sulphate of alumina and potash also a brown precipitate. Bichloride of tin, a light, flocculent, brown precipitate. A portion of the liquid evaporated to dryness left a black, shining, brittle mass.

B. The residue *A*, insoluble in boiling water, was treated with boiling alcohol, spec. grav. 0·835. The solution was light yellow. It became turbid on the addition of water, and deposited granular, semicrystalline, fatty matter by evaporation, which was solid when cold, but melted on application of heat.

C. The mixture of sand and organic matter insoluble in water and alcohol was next treated with solution of caustic potash, which without the aid of heat dissolved the greater portion of the remaining organic matter, and became deep brownish-black, although perfectly transparent when viewed in thin layers.

This was evidently the insoluble humic acid analogous to that found in vegetable earth. It was precipitated in a flocculent form, of a brown colour, by hydrochloric acid, and the supernatant liquid became light straw-colour. When washed and dried, it was left as a shining, black, friable mass.

D. A portion of the deposit was heated to redness in a test-tube. It swelled up, partially fused, gave off a dense white vapour, which condensed into drops of a light yellow empyreumatic oil, and left a bulky black charcoal. No odour of ammonia could be perceived, nor were any white vapours produced by the introduction of a rod moistened with hydrochloric acid into the tube, thus proving the absence of nitrogen. The odour of the vapour and of the oil was that of burnt vegetable matter.

Quantitative Analysis.

A. 39·7 grains, the whole of the purer organic matter separable from the sand, digested with alcohol, spec. grav. 0·835, yielded a deep brownish-red solution. The whole of the soluble matter was removed by repeated digestion with fresh portions of alcohol until exhausted. The alcoholic solutions were mixed and evaporated to dryness, leaving a brownish residue weighing 15·5 grains. During the evaporation, part of the waxy or resinous matter separated and collected on the surface of the fluid.

a. The alcoholic residue was treated with æther, which dissolved a considerable portion, forming a light reddish solution, which reddened blue litmus-paper strongly.

The ætherial solutions, on evaporation to dryness, left a solid reddish-brown matter weighing 11·4 grains, which approached more in consistence to wax than fat. This residue yielded scarcely a tinge of colour to boiling water, on the surface of which it melted and collected in drops.

b. The undissolved residue (*a*) was treated with cold distilled water, which formed a vinous-red solution, leaving the undissolved matter almost colourless. The solution reddened litmus rather strongly. On evaporation to dryness it left a brownish-black matter, similar in most of its characters to the aqueous extract (*B*), weighing 2·9 grains. Heated to redness, it first fused, then swelled up enormously, leaving a voluminous charcoal, which burned off very slowly and yielded 1·16 grain of ash.

c. The residue of the successive action of æther and water (*b*) was treated with a boiling mixture of equal parts of æther and alcohol, in which it wholly dissolved. The solution was yellowish, became turbid on cooling, and again recovered its transparency when heated. By evaporation it yielded a nearly white, friable, granular powder, which melted on exposure to heat, and burned with a white flame.

B. The residue (*A*) exhausted by alcohol, treated with cold distilled water, yielded a red-brown solution so deep in colour as to be opake when in bulk. It was finally exhausted, first with cold and afterwards with boiling water. The solution, evaporated to perfect dryness, left a black, shining, brittle residue, weighing 11·0 grains.

When heated, it burned with a very feeble flame, semi-fused, swelled moderately, and left a somewhat voluminous iridescent charcoal, which burned off very slowly. 5·5 grains yielded exactly 1·0 grain of ashes.

The ash was partly soluble in distilled water. The soluble portion had a decided alkaline reaction, effervesced with acids, remained clear on the addition of solution of caustic potash, gave a precipitate with nitrate of silver which was almost wholly redissolved by nitric acid in excess; yielded a copious white precipitate with chloride of barium, which did not wholly redissolve in hydrochloric acid, and gave a decided yellow precipitate with bichloride of platinum and alcohol.

The portion of the ashes insoluble in water dissolved completely in nitric acid with effervescence. The solution yielded,—with caustic potash, a white flocculent precipitate; with oxalate of ammonia, an abundant white precipitate; with caustic ammonia, a very slight precipitate, which became abundant on addition of phosphate of soda; and with ferrocyanide of potassium, a deep blue tint, the liquid remaining clear.

The ashes then consisted of carbonate of potash, traces of sulphate of potash, carbonate of lime, traces of carbonate of magnesia and of oxide of iron.

C. After complete exhaustion by alcohol and water, the insoluble residue *B* was treated, first with cold, and afterwards with boiling solution of caustic potash, so long as the liquid was coloured. The cold solution of potash became of so deep a red-brown colour as to be almost opake, even in a single drop.

The mixed liquids were precipitated by dilute hydrochloric acid in the slightest possible excess; the precipitate collected, washed and dried, weighed 5·5 grains. It was black, shining, and resembled perfectly in appearance the dried aqueous extract *B*.

D. After complete exhaustion by alcohol, water, and caustic potash, the residue was light fawn-coloured, approaching to white. When perfectly dried, it weighed 7·7 grains.

After complete incineration the loss was 2·7 grains, which must be considered as insoluble humus, the *terreau charbonneux* of Berzelius.

The ashes partially dissolved in concentrated hydrochloric acid, leaving only some grains of sand and insoluble silica. The liquid was thrown on a filter; the insoluble matter, collected, washed and dried, weighed exactly 1·0 grain; consequently the portion of the ash soluble in hydrochloric acid weighed 4·0 grains. The filtered liquid yielded a copious, white, flocculent precipitate, with solution of caustic potash; a copious, white, but not flocculent precipitate with oxalate of ammonia; a very slight turbidity with chloride of barium; a very slight flocculent precipitate with caustic ammonia; a blue tinge, but scarcely a perceptible precipitate, with ferrocyanide of potassium; and a greenish-blue tinge with ferridcyanide of potassium.

The ash consisted then chiefly of carbonate of lime, with traces of carbonate of magnesia; traces of sulphate of lime and of oxide of iron, silica and sand.

The substances submitted to examination are then,—

Resinous or waxy matter soluble in æther (*Aa*)	28·7
Friable, pulverulent, resinous matter, soluble in a mixture of alcohol and æther (*Ac*).	3·0
Extractive matter soluble in alcohol and water (*Ab*)	4·3
Red colouring matter soluble in water (*B*) . .	22·6
Humic acid (*C*)	13·8
Insoluble organic matter, the *terreau charbonneux* of Berzelius	6·8
Ashes	20·5
	99·7

The results of this analysis lead to the conjecture that the bottle was used for containing red wine; but as modern wines contain mere traces of resinous or waxy matter, it was necessary to refer to the customs of the ancients and their treatment of wines. The ancients, not contented as we are in the present day with wine in its natural condition, were accustomed to flavour their wines with a variety of substances. Dr. Smith* states that they were accustomed to throw into the vats or amphoræ various condiments or seasonings (medicamina, condituræ), among the chief of which were sea-water, turpentine, pitch, tar or resin, gypsum, burned marble, calcined shells, aromatic herbs, spices and gums. Another mixture called mulsum was composed of wine or must mixed with honey, in the proportion of four of the former to one of the latter. Honey contains wax, and it is by no means impossible that the residue in the Egyptian bottle was that of some one of these vinous compounds. The large amount of waxy or resinous matter evidences that it could not be pure wine, but either one of the compounds alluded to, or mulsum; most probably the former. As the Romans derived many of their customs from the Greeks, so in turn the Greeks in all probability borrowed from the Egyptians, with whom they held frequent intercourse. It has been suggested to me, that the matter contained in the bottle was a kind of varnish employed to fix the fresco-paintings adorning the Egyptian tombs, which might be a solution of some waxy or resinous substance in wine; but I am disinclined to this opinion, on account of the large quantity of red colouring matter extracted by water, as well as the small portion of residue contained in the bottle.

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* Dictionary of Greek and Roman Antiquities.

The red colouring matter was very analogous in its characters to that in port wine, but it had evidently undergone gradual and partial change into humic acid and insoluble humus. These together constitute above one-third of the whole organic matter. The large quantity of ashes is partly owing to the admixture of loose sand from the sides of the bottle, and to what must have penetrated from without in so sandy a country as Egypt.

XLVII. *On the Experiment of* Leidenfrost. *Extracted from a Letter from* Professor BUFF *to* Professor TYNDALL.

MY DEAR SIR,

WHEN I consider how well grounded your views are with regard to the origin of the phænomenon described by Trevelyn, I am surprised to find, with reference to another of the phænomena of heat, namely the experiment of Leidenfrost, that the theory of a repulsive action at a distance by heat seems to find favour in your eyes. At least I must conclude so much from a casual remark in a memoir upon the subject by Mr. A. Church. Mr. Church mentions an experiment, which he witnessed in your lectures, by which you show that water in the spheroidal condition interrupts the passage of an electric current to the red-hot metal underneath*, and observes that you regard this experiment as a proof that the surface of the vessel is separated by an interval from the drop within it.

The same experiment has been often made and observed under various conditions by myself, but I am not able to ascribe to it the same force of proof. I only find that the passage of electricity through water in the spheroidal state is rendered extremely difficult, and solely because the points of mutual contact between water and metal are reduced to a comparatively small number. The current can thereby certainly to all appearance be interrupted; whether this occurs or not, depends, according to my experience, for a given liquid, on the delicacy of the galvanometer, on the strength of the electromotive force, and finally on the size of the drop itself. The spheroidal water behaves towards the hot surface as mercury towards a wire dipped into it, which is not wetted by the liquid metal. In a memoir published several years ago (*Annalen der Chemie und Pharmacie*, vol. lxxvii. p. 1), I have communicated different modes of experiment, which are, moreover, very easily devised, for the purpose of rendering visible the propagation of an electric current through water in the spheroidal condition. The most evident, however, as I learned afterwards, is the electric glow first observed by Mackrell on thin metallic wires, when they are used as electrodes of a very powerful electric circuit and immersed in hot water. In the case of the negative pole, it might be objected that during the continuance of the red heat it is surrounded by a brush (*sprühlicht*); this, however, is not necessarily the case with the positive wire, and when it is absent, the wire exhibits at a red heat the phænomenon of Leidenfrost perfectly. The current, and even the electrolytic decomposition, also continue; for when the positive electrode disperses no brush, the gas developed upon it is always pure oxygen. The continuance of the current in this and similar cases is perhaps not a certain proof of the existence of direct contact; for the layer of gas, or vapour between the water and the red-hot surface, may at this high temperature assume a small degree of conductibility. I am, however, only desirous of showing, that the extinction of the current in other cases cannot be regarded as a proof of the existence of an interval between the water and the surface by which it is sustained.

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* M. Poggendorff was, I believe, the first to perform this experiment. —J. T.

Indeed, the spheroidal condition of water exhibits in its most essential phænomena sufficient evidence against the idea that insulation and repulsion are necessary conditions of the spheroidal state. Permit me to lay before you briefly those proofs which I regard as the most important.

In a silver spoon coated with soot, water asserts the spheroidal form at ordinary temperatures, exactly as when the spoon is raised to a high temperature.

The drop which rolls upon the hot surface of a liquid, for example a drop of æther upon water, bends the surface of the liquid, as a mercury surface is bent by the pressure of a rod of glass.

Large drops assume an oscillatory motion, which often continue with remarkable regularity, and cause the formation of star-shaped figures: without a partial contact of the water with the surface beneath, the production of these figures would be an impossibility.

The greatest drops are not obtained upon such surfaces as radiate heat best, but on those which possess the highest capacity of conduction; surfaces of silver are therefore preferable to all others for these experiments.

The phænomenon is produced with volatile liquids only; the higher the boiling-point of the liquid, the more strongly heated must be the surface on which it rests, in order to hold it in the spheroidal state. The experiment therefore succeeds better with æther than with water when bad conductors are used as vessels, such, for example, as porcelain and glass. The lowest temperature at which the heated surface is not wetted by the liquid poured upon it, must always be higher than the boiling-point of the liquid. For example, in a silver basin at 75° C. æther, at 137° C. alcohol, and at 144° C. water first assume the spheroidal form.

In making these observations, I will not assert that Leidenfrost's drop cannot be separated from the heated surface which supports it. I believe, on the contrary, that even drops of considerable magnitude may be torn from the surface by the force of the developed vapour. I only affirm that such insulation is not the necessary condition for the production of the phænomenon. This is caused, in my opinion, by the alteration produced by heat in the relative attractions exerted by the surface of the vessel upon the liquid, and by the liquid particles upon each other.

The surface of a vessel will, it is known, be wet by a liquid poured into it when the attraction of the matter of the vessel for the liquid is greater than that of the particles of the liquid for each other. In the reverse case the liquid asserts the spheroidal form. The mutual attraction of the particles of the body is without doubt diminished at a high temperature. If a silver basin be heated with the water within it, the surface continues wet even when the liquid boils. In fact, in this case the temperature of the silver basin is only a little higher than that of the water. The mutual attraction of the silver and the particles of water has therefore been diminished almost in the same proportion as the attraction of the water particles for each other; the relation of the attractions remains constant. If the silver basin be heated beyond the temperature of boiling water, its adhesion for water must diminish in a greater degree than the cohesion of the particles of the liquid which still remains at its boiling-point. Hence sooner or later a temperature will be attained, at which the cohesion overpowers the adhesion. The wetting of the surface then ceases, and the spheroidal condition sets in, quite in harmony with the ordinary laws of capillarity.

Perhaps, my dear Sir, you will be inclined to agree with this manner of viewing the subject; if so, I entertain the hope that you will excuse this somewhat elaborate statement of the question. With great esteem, I have the pleasure to subscribe myself,

Yours most sincerely,
Giessen, Sept. 29, 1855. H. Buff.

With reference to the subject of Prof. Buff's interesting letter, I may be permitted to say that I have never published anything on the experiment of Leidenfrost, though I have taken some pains to instruct myself as to the cause of this singular phænomenon. At an early period of railway travelling, Mr. George Stephenson conceived the idea of making his locomotives rest on steam springs. The steam within the boiler formed an elastic cushion for the support of the boiler. My belief is, that the spring suggested by the great engineer is precisely that which supports the drop in the experiment of Leidenfrost; the liquid, I believe, rests upon a cushion of its own vapour. Some of my reasons for agreeing with those who hold this belief are as follows:—1. The incessant motion of the drop necessitates a continuous expenditure of force, and this force is only attainable by the production of vapour underneath the drop, which, as is well known, speedily diminishes in bulk in consequence of the evaporation. 2. In a deep concave vessel, the vapour escapes laterally from the liquid as it encounters the least resistance in this direction; but if the liquid be suffered to spread itself over a comparatively flat surface, or better still, if the surface be rendered a little convex towards the centre of the drop, the vapour finds the resistance in a lateral direction greater than that in a vertical direction, and the steam actually breaks through the thin liquid layer above it and escapes at the centre. The existence of the vapour beneath the drop is thus easily reduced to demonstration. 3. If an interval really exists between the drop and the surface, it occurred to me that if this interval could be made manifest to the eye it would tend to settle the question. With this view I made the following experiment:—A silver basin was inverted, and the polished convex surface slightly dinted so that a drop might rest upon it. A fine platinum wire was stretched vertically behind the basin and united with the poles of a small voltaic battery; the wire was in this way rendered vividly luminous. A drop of deeply black ink was placed upon the surface of the basin; bringing the eye on a level with the bottom of the drop, and looking towards the platinum wire, the latter was seen through the space between the drop and the bottom of the basin*. In this way the under surface of a drop,

* A little spirit mixed with the ink renders the experiment more easy.

half an inch or more in diameter, may be examined, and it will be found that the liquid is at no point in contact with the metal. While, therefore, I agree with M. Buff in rejecting the explanation which attributes the phænomenon to a propping-up of the drop by the calorific rays, I think the facts justify the opinion that an interval exists between the liquid and the metal. This interval, as proved by Poggendorff, is sufficient to interrupt an ordinary voltaic current; but being filled with vapour at a high temperature, it is quite conceivable, as surmised by M. Buff, that a small portion of a strong current may pass through it. M. Buff's observation, that the phænomenon is not exhibited except by those liquids which are capable of furnishing the necessary vapour, is also confirmatory of the above view: and the fact that good conductors, which quickly supply the heat necessary for evaporation, show the phænomenon better than good radiators which do not furnish this supply, appears to be equally corroborative of the opinion which I have ventured to espouse.

XLVIII. *On the Hypothesis of Molecular Vortices, or Centrifugal Theory of Elasticity, and its Connexion with the Theory of Heat.* By William John Macquorn Rankine, *C.E., F.R.SS.L. & E., F.R.S.S.A. &c.**

Section First.—*Relations between Heat and Expansive Pressure.*

(1.) IN February 1850, I laid before the Royal Society of Edinburgh a paper, in which the laws of the pressure and expansion of gases and vapours were deduced from the supposition, that that part of the elasticity of bodies which depends upon heat, arises from the centrifugal force of the revolutions of the particles of elastic atmospheres surrounding nuclei or atomic centres. A summary of the results of this supposition, which I called the Hypothesis of Molecular Vortices, was printed in the Transactions of this Society, vol. xx., as an introduction to a series of papers on the Mechanical Action of Heat; and the original paper has since appeared in detail in the Philosophical Magazine for December 1851.

In that paper, *the bounding surfaces of atoms* were defined to be imaginary surfaces, situated between and enveloping the atomic nuclei, and symmetrically placed with respect to them, and having this property—that at these surfaces the attractive and repulsive actions of the atomic nuclei and atmospheres upon each particle of atomic atmosphere balance each other. The pressure of the atomic atmospheres at those imaginary boundaries is the part of the total expansive pressure of the body which varies with heat; the effect of the centrifugal force of molecular vortices being to increase it.

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* Communicated by the Author, having been read to the Royal Society of Edinburgh, December 15, 1851.

[The following paper is republished by permission of the Council of the Royal Society of Edinburgh. It has received no alterations except some verbal corrections, some additions to the forms of notation, and the omission of some unnecessary repetitions. A few notes have been added, which are distinguished by dates.]

In the subsequent investigation it was assumed, that, owing to the symmetrical action of the particles of gases in all directions, and the small amount of those attractive and repulsive forces which interfere with the elasticity of their atmospheres, no appreciable error would arise from treating the boundary of the atmosphere of a single atom, in calculation, as if it were spherical; an assumption which very much simplified the analysis.

An effect, however, of this assumption was, to make it doubtful whether the conclusions deduced from the hypothesis were applicable to any substances except those nearly in the state of perfect gas. I have, therefore, in the present paper investigated the subject anew, without making any assumption as to the arrangement of the atomic centres, or the form of the boundaries of their atmospheres. The equations deduced from the hypothesis, between expansive pressure and heat, are therefore applicable to all substances in all conditions; and it will be seen that they are identical with those in the original paper; showing that the assumption, that the atomic atmospheres might be treated in calculation as if spherical, did not give rise to any error.

By the aid of certain transformations in those equations, I have been enabled, in investigating the principles of the mutual transformation of heat and expansive power, to deduce Joule's *law* of the equivalence of heat and mechanical power directly from them, instead of taking it (as I did in my previous papers) as a consequence of the principle of *vis viva*. Carnot's *law* is also deduced directly from the hypothesis, as in one of the previous papers.

(2.) *Classification of Elastic Pressures.*—The pressures considered in the present paper are those only which depend on the volume occupied by a given weight of the substance; not those which resist change of figure in solids and viscous liquids. Certain mathematical relations exist between those two classes of pressures, but they do not affect the present investigation.

To illustrate this symbolically, let V represent the volume occupied by unity of weight of the substance, so that $\frac{1}{V}$ is the mean density; Q the quantity of heat in unity of weight, that is to say, the *vis viva* of the molecular revolutions, which, according to the hypothesis, give rise to the expansive pressure depending on heat; and let P denote the total expansive pressure. Then

$$P = F(V, Q) + f(V). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In this equation, F(V, Q) is the pressure of the atomic atmospheres at the surfaces called their boundaries, which varies with the centrifugal force of the molecular vortices as well as with the mean density; and $f(V)$ is a portion of pressure due to the mutual attractions and repulsions of distinct atoms, and varying with the number of atoms in a given volume only. If the above equation be differentiated with respect to the hyperbolic logarithm of the density, we obtain the coefficient of elasticity of volume

$$\frac{1}{\mathfrak{d}} = -\frac{dP}{\frac{dV}{V}} = -\frac{d}{\frac{dV}{V}} F(V, Q) - \frac{d}{\frac{dV}{V}} f(V), \quad . \quad . \quad . \quad (1A)$$

where $\mathfrak{d}$ denotes the cubic compressibility.

The latter portion of this coefficient, $-\frac{d}{\frac{dV}{V}} f(V)$, consists of two parts, one of which gives rise to rigidity, or elasticity of figure, as well as to elasticity of volume, while the other gives rise to elasticity of volume only. The ratio of each of those parts to their sum must be a function of the heat, the former part being greater, and the latter less, as the atomic atmosphere is more concentrated round the nucleus; that is to say, as the heat is less; but their sum, so far as elasticity of volume is concerned, is a function of the density only.

That is to say, let the total coefficient of elasticity of volume be denoted thus,

$$\frac{1}{\mathfrak{d}} = J + \phi(C_1, C_2, C_3), \quad . \quad . \quad . \quad . \quad . \quad (1B)$$

C_1, C_2, C_3 being coefficients of rigidity round the three axes of elasticity, and J a coefficient of fluid elasticity; then

$$\left.\begin{array}{l} J = -\frac{d}{\frac{dV}{V}} F(V, Q) - \psi(V, Q) \cdot \frac{d}{\frac{dV}{V}} f(V) \\ \phi(C_1, C_2, C_3) = -\Big(1 - \psi(V, Q)\Big) \cdot \frac{d}{\frac{dV}{V}} f(V) \end{array}\right\} \quad . \quad . \quad (1C)$$

For the present, we have to take into consideration that portion only of the expansive pressure which depends on density and heat jointly, and is the means of mutually converting heat and expansive power; that is to say, the pressure at the boundaries of the atomic atmospheres, which I shall denote by

$$p = \mathrm{F}(\mathrm{V}, \mathrm{Q}).$$

Pressures, throughout this paper, are supposed to be measured by units of weight upon unity of area; densities, by the weight of unity of volume*.

(3.) *Determination of the External Pressure of an Atomic Atmosphere.*—Let a body be composed of equal and similar atomic nuclei, arranged in any symmetrical manner, and enveloped by an atmosphere, the parts of which are subject to attractive and repulsive forces exercised by each other and by the nuclei. Let it further be supposed, that this atmosphere at each point has an elastic pressure proportional to the density at that point, multiplied by a specific coefficient depending on the nature of the substance, which I shall denote by h. (This coefficient was denoted by b in previous papers.)

Let ρ and p' denote the density and pressure of the atomic atmosphere at any point; then

$$p' = h\rho.$$

Let

$$-g\frac{d\Phi}{dx}, \quad -g\frac{d\Phi}{dy}, \quad -g\frac{d\Phi}{dz}$$

be the accelerative forces operating on a particle of atomic atmosphere, in virtue of the molecular attractions and repulsions, which I have made explicitly negative, attractions being supposed to predominate. The property of the surfaces called the boundaries of the atoms is this,—

$$\left(\frac{d\Phi}{dx}\right)_1 = 0, \quad \left(\frac{d\Phi}{dy}\right)_1 = 0, \quad \left(\frac{d\Phi}{dz}\right)_1 = 0,$$

the suffix $_1$ being used to distinguish the value of quantities at those surfaces. Hence Φ_1 is a maximum or minimum. Those surfaces are symmetrical in form round each nucleus, and equidistant between pairs of adjacent nuclei. Their equation is

$$\Phi - \Phi_1 = 0.$$

Let M denote the total weight of an atom, μ that of its atmospheric part, and $\mathrm{M} - \mu$ that of its nucleus; then

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* Sept. 1855.—There is reason to believe that in many substances the elasticity of figure depends on more than three independent coefficients; but as the present paper relates to elasticity of volume only, the above equations are nevertheless sufficient to illustrate the classification of elastic pressures.

MV is the volume of the atom,

$\frac{\mu}{\text{MV}}$ the mean density of the atmospheric part, measured by weight, the nucleus being supposed to be of insensible magnitude,

and we have the following equations:—

$$\left.\begin{aligned} \text{MV} &= \iiint_{(1)} dx\,dy\,dz \\ \mu &= \frac{\mu}{\text{MV}}\iiint_{(1)} dx\,dy\,dz = \iiint_{(1)} \rho\,dx\,dy\,dz \end{aligned}\right\} \quad . \quad . \quad (2)$$

The suffix $(_1)$ denoting that the integration is to be extended to all points within the surface

$$(\Phi-\Phi_1=0).$$

According to the hypothesis now under consideration, *heat* consists in a revolving motion of the particles of the atomic atmosphere communicated to them by the nuclei. Let v be the common mean velocity possessed by the nucleus of an atom and the atmospheric particles, when the distribution of this motion has been equalized. I use the term *mean* velocity to denote that the velocity of each particle may undergo small periodic changes, which it is unnecessary to consider in this investigation.

Then the quantity of heat in unity of weight is

$$\text{Q}=\frac{v^2}{2g},$$

being equal to the mechanical power of unity of weight falling through the height $\frac{v^2}{2g}$. The quantity of heat in one atom is of course MQ, and in the atmospheric part of an atom μQ.

I shall leave the form of the paths described by the atmospheric particles indeterminate, except that they must be closed curves of permanent figure, and included within the surface $(\Phi-\Phi_1=0)$. Let the nucleus be taken as the origin of coordinates, and let α, β, γ be the direction-cosines of the motion of the particles at any point (x, y, z). Then the equations of a permanent condition of motion at that point are

$$\left.\begin{aligned} -\frac{1}{\rho}\cdot\frac{dp'}{dx}-\frac{d\Phi}{dx}-2\text{Q}\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\alpha=0 \\ -\frac{1}{\rho}\cdot\frac{dp'}{dy}-\frac{d\Phi}{dy}-2\text{Q}\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\beta=0 \\ -\frac{1}{\rho}\cdot\frac{dp'}{dz}-\frac{d\Phi}{dz}-2\text{Q}\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\gamma=0 \end{aligned}\right\}. \quad (3)$$

Let r be the length, and α', β', γ' the direction-cosines of the radius of curvature of the path of the particles through (x, y, z); then the above equations obviously become

$$\left.\begin{aligned} -\frac{1}{\rho}\frac{dp'}{dx}-\frac{d\Phi}{dx}-2\mathrm{Q}\frac{\alpha'}{r}&=0\\ -\frac{1}{\rho}\frac{dp'}{dy}-\frac{d\Phi}{dy}-2\mathrm{Q}\frac{\beta'}{r}&=0\\ -\frac{1}{\rho}\frac{dp'}{dz}-\frac{d\Phi}{dz}-2\mathrm{Q}\frac{\gamma'}{r}&=0 \end{aligned}\right\} \quad . \ . \ . \ . \ . \ . \ . \ . \quad (3\mathrm{A})$$

If these equations are integrable,

$$\frac{\alpha'}{r}dx+\frac{\beta'}{r}dy+\frac{\gamma'}{r}dz$$

must be an exact differential. Let $-\phi$ be its primitive function, the negative sign being used, because α', β', γ' must be generally negative. Then the integral of the equations (3) is

$$\log_e \rho=\frac{1}{h}\int\frac{dp'}{\rho}=\frac{1}{h}(2\mathrm{Q}\phi-\Phi)+\text{ constant};$$

or taking ρ_1 to denote the pressure at the bounding surface of the atom,

$$\rho=\rho_1 e^{\frac{2\mathrm{Q}}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)} \quad . \ . \ . \ . \ . \ . \ . \ . \quad (4)$$

Our present object is to determine the superficial-atomic density ρ_1, and thence the pressure $p=h\rho_1$, in terms of the mean density $\frac{1}{\mathrm{V}}$ and heat Q. For this purpose we must introduce the above value of ρ into equation (2), giving

$$\mu=\rho_1 . \iiint_{(1)} e^{\frac{2\mathrm{Q}}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)}\,dx\,dy\,dz,$$

whence

$$p=h\rho_1=h\mu\div\iiint_{(1)} e^{\frac{2\mathrm{Q}}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)}\,dx\,dy\,dz. \quad (5)$$

Let the volume of the atom be conceived to be divided into layers, in each of which ϕ has a constant value. Then we may make the following transformations :—

$$\left.\begin{aligned} \iiint dx\,dy\,dz&=k\mathrm{MV}\int e^{k(\phi-\phi_1)}\,\frac{\psi}{\psi_1}d\phi\\ \iiint e^{-\frac{1}{h}(\Phi-\Phi_1)}dx\,dy\,dz&=k\mathrm{MV}\int e^{k(\phi-\phi_1)}\frac{\omega}{\omega^1}d\phi \end{aligned}\right\} \ . \ . \quad (6)$$

k being a specific constant, and ψ and ω functions of ϕ, and of the nature and density of the substance.

The lower limit of integration of ϕ must be made $-\infty$, that it may include orbits of indefinitely small magnitude described round the atomic centre.

The nature of the function ψ is limited by the following condition:—

$$1=k\int_{-\infty}^{\phi_1} e^{k(\phi-\phi_1)}\frac{\psi}{\psi_1}\,d\phi. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

Let

$$\frac{2Q}{hk}+1=\theta.$$

Then these transformations give the following result for the pressure at the bounding surface of an atom:—

$$\left.\begin{aligned} p=h\rho_1 &= \frac{h\mu}{\mathrm{MV}} \div \int_{-\infty}^{k\phi_1} e^{\theta k(\phi-\phi_1)}\frac{\omega}{\omega_1}\,kd\phi \\ &= \frac{h\mu}{\mathrm{MV}}\cdot\frac{\theta\omega_1}{\omega_1-\frac{\omega'_1}{\theta}+\frac{\omega''_1}{\theta^2}-\frac{\omega'''_1}{\theta^3}+\&c.} \end{aligned}\right\}, \quad . \quad . \qquad (8)$$

ω'_1, &c. being the successive differential coefficients of ω with respect to $k\phi$, when $\phi=\phi_1$.

(4.) The following transformation will be found useful in the sequel.

Let λ be the indefinite value of $\log_e V$, and λ_1 its actual value in the case under consideration. Let G be the same function of λ which ω is of $k\phi$, and let G′, G″, &c. be its successive differential coefficients with respect to λ.

Let

$$H_1=\int_{-\infty}^{\lambda_1} e^{\theta(\lambda-\lambda_1)}G\,d\lambda=\frac{G_1}{\theta}-\frac{G'_1}{\theta^2}+\frac{G''_1}{\theta^3}-\&c.$$

Then

$$p=\frac{h\mu G_1}{\mathrm{MVH}_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

The function H has the following properties, which will be afterwards referred to:—

$$\left.\begin{aligned} &\frac{dH_1}{d\lambda_1}+\theta H_1-G_1=0 \\ &\int_{-\infty}^{\lambda_1} H\,d\lambda=-\frac{dH_1}{d\theta} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

(5.) *Case of a Perfect Gas.*—As a substance is rarefied, it gradually approaches a condition in which the pressure, under like circumstances as to heat, varies proportionally to the density. This is because the effect of the molecular attractions and repulsions on the pressure diminishes with the density, so that

Φ, ω, and G approximate to constant quantities. In the limiting or perfectly gaseous condition, therefore,

$$H_1=\frac{G_1}{\theta},$$

and

$$p=\frac{h\mu\theta}{MV}=\frac{h\mu}{MV}\left(\frac{2Q}{hk}+1\right). \quad . \quad . \quad . \quad . \quad (11)$$

(6.) *Equilibrium of Heat: Nature of Temperature and Real Specific Heat.*—When the atmospheres of atoms of two different substances are in contact at their common bounding surface, it is necessary to a permanent condition that the pressure in passing that surface should vary continuously.

Let (a) and (b) be taken as characteristics, to distinguish the specific quantities peculiar to the two media respectively. Let dm denote the volume of an indefinitely thin layer close to the bounding surface. Then the following equations must be fulfilled to ensure a permanent condition:—

$$p(a)=p(b)\,; \ \frac{dp'}{dm}(a)=\frac{dp'}{dm}(b) \text{ when } p'=p. \quad . \quad (12)$$

By making the proper substitutions in equation (4), it appears that

$$p'=pe^{k\theta(\Phi-\Phi_1)}\frac{\psi_1}{\omega_1}\cdot\frac{\omega}{\psi}.$$

Hence

$$\frac{dp'}{dm}(p'=p)=p\left(\theta\frac{d(k\phi)}{dm}+\frac{d\,.\,\frac{\omega}{\psi}}{dm}\right).$$

Now p is the same for both media: $\frac{\psi_1}{\omega_1}\cdot\frac{\omega}{\psi}=e^{-\frac{1}{h}(\Phi-\Phi_1)}$ is either a maximum or a minimum, so that its differential is null; and dm is a continuous function of $k\phi$, so that $\frac{d(k\phi)}{dm}(a)=\frac{d(k\phi)}{dm}(b)$. There remains only the function of heat,

$$\theta=\frac{2Q}{hk}+1.$$

Therefore the condition of a permanent state of molecular motion, that is to say, the condition of *equilibrium of heat*, is that this function shall be the same for the two substances; or that

$$\frac{2Q_a}{h_ak_a}=\frac{2Q_b}{h_bk_b}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Hence *temperature* depends on the above function only; for the definition of temperature is, that bodies at the same temperature are in a permanent condition as to heat, so far as their mutual action is concerned.

The ratio of the *real specific heat* of (a) to that of (b) is obviously

$$h_a k_a : h_b k_b. \quad \ldots \ldots \quad (14)$$

(7.) *Measure of Temperature and Specific Heat.*—The function θ is proportional to the pressure of a perfect gas at a constant density. That pressure, therefore, is the most convenient *measure* of temperature.

Let τ denote *absolute* temperature, as measured by the pressure of a perfect gas at constant density, and reckoned from a certain *absolute zero*, 274°·6 Centigrade, or 494°·28 Fahrenheit below the temperature of melting ice. Let κ be a constant which depends on the length of a degree on the thermometric scale, and is the same for all substances in nature.

Then

$$\left.\begin{aligned} \tau = \kappa\theta = \frac{2\kappa Q}{hk} + \kappa \\ Q = (\tau - \kappa)\,\frac{hk}{2\kappa} \end{aligned}\right\}; \quad \ldots \ldots \quad (15)$$

and the real specific heat of the substance, that is to say, the depth of fall, under the influence of gravity, which is equivalent to a rise of one degree of temperature in the body, is represented by

$$\mathrm{k} = \frac{hk}{2\kappa}. \quad \ldots \ldots \quad (16)$$

The pressure of a perfect gas is represented in terms of temperature by

$$p = \frac{h\mu\tau}{MV\kappa}. \quad \ldots \ldots \quad (17)$$

It may also be expressed thus: let τ_0 denote the absolute temperature of melting ice in degrees of the scale employed, and V_0 the volume of unity of weight of the substance in the theoretical state of perfect gas, at the temperature of melting ice and pressure unity; then

$$p = \frac{V_0}{V}\frac{\tau}{\tau_0}. \quad \ldots \ldots \quad (18)$$

On comparing this with equation (17), we see that

$$\left.\begin{aligned} \frac{h\mu}{\kappa M} = \frac{V_0}{\tau_0}; \\ \kappa = \frac{h\mu\tau_0}{MV_0};\ \frac{h\mu}{MV_0} = \frac{\kappa}{\tau_0}. \end{aligned}\right\} \quad \ldots \quad (19)$$

Now h is the specific elasticity of the atomic atmosphere of the substance, $\frac{\mu}{\mathrm{MV}_0}$ is the mean specific gravity of that atmosphere when the body is in the theoretical state of perfect gas, and κ and τ_0 are the same for all substances in nature. Therefore, *for every substance in nature, the mean specific gravity of the atomic atmosphere in the theoretical state of perfect gas is inversely proportional to the specific elasticity of that atmosphere.*

Real specific heat may also be thus expressed:—

$$\mathfrak{k} = \frac{\mathrm{V}_0}{\tau_0} \cdot \frac{k\mathrm{M}}{2\mu}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (20)$$

in which $\frac{\mathrm{V}_0}{\tau_0}$ corresponds to $\frac{1}{\mathrm{C}n\mathrm{M}}$ in my former papers, and $\frac{k\mathrm{M}}{2\mu}$ to $\frac{3k\mathrm{M}}{2\mu}$ or $\frac{1}{\mathrm{N}}$; so that

$$\frac{h\mu}{\kappa\mathrm{M}} = \frac{\mathrm{V}_0}{\tau_0} = \mathrm{N}\mathfrak{k}. \quad . \quad . \quad . \quad . \qquad (20\,\mathrm{A})$$

The factor N appears to depend on the chemical constitution of the substance[*].

(8.) *Total Pressure of Substances in general, expressed in terms of temperature.*

In equation (9) let $\frac{\tau}{\kappa}$ be put for θ; then

$$\mathrm{P} = p + f(\mathrm{V}) = f(\mathrm{V}) + \frac{h\mu\tau}{\kappa\mathrm{MV}}\mathrm{G}_1 \div \left\{\mathrm{G}_1 - \frac{\kappa\mathrm{G}_1{}'}{\tau} + \frac{\kappa^2\mathrm{G}''{}_1}{\tau^2} - \&\mathrm{c.}\right\}$$

$$= f(\mathrm{V}) + \frac{\mathrm{V}_0}{\tau_0}\frac{\tau}{\mathrm{V}}\left\{1 - \frac{\mathrm{A}_1}{\tau} - \frac{\mathrm{A}_2}{\tau^2} - \frac{\mathrm{A}_3}{\tau^3} - \&\mathrm{c.}\right\}, \quad . \quad . \qquad (21)$$

where

$$\mathrm{A}_1 = \frac{-\kappa\mathrm{G}'}{\mathrm{G}_1}; \quad \mathrm{A}_2 = -\frac{\kappa^2}{\mathrm{G}_1{}^2}(\mathrm{G}_1{}'^2 - \mathrm{G}_1{}''),$$

$$\mathrm{A}_3 = -\frac{\kappa^3}{\mathrm{G}_1{}^3}(\mathrm{G}_1{}'^3 - 2\mathrm{G}_1{}'\mathrm{G}_1{}'' + \mathrm{G}_1{}'''); \ \&\mathrm{c.}$$

This formula is identical with that which I employed in my former paper, to represent the pressure of an imperfect gas, and which I found to agree with M. Regnault's experiments, when the coefficients A and the function $f(\mathrm{V})$ had been calculated empirically[†].

[To be continued.]

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[*] The values of N for atmospheric air, oxygen, hydrogen, and carbonic oxide, differ from 0·41 by quantities which are within the limits of errors of observation. (1853.) See also Phil. Mag. June 1853, and Trans. Roy. Soc. Edinb. vol. xx. p. 589.

[†] For further applications of equation (21), see the paper of Messrs. Joule and Thomson in the Phil. Trans. for 1854. (Sept. 1855.)

XLIX. *On the Preparation of Aluminium from Kryolite.*
By ALLAN DICK, *Esq.*[*]

IN the last Number of this Journal there was a translation of a paper of H. Rose of Berlin, describing a method of preparing aluminium from kryolite. Previously, at the suggestion of Dr. Percy, I had made some experiments on the same subject in the metallurgical laboratory of the School of Mines; and as the results obtained agree closely with those of Rose, it may be interesting to give a short account of them now, though no detailed description was published at the time, a small piece of metal prepared from kryolite having simply been shown at the weekly meeting of the Royal Institution (March 30, 1855), accompanied with a few words of explanation by Faraday.

Shortly after the publication of M. Deville's process for preparing the metal from chloride of aluminium, along with Mr. Smith I tried to make a specimen of the metal; but we found it a much more difficult thing to do than Deville's paper had led us to anticipate, and had to remain contented with a much smaller piece than we had hoped to obtain. It is, however, undoubtedly only a matter of time, skill and expense, to join successful practice to the details of M. Deville.

Whilst making these experiments, Dr. Percy had often requested us to try whether kryolite could not be used instead of the chlorides, but some time elapsed before we obtained a specimen of the mineral.

The first experiments were made in glass tubes sealed at one end, into which alternate layers of finely-pounded kryolite and sodium cut into small pieces were introduced, and covered in some instances by a layer of kryolite, in others by one of chloride of sodium. The tube was then heated over an air-gas blowpipe for a few minutes, till decomposition had taken place and the product was melted. When cold, on breaking the tube it was found that the mass was full of small globules of aluminium; but owing to the specific gravity of the metal and flux being nearly alike, the globules had not collected into one button at the bottom: to effect this, long-continued heat would be required, which cannot be given in glass tubes, owing to the powerful action of the melted fluoride upon them. To obviate this difficulty, a platinum crucible was lined with magnesia, by ramming the same hard in, and subsequently cutting out all but a lining. Into this, alternate layers of kryolite and sodium were placed, with a thickish layer of kryolite on the top. The crucible was covered with a tight-fitting lid, and heated to redness for about half an hour over the air-blowpipe. When cold, it was placed in water, and after soaking for some time, the contents dug out, gently crushed in a mortar, and washed by decantation. Two or three globules of aluminium, tolerably large considering the scale of the experiment, were obtained along with a great number of very small ones. The larger ones were melted together under chloride of potassium.

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[*] Communicated by the Author.

Some experiments made in iron crucibles were not attended with the same results as those of Rose. No globules of any considerable size remained in the melted fluorides; the metal seemed to alloy with the sides of the crucibles, which acquired a colour like zinc. It is possible that this difference may have arisen from using a higher temperature than Rose, as these experiments were made in the furnace, not over the air-blowpipe.

Porcelain and clay crucibles were also tried, but laid aside after a few experiments owing to the action of the fluorides upon them, which in most cases was sufficient to perforate them completely.

These remarks, it is hoped, may tend to draw the attention of experimenters still further to kryolite as a source of this interesting metal; although the experiments which led to them were carried only a short way towards the perfection which practice would probably confer.

Metallurgical Laboratory, School of Mines,
October 26, 1855.

L. *On the Potash and Soda Felspars of the Dublin Mountains.*
By Francis M. Jennings, *M.R.I.A., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE paper of the Rev. Prof. Galbraith in the August Number of your Journal, relative to the composition of the felspars of the Dublin Mountains, shows that he entirely mistakes the object of my remarks, which were to this effect: that when an inference as to the composition of the felspar of a granite mountain is to be made, the specimen analysed should partake as closely as possible of the general lithological character of the mountain. Now the specimen selected by Mr. W. Wilson of the Geological Survey, and carefully analysed by Mr. Dugald Campbell, is very similar in its composition to those examined by Prof. England and Mr. Foster (see Proc. Royal Irish Acad. Feb. 28, 1853), whilst the analyses of the Rev. Prof. Galbraith are very different; he, therefore, is not in a position to charge others with want of care in the analyses, nor is he correct in saying, that to estimate the quantities of potash or soda in felspar it is necessary to determine the quantities of silica, &c.

	Prof. Galbraith (Phil. Mag. Jan. 1855), his own selection.	Prof. Galbraith, off same specimen as Mr. Campbell's.
KO . . .	10·68	4·65
NaO . . .	3·26	3·39

	Mr. Campbell, same specimen as Prof. Galbraith's.	Mr. Foster.	Prof. England.
KO . . .	3·02	4·15	5·72
NaO . . .	3·98	4·34	6·89

The difference in Prof. Galbraith's and Mr. Campbell's analyses is not owing, I presume, to any difference in the constitution of the felspars, but *most probably* to the Professor's not having taken out all the mica.

These analyses leave the question of the quantity of potash and soda in the felspar of the Three Rock Mountain thus: three analyses by three different men give the soda in excess; two by Prof. Galbraith give the potash in excess, that one his own selection giving the smallest quantity of soda. With this evidence before us, is it not rather premature to argue that this mountain contains in its felspar a greater quantity of potash than soda? And it shows the danger of making inferences as to the composition of minerals constituting a large portion of a mountain from one analysis, as done by Prof. Galbraith. How many it requires to be able to judge I shall not say, but I think those given, unless all selected specially as being like in general character, cannot have much bearing on the matter, and prove how much more is to be done before drawing conclusions from one analysis, and that one different from all the others. Further discussion, I think, must be useless, until Prof. Galbraith exhibits portions of the specimens which he has analysed; and my calling for their public exhibition is, I doubt not, the reason why my entering into this discussion is looked on by Prof. Galbraith as an "interference."

I am, Gentlemen,
Yours,
Francis M. Jennings.

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 309.]

April 19, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—

"Reply of the President and Council of the Royal Society to an application from the Lords of the Committee of Privy Council for Trade, on the subject of Marine Meteorological Observations."

[This Letter was communicated to the Society in pursuance of a resolution of the Council. The Secretary explained that it had been drawn up by the Treasurer, Colonel Sabine, and submitted, before final adoption by the Council, to several Fellows of the Society specially conversant with the subjects to which it refers.]

Royal Society, Somerset House,
February 22, 1855.

Sir,—In the month of June last, the Lords of the Committee of the Privy Council for Trade caused a letter to be addressed to the President and Council of the Royal Society, acquainting them that their Lordships were about to submit to Parliament an estimate for an Office for the Discussion of the Observations on Meteorology, to be made at sea in all parts of the globe, in conformity with the recommendation of a conference held at Brussels in 1853; and that they were about to construct a set of forms for the use of that Office, in which they proposed to publish from time to time and to circulate such statistical results obtained by means of the observations referred to, as might be considered most desirable by men learned in the science of Meteorology, in addition to such other information as might be required for the purposes of Navigation.

Before doing so, however, their Lordships were desirous of having the opinion of the Royal Society, as to what were the great desiderata in Meteorological science; and as to the forms which may be best calculated to exhibit the great atmospheric laws which it may be most desirable to develope.

Their Lordships further state, that as it may possibly happen that observations on land upon an extended scale may hereafter be made and discussed in the same Office, it is desirable that the reply of the Royal Society should keep in view, and provide for such a contingency.

Deeply impressed with a sense of the magnitude and importance of the work which has been thus undertaken by Her Majesty's Government and confided to the Board of Trade, and fully appreciating the honour of being consulted, and the responsibility of the reply which they are called upon to make;—considering also that by including the contingency of *land* observations, the inquiry is, in fact, co-extensive with the requirements of Meteorology over all accessible parts of the earth's surface,—the President and Council of the Royal Society deemed it advisable, before making their reply, to obtain the opinion of those amongst their foreign members who are known as distinguished cultivators of Meteorological science, as well as of others in foreign countries, who either hold offices connected with the advancement of Meteorology, or have otherwise devoted themselves to this branch of science.

A circular was accordingly addressed to several gentlemen whose names were transmitted to the Board of Trade in June last, containing a copy of the communication from the Board of Trade, and a request to be favoured with any suggestions which might aid Her Majesty's Government in an undertaking which was obviously one of general concernment.

Replies in some degree of detail have been received from five of these gentlemen*, copies of which are herewith transmitted.

The President and Council are glad to avail themselves of this opportunity of expressing their acknowledgements to these gentlemen, and more particularly to Professor Dove, Director of the Meteorological Establishments and Institutions in Prussia, whose zeal for the advancement of Meteorology induced him to repair personally to England, and to join himself to the Committee by whom the present reply has been prepared. Those who are most familiar with the labours and writings of this eminent meteorologist will best be able to appreciate the value of his co-operation.

The President and Council have considered it as the most convenient course to divide their reply under the different heads into which the subject naturally branches. But before they proceed to treat of these, they wish to remark generally, that one of the chief impediments to the advancement of Meteorology consists in the very slow progress which is made in the transmission from one country to another of the observations and discussions on which, under the fostering aid of different Governments, so much labour is bestowed in Europe and America; and they would therefore recommend that such steps as may appear desirable should be taken by Her Majesty's Government, to promote and facilitate the mutual interchange of Meteorological publications emanating from the Governments of different countries.

Barometer.

It is known that considerable differences, apparently of a permanent character, are found to exist in the mean barometric pressure in different places; and that the periodical variations in the pressure in different months and seasons at the same place, are very different in different parts of the globe, both as respects period and amount; insomuch that in extreme cases, the variations have even opposite features in regard to period, in places situated in the same hemisphere and at equal distances from the equator.

For the purpose of extending our knowledge of the facts of these departures from the state of equilibrium, and of more fully investigating the causes thereof, it is desirable to obtain, by means of barometric observations strictly comparable with each other, and extending over all parts of the globe accessible by land or sea, *tables*, showing the mean barometric pressure *in the year*, *in each month of the year*, and *in the four meteorological seasons*,—on land, at all stations of observation,—and at sea, corresponding to the middle points of spaces bounded by geographical latitudes and longitudes, not far distant from each other.

The manner of forming such tables from the marine observations which are now proposed to be made, by collecting together observations of the same month in separate ledgers, each of which should correspond to a *geographical space* comprised between specified meridians and parallels, and to a *particular month*, is too obvious to require to be further dwelt upon. The distances apart of the meridians and parallels will require to be varied in different parts of the globe, so that the magnitudes of the spaces which they enclose, and for each of which a table will be formed, may be more circumscribed, when the rapidity of the variation of the particular phenomenon to be elucidated is greatest in regard to geographical space. Their magnitude will also necessarily vary with the number of observations which it may be possible to collect in each space, inasmuch as it is well known that there are extensive portions of the ocean which are scarcely ever traversed by ships, whilst other portions may be viewed as the highways of a constant traffic.

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* Dr. Erman of Berlin; Dr. Heis of Münster; Prof. Kreil of Vienna; Lieut. Maury of Washington; and M. Quetelet of Brussels.

The strict comparability of observations made in different ships may perhaps be best assured, by limiting the examination of the instruments to comparisons which it is proposed to make at the Kew Observatory, before and after their employment in particular ships. From the nature of their construction, the barometers with which Her Majesty's navy and the mercantile marine are to be supplied are not very liable to derangement, except from such accidents as would destroy them altogether. Under present arrangements they will all be carefully compared at Kew before they are sent to the Admiralty or to the Board of Trade; and similar arrangements may easily be made by which they may be returned to Kew for re-examination at the expiration of each tour of service. The comparison of barometers when embarked and in use, with standards, or supposed standards, at ports which the vessels may visit, entails many inconveniences, and is in many respects a far less satisfactory method. The limitation here recommended is not, however, to be understood as applicable in the case of other establishments than Kew, where a special provision may be made for an equally careful and correct examination.

At land stations, in addition to proper measures to assure the correctness of the barometer and consequent comparability of the observations, care should be taken to ascertain by the best possible means (independently of the barometer itself), the height of the station above the level of the sea at some stated locality. For this purpose the extension of levels for the construction of railroads will often afford facilities.

It may be desirable to indicate some of the localities where the data, which tables such as those which have been spoken of would exhibit, are required for the solution of problems of immediate interest.

1°. It is known, that, over the Atlantic Ocean, a low mean annual pressure exists near the equator, and a high pressure at the north and south borders of the torrid zone (23° to 30° north and south latitudes); and it is probable that from similar causes similar phenomena exist over the corresponding latitudes in the Pacific Ocean: the few observations which we possess are in accord with this supposition; but the extent of space covered by the Pacific is large and the observations are few; they may be expected to be greatly increased by the means now contemplated. But it is particularly over the Indian Ocean, both at the equator and at the borders of the torrid zone, that the phenomena of the barometric pressure, not only annual but also monthly, require elucidation by observations. The Trade-winds, which would prevail generally round the globe if it were wholly covered by a surface of water, are interrupted by the large continental spaces in Asia and Australia, and give place to the phenomena of monsoons, which are the indirect results of the heating action of the sun's rays on those continental spaces. These are the causes of that displacement of the trade-winds, and substitution of a current flowing in another direction, which occasion the atmospheric phenomena over the Indian Ocean, and on the north and south sides of that Ocean, to be different from those in corresponding localities over, and on either side of the equator in the Atlantic Ocean, and (probably generally also) in the Pacific Ocean.

It is important alike to navigation and to general science to know the limits where the phenomena of the trade-winds give place to those of the monsoons; and whether any and what variations take place in those limits in different parts of the year. *The barometric variations are intimately connected with the causes of these variations, and require to be known for their more perfect elucidation.*

The importance, indeed, of a full and complete knowledge of the variations which take place in the limits of the trade-winds generally in both hemispheres, at different seasons of the year, has long been recognized. On this account, although the present section is headed "Barometer," it may be well to remark here, that it is desirable that the forms supplied to ships should contain headings, calling forth a special record of the latitude and longitude where the trade-wind is first met with, and where it is first found to fail.

2°. The great extent of continental space in Northern Asia causes, by reason of the great heat of the summer and the ascending current produced thereby, a remarkable diminution of atmospheric pressure in the summer months, extending in the north to the Polar Sea, and on the European side as far as Moscow. Towards the east it is known to include the coasts of China and Japan, but the extent of this great diminution of summer pressure beyond the coasts thus named is not known. A determination of the monthly variation of the pressure over the adjacent parts of the Pacific Ocean is therefore a desideratum; and for the same object it is desirable to have a more accurate knowledge than we now possess of the prevailing direction of the wind in different seasons in the vicinity of the coasts of China and Japan.

3°. With reference to regions or districts of increased or diminished *mean annual* pressure, it is known that in certain districts in the temperate and polar zones, such as in the vicinity of Cape Horn extending into the antarctic polar Ocean, and in the vicinity of Iceland, the mean annual barometric pressure is *considerably* less than the average pressure on the surface of the globe generally; and that anomalous differences, also of considerable amount, exist in the mean annual pressure in different parts of the arctic ocean. These all require special attention, with a view to obtain a more perfect knowledge of the facts, in regard to their amount, geographical extension, and variation with the change of seasons, as well as to the elucidation of their causes.

Dry Air and Aqueous Vapour.

The apparently anomalous variations which have been noticed to exist in the mean annual barometric pressure, and in its distribution in the different seasons and months of the year, are also found to exist in each of the two constituent pressures which conjointly constitute the barometric pressure. In order to study the problems connected with these departures from a state of equilibrium under their most simple forms,—and generally for the true understanding of almost all the great laws of atmospheric change,—it is necessary to have a separate knowledge of the two constituents (viz. the pressures of the dry air and of the aqueous vapour) which we are accustomed to measure together by the barometer. This separate knowledge is obtained by means of the hygrometer, which determines the elasticity of the vapour, and leads to the determination of that of the dry air, by enabling us to deduct the elasticity of the vapour from that of the whole barometric pressure. It is therefore extremely desirable that tables, similar to those recommended under the preceding head of the barometer, should be formed at every land station, and over the ocean at the centres of geographical spaces bounded by certain values of latitude and longitude, for the *annual*, *monthly*, and *season* pressures,—1. Of the aqueous vapour; and 2. Of the dry air; each considered separately. Each of the said geographical spaces will require its appropriate ledger for each of the twelve months.

It may be desirable to notice one or two of the problems connected with extensive and important atmospherical laws which may be materially assisted by such tables.

1°. By the operation of causes which are too well known to require explanation here, the dry air should always have a minimum pressure in the hottest months of the year. But we know that there are places where the contrary prevails, namely, that the pressure of the dry air is greater in summer than in winter. We also know that when comparison is made between places in the same latitude, and having the same, or very nearly the same, differences of temperature in summer and in winter, the differences between the summer and winter pressures of the dry air are found to be subject to many remarkable anomalies. The variations in the pressure of the dry air do not therefore, as might be at first imagined, depend altogether on the differences between the summer and winter temperatures at the places where the variations themselves occur. The increased pressure in the hottest months appears rather to point to the existence of an overflow of air in the higher regions of the atmosphere from *lateral sources*; the statical pressure at the base of the column being increased by the augmentation of the superincumbent mass of air arising from an influx in the upper portion. Such lateral sources may well be supposed to be due to *excessive ascensional currents* caused by *excessive summer heats* in certain places of the globe (as, for example, in Central Asia). Now the lateral overflow from such sources, traversing in the shape of currents the higher regions of the atmosphere, and encountering the well-known general current flowing from the equator towards the pole, has been recently assigned with considerable probability (derived from its correspondence with many otherwise anomalous phenomena already known, and which all receive an explanation from such supposition) to be the original source or primary cause of the *rotating storms* or *cyclones*, so well known in the West Indies and in China under the names of hurricanes and typhoons. A single illustration may be desirable. Let it be supposed that such an excessive ascensional current exists over the greatly heated parts of Asia and Africa in the northern tropical zone,—giving rise, in the continuation of the same zone over the Atlantic Ocean, to a lateral current in the upper regions; this would then be a current prevailing in those regions from east to west: and it would encounter over the Atlantic Ocean the well-known upper current proceeding from the equator towards the pole, which is a current from the south-west. An easterly current impinging on a south-west current may give rise, by well-known laws, to a rotatory motion in the atmosphere, of which the direction may be the same as that which characterises the cyclones of the northern hemisphere. To test the accuracy of this explanation, we desire to be acquainted with the variations which the *mean pressure of the dry air undergoes in the different seasons* in the part of the globe, where, according to this explanation, considerable variations having particular characters ought to be found.

2°. We have named one of the explanations which have been recently offered of the primary cause of the northern cyclones. Another mode of explanation has been proposed, by assuming the condensation of large quantities of vapour, and the consequent influx of air to supply the place. In such case the phenomena are to be tested in considerable measure by the variations which the *other constituent* of the barometric pressure, namely, the *aqueous vapour*, undergoes.

3°. The surface of sea in the southern hemisphere *much* exceeds that in the northern hemisphere. It is therefore probable that at the season when the sun is over the southern hemisphere, evaporation over the whole surface of the globe is more considerable than in the opposite season when the sun is over the northern hemisphere. Supposing the pressure of the dry air to be a constant, the difference of evaporation in the two seasons may thus produce for the whole globe an *annual barometric variation*, the aggregate barometric pressure over the *whole* surface being highest during the northern winter. The separation of the barometric pressure into its two constituent pressures would give direct and conclusive evidence of the cause to which such a barometric variation should be ascribed. It would also follow, that evaporation being greatest in the south, and condensation greatest in the north, the water which proceeds from south to north in a state of vapour, would have to return to the south in a liquid state, and might possibly exert some discernible influence on the currents of the ocean. The tests by which the truth of the suppositions thus advanced may be determined, are the variations of the meteorological elements in different seasons and months, determined by methods and instruments strictly comparable with each other, and arranged in such tables as have been suggested. A still more direct test would indeed be furnished by the fact (if it could be ascertained), that the quantity of rain which falls in the northern is greater than that which falls in the southern hemisphere; and by examining its distribution into the different months and seasons of its occurrence. Data for such conclusions are as yet very insufficient; they should always, however, form a part of the record at all land stations where registers are kept.

In order that all observations of the elasticity of the aqueous vapour may be strictly comparable, it is desirable that all should be computed by the same tables; those founded upon the experiments of MM. Regnault and Magnus may be most suitably recommended for this purpose, not only on their general merits, but also as being likely to be most generally adopted by observers in other countries.

Temperature of the Air.

Tables of the mean temperature of the air in the year, and in the different months and seasons of the year, at above 1000 stations on the globe, have recently been computed by Professor Dove, and published under the auspices of the Royal Academy of Sciences at Berlin. This work,—which is a true model of the method in which a great body of meteorological facts, collected by different observers and at different times, should be brought together and coordinated,—has conducted, as is well known, to conclusions of very considerable importance in their bearing on climatology, and on the general laws of the distribution of heat on the surface of the globe. These tables have, however, been formed exclusively from observations made *on land*. For the completion of this great work of physical geography, there is yet wanting a similar investigation for the *oceanic* portion; and this we may hopefully anticipate as likely to be now accomplished by means of the marine observations about to be undertaken. In the case of the temperature of the air, as in that of the atmospheric pressure previously adverted to, the centres of geographical spaces bounded by certain latitudes and longitudes will form points of concentration for observations, which may be made within those spaces, not only by the same but also by different ships; provided that the system be steadily maintained of employing only instruments which shall have been examined, and their intercomparability ascertained, by a competent and responsible Authority;—and provided that no observations be used but those in which careful attention shall have been given to the precautions which it will be necessary to adopt, for the purpose of obtaining the correct knowledge of the temperature of the external air, amidst the many disturbing influences from heat and moisture so difficult to escape on board ship. In this respect additional precautions must be used if ***night observations*** are to be required, since the ordinary difficulties are necessarily much enhanced by the employment of artificial light. Amongst the instructions which will be required, perhaps there will be none which will need to be more carefully drawn, than those for obtaining the correct temperature of the external air under the continually varying circumstances that present themselves on board ship.

In regard to ***land stations***, Professor Dove's tables have shown that data are still pressingly required from the British North American possessions intermediate between the stations of the Arctic Expeditions and those of the United States; and that the deficiency extends across the whole North American Continent in those latitudes from the Atlantic to the Pacific. Professor Dove has also indicated as desiderata, observations at the British Military stations in the Mediterranean (Gibraltar, Malta and Corfu), and around the Coasts of Australia and New Zealand: also that ***hourly*** observations, continued for at least one year, are particularly required at some one station in the West Indies, to supply the diurnal corrections for existing observations.

Whilst the study of the distribution of heat at the surface of the globe has thus been making progress, in respect to the ***mean annual temperature*** in different places, and to its ***periodical variations*** in different parts of the year at the same place, the attention of physical geographers has recently been directed (and with great promise of important results to the material interests of men as well as to general science) to the causes of those fluctuations in the temperature, or departures from its mean or normal state at the same place and at the same period of the year, which have received the name of "non-periodic variations." It is known that these frequently affect extensive portions of the globe at the same time; and are generally, if not always, accompanied by a fluctuation of an opposite character, prevailing at the same time in some adjoining but distant region; so that by the comparison of synchronous observations a progression is traceable, from a locality of maximum increased heat in one region, to one of maximum diminished heat in another region. For the elucidation of the non-periodic variations even ***monthly*** means are insufficient; and the necessity has been felt of computing the mean temperatures for periods of much shorter duration. The Meteorological Institutions of those of the European States which have taken the foremost part in the prosecution of meteorology, have in consequence adopted ***five-day means***, as the most suitable intermediate gradation between daily and monthly means: and as an evidence of the conviction which is entertained of the value of the conclusions to which this investigation is likely to lead, it has been considered worth while to undertake the prodigious labour of calculating the five-day means of the most reliable existing observations during a century past. This work is already far advanced; and it cannot be too strongly recommended, that at all fixed stations, where observations shall hereafter be made with sufficient care to be worth recording, five-day means may invariably be added to the daily, monthly, and annual means into which the observations are usually collected. The five-day means should always commence with January 1, for the purpose of preserving the uniformity at different stations, which is essential for comparison: in leap years, the period which includes the 29th of February will be of six days.

In treating climatology as a *science*, it is desirable that some correct and convenient mode should be adopted, for computing and expressing the *comparative variability* to which the temperature in different parts of the globe, and in different parts of the year in the same place, is subject from non-periodic causes. The *probable variability*, computed on the same principle as the *probable error* of each of a number of independent observations, has recently been suggested as furnishing an index "of the probable daily non-periodic variation" at the different seasons of the year; and its use in this respect has been exemplified by calculations of the "index" from the five-day means of twelve years of observations at Toronto, in Canada (Phil. Trans. 1853, Art. V.). An index of this description is of course of absolute and general application; supplying the means of comparing the probable variability of the temperature in different seasons at *different places* (where the same method of computation is adopted) as well as at the *same place*. It is desirable that this (or some preferable method if such can be devised for obtaining the same object) should be adopted by those who may desire to make their observations practically useful for sanitary or agricultural purposes, or for any of the great variety of objects for which climatic peculiarities are required to be known. Having these three data, viz. the mean annual temperature,—its periodical changes in respect to days, months, and seasons,—and the measure of its liability to non-periodic (or what would commonly be called, irregular) variations,—we may consider that we possess as complete a representation of the climate of any particular place (so far as temperature is concerned), as the present state of our knowledge permits.

It is obvious that much of what has been said under this article is more applicable to land than to sea observations; but the letter of the Board of Trade, to which this is a reply, requests that both should be contemplated.

Temperature of the Sea, and Investigations regarding Currents.

It is unnecessary to dwell on the practical importance to *navigation* of a correct knowledge of the currents of the ocean; their direction, extent, velocity, and the temperature of the surface-water relatively to the ordinary ocean temperature in the same latitude; together with the variations in all these respects which currents experience in different parts of the year, and in different parts of their course. As the information on these points, which may be expected to follow from the measures adopted by the Board of Trade, must necessarily depend in great degree on the *intelligence*, as well as the *interest* taken in them by the observers, it is desirable that the instructions to be supplied with the meteorological instruments should contain a brief summary of what is already known in regard to the principal oceanic currents; accompanied by charts on which their supposed limits in different seasons, and the variations in those limits which may have been observed in particular years, may be indicated, with notices of the particularities of the temperature of the surface-water by which the presence of the current may be recognized. Forms will also be required for use in such localities, in which the surface temperatures may be recorded at hourly or half-hourly intervals, with the corresponding geographical positions of the ship, as they may be best inferred from observation and reckoning. For such localities also it will be necessary that the tables, into which the observations of different ships at different seasons are collected, should have their bounding lines of latitude and longitude brought nearer together than may be required for the ocean at large.

In looking forward to the results which are likely to be obtained by the contemplated marine observations, it is reasonable that those which may bear practically on the interests of navigation should occupy the first place; but, on the other hand, it would not be easy to over-estimate the advantages to physical geography, of general tables of the surface temperature of the ocean in the different months of the year, exhibiting, as they would do, its normal and its abnormal states, the mean temperature of the different parallels, and the deviations therefrom, whether permanent, periodical, or occasional. The knowledge which such tables would convey is essentially required for the study of climatology *as a science*.

The degree in which climatic variations extending over large portions of the earth's surface may be influenced by the variable phenomena of oceanic currents in different years, may perhaps be illustrated by circumstances of known occurrence in the vicinity of our own coasts. The admirable researches of Major Rennell have shown that in ordinary years, the warm water of the great current known by the name of the Gulf-stream is not found to the east of the meridian of the Azores; the sea being of ordinary ocean temperature for its latitude at all seasons and in every direction, in the great space comprised between the Azores, and the coasts of Europe and North Africa: but Major Rennell has also shown that on two occasions, viz. in 1776 and in 1821–1822, the warm water by which the Gulf-stream is characterised throughout its whole course (*being several degrees* above the ordinary ocean temperature in the same latitude), was found to extend across this great expanse of ocean, and in 1776 (in particular) was traced (by Dr. Franklin) quite home to the coast of Europe. The presence of a body of unusually heated water, extending for several hundred miles both in latitude and in longitude, and continuing for several weeks, at a season of the year when the prevailing winds blow from that quarter on the coasts of England and France, can scarcely be imagined to be without a considerable influence on the relations of temperature and moisture in those countries. In accordance with this supposition, we find in the Meteorological Journals of the more recent period (which are more easily accessible), that the state of the weather in November and December 1821 and January 1822 was so unusual in the southern parts of Great Britain and in France, as to have excited general observation; we find it characterised as "most extraordinarily hot, damp, stormy, and oppressive," that "the gales from the W. and S.W. were almost without intermission," "the fall of rain was excessive" and "the barometer lower than it had ever been known for 35 years before."

There can be little doubt that Major Rennell was right in ascribing the unusual extension of the Gulf-stream in particular years to its greater initial velocity, occasioned by a more than ordinary difference in the levels of the Gulf of Mexico and of the Atlantic in the preceding summer. An unusual height of the Gulf of Mexico at the head of the stream, or an unusual velocity of the stream at its outset in the Strait of Florida, are facts which may admit of being recognized by properly directed attention; and as these must precede, by many weeks, the arrival of the warm water of the stream at above 3000 miles' distance from its outset, and the climatic effects thence resulting, it might be possible to anticipate the occurrence of such unusual seasons upon our coasts.

Much, indeed, may undoubtedly be done towards the increase of our partial acquaintance with the phænomena of the Gulf-stream, and of its counter currents, by the collection and coordination of observations made by casual passages of ships in different years and different seasons across different parts of its course; but for that full and complete knowledge of all its particulars, which should meet the maritime and scientific requirements of the period in which we live, we must await the disposition of Government to accede to the recommendation, so frequently made to them by the most eminent hydrographical authorities, of a specific survey of the stream by vessels employed for that special service. What has been recently accomplished by the Government of the United States in this respect, shows both the importance of the inquiry and the great extent of the research; and lends great weight to the proposition, which has been made to Her Majesty's Government on the part of the United States, for a joint survey of the whole stream by vessels of the two countries. The establishment of an office under the Board of Trade specially charged with the reduction and coordination of such data may materially facilitate such an undertaking.

Storms or Gales.

It is much to be desired both for the purposes of navigation and for those of general science, that the captains of Her Majesty's ships and masters of merchant vessels should be correctly and thoroughly instructed in the methods of distinguishing *in all cases* between the rotatory storms or gales, which are properly called *Cyclones*, and gales of a more ordinary character, but which are frequently accompanied by a veering of the wind, which under certain circumstances might easily be confounded with the phenomena of Cyclones, though due to a very different cause. It is recommended therefore that the instructions, proposed to be given to ships supplied with meteorological instruments, should contain clear and simple directions for distinguishing *in all cases* and *under all circumstances* between these two kinds of storms; and that the forms to be issued for recording the meteorological phenomena during great atmospheric disturbances should comprehend a notice of all the particulars which are required for forming a correct judgment in this respect.

Thunder-storms.

It is known that in the high latitudes of the northern and southern hemispheres thunder-storms are almost wholly unknown; and it is believed that they are of very rare occurrence over the ocean in the middle latitudes when distant from continents. By a suitable classification and arrangement of the documents which will be henceforward received by the Board of Trade, statistical tables may in process of time be formed, showing the comparative frequency of these phenomena in different parts of the ocean, and in different months of the year.

It is known that there are localities on the globe where, during certain months of the year, thunder-storms may be considered as a periodical phenomenon of daily occurrence. In the Port Royal Mountains in Jamaica, for example, thunder-storms are said to take place *daily* about the hour of noon from the middle of November to the middle of April. It is much to be desired that a full and precise account of such thunder-storms, and of the circumstances in which they appear to originate, should be obtained.

In recording the phenomena of thunder and lightning it is desirable to state the duration of the interval between the flashes of lightning and the thunder which follows. This may be done by means of a seconds-hand watch, by which the time of the apparition of the flash, and of the commencement (and of the conclusion also) of the thunder may be noted. The interval between the flash, and the commencement of the thunder, has been known to vary in different cases, from less than a single second to between 40 and 50 seconds, and even on very rare occasions to exceed 50 seconds. The two forms of ordinary lightning, viz. zigzag (or forked) lightning, and sheet lightning, should always be distinguished apart; and particular attention should be given both to the observation and to the record, in the rare cases when zigzag lightning either bifurcates, or returns upwards. A special notice should not fail to be made when thunder and lightning, or either separately, occur in a perfectly cloudless sky. When globular lightning (balls of fire) is seen, a particular record should be made of all the attendant circumstances. These phenomena are known to be of the nature of lightning, from the injury they have occasioned in ships and buildings that have been struck by them; but they differ from ordinary lightning not only by their globular shape, but by the length of time they continue visible, and by their slow motion. They are said to occur sometimes without the usual accompaniments of a storm, and even with a perfectly serene sky. Conductors are now so universally employed in ships, that it may seem almost superfluous to remark that, should a ship be struck by lightning, the most circumstantial account will be desirable of the course which the lightning took, and of the injuries it occasioned; or to remind the seaman that it is always prudent, after such an accident has befallen a ship, to distrust her compasses until it has been ascertained that their direction has not been altered. Accidents occurring *on land* from lightning will, of course, receive the fullest attention from meteorologists who may be within convenient distance of the spot.

Auroras and Falling Stars.

Auroras are of such rare occurrence in seas frequented by ships engaged in commerce, that it may seem superfluous to give any particular directions for their observation *at sea*; and land observatories are already abundantly furnished with such. It is, of course, desirable that the meteorological reports received from ships should always contain a notice of the time and place where Auroras may be seen, and of any remarkable features that may attract attention.

The letter from Professor Heis, which is one of the foreign communications annexed, indicates the principal points to be attended to in the instructions which it may be desirable to draw up for the observation of "Falling Stars." For directions concerning Halos and Parhelia, a paper by Monsieur Bravais in the 'Annuaire Météorologique de la France' for 1851, contains suggestions which will be found of much value.

Charts of the Magnetic Variation.

Although the variation of the compass does not belong in strictness to the domain of meteorology, it has been included, with great propriety, amongst the subjects treated of by the Brussels Conference, and should not therefore be omitted here. It is scarcely necessary to remark, that whatever may have been the practice in times past, when the phenomena of the earth's magnetism were less understood than at present, it should in future be regarded as indispensable, that variation-charts should always be constructed for a *particular epoch*, and that *all parts* of the chart should show *the variation corresponding to the epoch for which it is constructed.* Such charts should also have, either engraved on the face or attached in some convenient manner, a table, showing the approximate annual rate of the secular change of the variation in the different latitudes and longitudes comprised: so that, by means of this table, the variation taken from the chart for any particular latitude and longitude may be corrected to the year for which it is required, if that should happen to be different from the epoch for which the chart is constructed.

A valuable service would be rendered to this very important branch of hydrography, if, under the authority of the new department of the Board of Trade, variation-charts for the North and South Atlantic Oceans, for the North and South Pacific Oceans, for the Indian Ocean, and for any other localities in which the requirements of navigation might call for them, were published at *stated intervals*, corrected for the secular change that had taken place since the preceding publication. Materials would be furnished for this purpose by the observations which are now intended to be made, supposing them to be collected and suitably arranged with proper references to date and to geographical position, and to the original reports in which the results and the data on which they were founded were communicated. By means of these observations the tables of approximate correction for secular change might also be altered from time to time as occasion should require, since the rate of secular change itself is not constant.

All observed variations, communicated or employed as data upon which variation-charts may be either constructed or corrected, should be accompanied by other observational data (the nature of which ought now to be well understood), for correcting the observed variation for the error of the compass occasioned by the ship's iron. It is also strongly recommended that no observations be received as data for the formation or correction of variation-charts, but such as are accompanied by a detailed statement of the principal elements both of observation and of calculation. Proper forms should be supplied for this purpose; or, what is still better, books of blank forms may be supplied, in which the observations themselves may be entered, and the calculation performed by which the results are obtained. Such books of blank forms would be found extremely useful both for the variation of the needle, and for the chronometrical longitude (as well as for lunar observations, if the practice of lunar observations be not, as there is too much reason to fear it is, almost wholly discontinued). By preparing and issuing books of blank forms suitable for these purposes, and by requesting their return in accompaniment with the other reports to be transmitted to the Board of Trade at the conclusion of a voyage, the groundwork would be laid for the attainment of greatly improved habits of accuracy in practical navigation in the British mercantile marine.

The President and Council are aware that they have not exhausted the subject of this reply in what they have thus directed me to address to you; but they think that perhaps they have noticed as many points as may be desirable for *present* attention; and they desire me to add, that they will be at all times ready to resume the consideration if required, and to supply any further suggestions which may appear likely to be useful.

I have the honour to be, Sir,
Your obedient Servant,
W. SHARPEY,
Sec. R.S.

The Secretary of the Lords of the Committee of Privy Council for Trade.

June 21.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

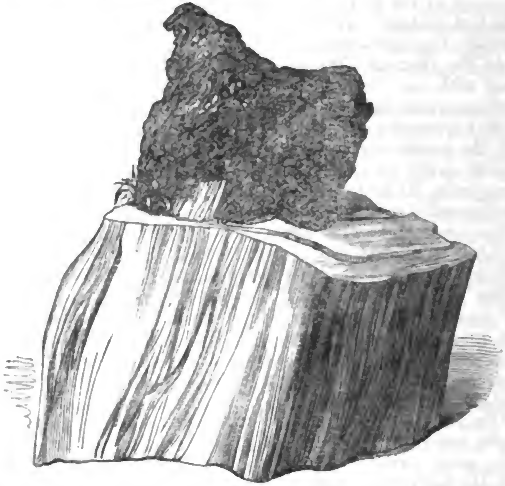
"On a supposed Aërolite or Meteorite found in the Trunk of an old Willow Tree in the Battersea Fields." By Sir Roderick Impey Murchison, F.R.S.

In bringing this notice before the Royal Society, it is unnecessary to recite, however briefly, the history of the fall of aërolites or meteorites, as recorded for upwards of three thousand years, though I may be pardoned for reminding my Associates, that the phenomenon was repudiated by the most learned academies of Europe up to the close of the last century. The merit of having first endeavoured to demonstrate the true character of these extraneous bodies is mainly due to the German Chladni (1794), but his efforts were at first viewed with incredulity. According to Vauquelin and other men of eminence who have reasoned on the phenomena, it was in 1802 only that meteorites obtained a due degree of consideration and something like a definite place in science through the studies of Howard, as shown in his memoir published in the Philosophical Transactions.

Vauquelin, Klaproth, and other distinguished chemists, including Berzelius and Rammelsberg, have successively analysed these bodies, and the result of their labours, as ably brought together in the work of the last-mentioned author, is, that whilst they have a great general resemblance and are distinguishable on the whole by their composition from any bodies found in the crust of the earth, each of their component substances is individually found in our planet. They are also peculiarly marked by the small number of minerals which have collectively been detected in any one of them; nickel and cobalt, in certain relations to iron, being the chief characteristics of the metallic meteorites.

Of the various theories propounded to account for the origin of these singular bodies, it would indeed ill become a geologist like myself to speak; and referring in the sequel to some of the various works in which the subject has been brought within formula, I will at once detail the facts connected with the discovery of this metalliferous body in the heart of a tree, as now placed before the Members of our Society, feeling assured that, whatever be their ultimate decision, my contemporaries will approve of the efforts that have been made to account for this singular and mysterious phenomenon.

On the 2nd of June, a timber merchant, residing at North Brixton, named Clement Poole, brought the specimen now exhibited to the Museum of Practical Geology, when it occurred to Mr. Trenham Reeks, our Curator, that it might be a meteorite, and on inspecting its position in the mass of wood, and having heard all the evidence connected with it, I was disposed to form the same conclusion. On submitting a small portion of the metallic part to a qualitative test in the metallurgical laboratory of our establishment, the presence of nickel, cobalt and manganese was detected in the iron included in the mass, and as the surface was scorified, indented, uneven, and partially coated with a peculiar substance, the surmise as to the meteoric nature of the imbedded material seemed to be rendered much more probable. Again, in looking at the wood which immediately surrounded that portion of the mass which remained, as it is now, firmly inserted in the tree, a blackened substance was observed to be interpolated between the supposed meteorite and the surrounding sound wood. On the outside of this substance (which had somewhat a charred aspect) we observed a true bark, which follows the sinuosities of the wood wherever the latter appears to have been influenced by the intrusion of the foreign mineral matter. [The specimen is represented in the annexed wood-cut.]



Seeing thus enough to satisfy our conjecture, if sanctioned by other evidence, I desired Mr. Poole to bring all the fragments of the wood he had not destroyed which surrounded this body. On placing the ends of some of these (also now exhibited) on the parts from which they had been sawed off, they indicated that the space between the mineral substance and the surrounding sound wood widened upwards; the decayed wood passing into brown earthy matter with an opening or cavity into which rootlets extended. On interrogating Mr. Poole, who cut down the tree and superintended the breaking up of its timber, I learnt from him all requisite particulars respecting its dimensions, the position of the ferruginous mass, the quantity of wood above and below it, a description of the place where the stool of the tree was still to be seen, and of the parties who, living on the spot, were acquainted with every circumstance which could throw light on the case.

At this period of the inquiry, the Museum in Jermyn Street was visited by Dr. Shepard, Professor in the University College, Amherst, United States, whose researches on meteorites are widely known, and who has furnished an able classification of them by which they are divided into the two great classes of stony and metallic. Having carefully examined the specimen, Dr. Shepard expressed his decided belief that it was a true meteorite, and the next day wrote to me the following account of it; at the same time referring me most obligingly to a series of interesting publications on the subject as printed in America and Europe*:—

"Concerning the highly interesting mineral mass, lately found enclosed in the trunk of a tree, and of which you have done me the honour to ask my opinion, I beg leave to observe, that I have no hesitation in pronouncing it to be a true meteoric stone.

"Aside from the difficulty of otherwise accounting for it, under the circumstances in which it is found, the mass presents those peculiar traits that are regarded as characteristic of meteorites. It has, for example, a fused, vitrified black coating, which is quite continuous over a considerable part of the mass, and contains several grains and imbedded nodular and vein-like portions of metallic iron, in which I understand nickel and cobalt have been detected.

"The general character of the body of the stone is indeed peculiar; and as a whole, unlike any one I have yet seen; it being principally made up of a dull greyish yellow, peridotic mineral, which I have nowhere met with among these productions, except in the Hommoney Creek meteoric iron mass, and which exists in it only in a very limited quantity. It is singular to remark also, that the stone under notice strikingly resembles in size, shape and surface, the iron above alluded to.

"The absence of the black, slaggy coating on one of the broad surfaces of the stone, may arise from its having been broken away, by the violence to which it must have been subjected in entering the tree; for it appears to have buried itself completely at its contact, an operation which would probably have been impossible, in the case of a stone, but for its wedge-shaped configuration, and the coincidence of one of its edges with the vertical fibres of the wood."

In reply to a question I subsequently put to Dr. Shepard as to whether he knew of any examples of meteorites having struck trees in America, he replied as follows :—

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* Dr. Shepard's numerous memoirs on meteorites are all to be found in the volumes of the American Journal of Science and Art, and in the same work the reader will find not only the general classification of these bodies by this author, who possesses a collection from 103 localities, but also essays on the same subject by his countrymen Dr. Troost, Professor Silliman, jun., and Dr. Clark.

In our own country, Mr. Brayley published some years ago a comprehensive view of this subject in the Philosophical Magazine, and recently Mr. Greg has in the same publication put together all the previous and additional materials, with tables showing the geographical distribution of meteorites. Among the well-recorded examples of the fall of metalliferous meteorites, no one is more remarkable than that which happened in the year 1851, about sixteen leagues S.E. of Barcelona in Spain. In describing that phenomenon, Dr. Joaquim Balcells, Professor of Natural Sciences at Barcelona, has illustrated the subject with much erudition, whilst his theoretical views are ingenious in his endeavour to explain how meteorites are derived from the moon.

"I think you will find in the volume I left with Mr. Reeks at the Museum, an account of the fall of Little Piney, Missouri, February 13th, 1839; in which it is stated that the stone struck a tree and was shattered to fragments, it being one of a brittle character. In the interior of the Cabarras county, N. Carolina, a stone (October 31, 1849) I know struck a tree, and I found it was difficult, indeed impossible, to separate completely the adhering woody fibres from the rough hard crust of the meteorite. The stone in this case is a peculiarly tough one, having a decidedly trappean character, rendering it as nearly infragile as cast iron."

Aware that some time must elapse before the precise analysis, which I wished to be made in the laboratory of Dr. Percy, could be completed, and that the last meeting of the Royal Society was to be held this evening, I announced the notice I am now communicating. At the same time I resolved to visit the locality where the tree stood, and to obtain on the spot all the details required. Having done so, accompanied by Mr. Robert Brown, Sir Philip Grey Egerton, Professor J. Nicol, and Mr. Trenham Reeks, the information ultimately obtained was as follows :—

The man who helped to cut down the tree confirmed in every respect the evidence of Mr. Poole as to its position, height, and dimensions, and pointed out to us the stump or stool we were in search of, which is to be seen at nearly 200 yards to the east of the St. George's Chapel, Lower Road, Battersea Fields, and at the eastern end of a nursery garden, between the railway and the road, occupied by Mr. Henry Shailer.

The tree was a large willow, probably about sixty years of age, which stood immediately to the east of the old parsonage house recently pulled down. Its stem measured about 10 feet in circumference at 3 feet above the ground, and had a length of between 9 and 10 feet; from its summit three main branches extended, one of which, pointing to the S.W. or W.S.W., had been for many years blighted, and was rotten to near its junction with the top of the main trunk; a portion of this blighted main branch is exhibited. The other two main branches, which rose to a height of 50 or 60 feet, were quite sound; a part of one of these offsets is also exhibited.

The stool of the tree was visibly perfect and without a flaw, and at the wish of Mr. R. Brown, a section of it has been obtained since our visit, which is also here, and the rings of which seem to confirm the supposition as to the age of the tree.

Mr. Poole having conveyed the tree to Brixton, cut the trunk into two nearly equal parts, intending to make cricket-bats out of each. In doing so, he perceived that the upper portion of the lower of the two segments was in a shaky or imperfect condition, and hence he resolved to saw off the upper part of it, intending thereby to obtain wood large enough for the "pods" of his cricket-bats, but not such entire bats as he was making out of the upper segment.

In dividing the tree, the saw was stopped at about 8 inches from the surface on one side (or the breadth of a large saw) by a very hard, impenetrable substance, which was supposed to be a nail, and hence Mr. Poole resolved to break up the portion of the wood he had previously condemned as of inferior quality, and hewing it down from the sides he uncovered, to his astonishment, the great lump of metalliferous matter, as now seen. Attaching little value to it, much of the surrounding wood was thrown away or used up before the specimen was brought to Jermyn Street; but enough has been obtained to throw light on the probable or possible origin of the included mass.

On interrogating Henry Shailer, a market gardener, who has long lived on the spot and managed the ground where the tree grew, when it was part of the garden of the former clergyman (Mr. Weddell), I learnt from him that he had known the spot for sixty years, that in his days of boyhood it was a fellmonger's yard, before it was attached to the garden. He had observed that the tree was blighted in one of its main branches for many years, and had always supposed that it was struck by lightning in one of two storms, the first of which happened about 1838 or 1839, the other about nine years ago.

So far the evidence obtained might be supposed to favour the theory that this ferruginous mass* had been discharged near to the blighted branch, and had penetrated downwards into the tree, to the position in which we now see it, charring and warping the wood immediately around it in its downward progress; whilst in the sixteen years which have elapsed, the wood renovating itself, produced the appearance which has so much interested the eminent botanists who have examined it, viz. Mr. R. Brown, Dr. Lindley, Professor Henfrey, Dr. J. Hooker, and Mr. Bennett.

On the other hand, I must now point out some features of this extraordinary case which check the belief in the included mass being a meteorite.

We found lying near the root of the tree two fragments, one of which is similar to the substance included in the tree, while the other is decidedly an iron slag. On bringing these fragments, weighing several pounds, to Jermyn Street, and on breaking one of them, it was found, like the supposed meteorite, to contain certain small portions of metallic iron, in which both nickel and cobalt were also present; and hence the scepticism which had prevailed from the beginning of the inquiry in the minds of some of my friends, was worked up into a definite shape.

The occurrence of stones enclosed in wood is not a novel phenomenon. Mr. Robert Brown has called my attention to two cases as recorded in the following works:—

"De Lapide in trunco Betulæ reperto. G. F. Richter in Acta Phys. Med. Acad. Nat. Curios. volume 3, page 66†."

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* The ferruginous mass is, it is supposed, about thirty pounds in weight; but as one of its extremities is still imbedded in the wood, the precise weight cannot be stated.

† "Lapis prædurus subalbicans et manifeste siliceus pruni ferme aut juglandis

"Descriptio Saxi in Quercu inventi. Kellander, Acta Literaria et Scientiæ Sueciæ." 1739, pp. 502, 503.

Since the Battersea phenomenon was announced, Professor Henslow, to whom I had applied, wrote to me saying, that he possessed a remarkable example of a stone which was found imbedded in the heart of a tree, in sawing it up in Plymouth Dockyard; and he has obligingly sent up the specimen, which is now also exhibited. In this case, judging from the mineral character of the rock, and its being slightly magnetic, Professor Henslow supposed that it was perhaps a volcanic bomb. On referring it to Dr. Shepard, that gentleman entertains the opinion that it is also a meteorite, and states that it resembles certain meteoric stones with which he is acquainted; suspicions of which had also been entertained by Professor Henslow, From the examination of a minute fragment which I detached from this stone, it appears to be composed of a base of felspathic matter, with minute crystals of felspar and of magnetic iron pyrites. Externally it has a trachytic aspect, though, when fractured, it more resembles, in the opinion of Mr. Warington Smyth, a pale Cornish elvan or porphyry than any other British rock with which it can be compared. Whatever may have been the origin of this stone, which is of the size of a child's head, it is essentially different from the metalliferous mass from Battersea, to which attention has been specially invited, and its position in the heart of an oak is equally remarkable. Like the Battersea specimen, the segment of wood from Plymouth Dockyard is characterised by an interior bark which folds round the sinuosities of the included stone.

In respect to the envelopment of manufactured materials in trees, my friend, Mr. H. Brooke, the distinguished mineralogist, tells me that he perfectly remembers the case of an iron chain which had been enclosed in the heart of a tree, the wood of which was sound around the whole of the included metallic body. This specimen was to be seen some years ago in the British Museum. Again, he informs me that at Stoke Newington he recollects to have seen a tree, the trunk of which had grown over and completely enclosed a scythe, except on the sides where its ends protruded*.

Whatever may have been the origin of the metalliferous mass from Battersea, its discovery has at all events served to develope certain peculiarities in the growth of plants which appear to be of high interest to the eminent botanists who have examined the parts of this tree which surrounded the supposed meteorite. Unwilling to endeavour to anticipate the final decision as to the origin of the body in question, I may be permitted to feel a satisfaction that its discoverer brought it to the Establishment of which I am the Director, and which numbers among its officers a Fellow of this Society, who is so

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minoris magnitudine. * * * * Nidus ad figuram lapidis non plane accommodatus, sed quadrangulus, et hinc illinc in mediocres rimas desinens, corticeque imprimis notabili, non multum ab exteriori cute diverso, maximam partem vestitus."

* Many other examples of extraneous bodies found enclosed in the heart of trees have been brought to my notice since this account was written. The most curious of these is perhaps that of an image of the Virgin, which having been placed in a niche had become imbedded by the growth of the tree around it.

well calculated, by his analytical researches, to settle the question on a permanent basis. Should the metallurgical analyses now under the conduct of Dr. Percy lead to the inevitable conclusion that the composition of this body is different from that of well-authenticated meteorites, and is similar to that of undoubted iron slags, we shall then have obtained proofs of the great circumspection required before we assign a meteoric origin to some of these crystalline iron masses, which though not seen to fall, have, from their containing nickel, cobalt and other elements, been supposed to be formed by causes extraneous to our planet.

Postscript, 30th June 1855.—The following are the analyses above referred to, which have been given to me by Dr. Percy since the preceding notice was read:—

"*The slag-like matter* (1) attached to the metal in the tree, as well as the similar matter (2) with adherent metal which was found by Mr. Reeks in the vicinity of the tree, has been analysed. The results are as follow:—

	No. 1.	No. 2.
Silica	58·70	63·52
Protoxide of iron	35·46	32·30
Lime	0·30	0·59
Magnesia	0·74	0·21
Protoxide of manganese	trace	trace
Alumina	3·40	2·85
Phosphoric acid	0·43	0·57
Sulphur as sulphide	trace	trace
	99·03	100·04

"No. 1. was analysed by Mr. Spiller, and No. 2. by Mr. A. Dick, chemists who have been incessantly engaged at the Museum during the last two years and a half in the analyses of the iron ores of this country, and whose great experience renders their results worthy of entire confidence. Cobalt and nickel were not sought for in either case, but the metallic iron enveloped in both specimens contained a minute quantity of cobalt and nickel. Another piece of slag-like matter, which was found on the ground near the tree, and which from its external characters I have no hesitation in pronouncing to be a slag, was examined for cobalt and nickel, and gave unequivocal evidence of the former in minute quantity, though not satisfactorily of the latter.

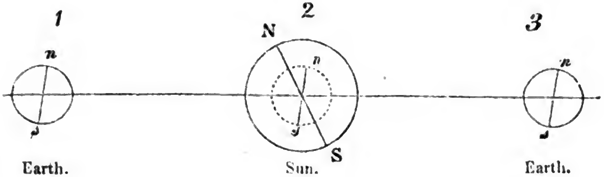
"The metal previously mentioned is malleable iron. That which was detached from the slag-like matter, found outside the tree, was filed and polished, and then treated with dilute sulphuric acid. After this treatment, the surface presented small, confused, irregularly-defined crystalline plates, and was identical in appearance with the surface of a piece of malleable iron similarly treated after fusion in a crucible."

Extract of a Letter from Professor Langberg of Christiania to Colonel Sabine, dated June 10, 1855.

"Of all the important results from the discussions of the British Colonial Observatory, the discovery of the *direct* action of the sun on the magnetism of the earth is certainly a fact of the highest interest, in opening quite a new field for investigation; and few modern discoveries in this branch of science have interested me more than yours of the annual variation of the diurnal variation of declination. It seems that M. Secchi of Rome has nearly touched at the same discovery, and I am indeed glad that the enormous quantity of calculations, which you are superintending, did not prevent you from publishing your results before the ripening fruit was plucked by another. The first beautiful result of this annual variation is the explanation of the fact, which you have deduced from the observations at St. Helena and the Cape of Good Hope, that the horary variation of declination does *not* vanish in passing from the northern to the southern magnetic hemisphere, but only changes signs at the equinoxes. I think every physicist will agree with you, that no thermic hypothesis will be able to explain this annual variation, but as you say (Toronto, ii. p. xix), it is 'obviously connected with, and dependent on, the earth's position in its orbit relatively to the sun, around which it revolves, as the diurnal variation is connected with the rotation of the earth on its axis.' But you have given no hint how this different position in its orbit can affect the magnetic condition of the earth, except so far as you suppose that the excentricity of the orbit is the reason that the total magnetic force is about $\frac{2}{1000}$ greater at the perihelion than at the aphelion (page xciii); but even granted that this variation is the effect of the excentricity, it cannot be the cause of the annual variation of the declination, as this is of contrary signs in the two semiannual periods.

"I have thought that this annual variation might possibly be explained by the following considerations, which I (although with extreme diffidence) shall venture to lay before you.

"As the recent magnetic observations have proved without doubt the *direct* magnetic action of the sun, or that the sun itself is a magnet, the sun must accordingly have magnetic polarity or magnetic poles. Now in our ignorance of the causes of the magnetic condition of the heavenly bodies, I think it reasonable to connect it in some way with their rotation on their axes, and to assume that generally their magnetic axis will nearly coincide with the axis of rotation; at all events, if these do not coincide, but include a small acute angle, the sum of the magnetic influences on a distant magnetic body during a whole rotation, will be nearly the same as if the magnetic poles were placed in the axis of rotation. If we suppose a magnet E revolving about another S, the magnetic axis of E remaining parallel with itself, but not parallel with the axis of S (as the earth around the sun), then the magnetic induction of S on E will depend on the relative position of both magnetic axes. Moreover, if we only regard the mean of the magnetic induction of the sun on the earth during several rotations of both on their axes, we may approximately assume that the magnetic axis of both coincides with the axis of rotation, and compare their relative position during a whole revolution or annual period, with the magnetic variation in question. Now, if a plane is laid through the sun's axis parallel with the axis of the earth, this plane intersects the ecliptic in two points, whose longitudes are 286° and 106°, and has an inclination of 83°·2, both axes forming an angle of 25°·8. Accordingly, the northerly prolongation of both axes will *converge* (as in 1 and 2) when the longitude is 286°, or about seventeen days after the summer solstice, and the southerly prolongation (as in 2 and 3) when the longitude of the earth is 106°, or sixteen days after the winter solstice. *About sixteen days after the equinoxes* the axes are in the position 2, and the radius vector forms the greatest angle with the above-named plane, viz. 83°·2. It is therefore evident, that the greatest magnetic induction takes place at the two solstices, but of opposite character, as the north poles converge in one, and the south poles in the other semiannual period: the change takes place *sixteen days after the equinoxes*, exactly as you have found by observation, and I regard this circumstance as very important evidence for my hypothesis, although you have shown that it could also be accounted for by the fact, that all magnetic induction takes some time ere it attains its maximum.



"If the sun's magnetic axis does not coincide with the axis of rotation, then I suppose we shall find, by more minute examination of the observations, that there exists also a small magnetic variation corresponding to the sun's geocentric rotation, or 27·68 days.

"There seems also to be strong reason to suppose that Hansteen's discovery about the annual periodic frequency of the aurora borealis, which has a marked maximum at the equinoxes, and even a more marked maximum at the solstices, is connected with the same cause."

LII. *Intelligence and Miscellaneous Articles.*

CONTRIBUTIONS TOWARDS THE KNOWLEDGE OF FLUORESCENT BODIES. BY G. WERTHER.

IN the following experiments a ray of light was thrown, by means of a heliostat, upon a prism provided with a lens, and the portion of the spectrum from the red to the violet was shut off. The substances to be examined should in this arrangement be held only in the violet and opake portion of the spectrum. It appeared, and indeed this is characteristic of fluorescent bodies, that all the violet was extinguished, and instead of this a yellowish or bluish-green light of greater or less intensity made its appearance. In the other parts of the spectrum these bodies exhibited the usual prismatic colours. The greater or less extent of the fluorescent part of the spectrum was always compared with some sulphate of quinine spread upon paper which was stretched over a glass plate. Some of the substances which are fluorescent in the solid form were also dissolved and laid upon paper. Protocyanide of potassium and platinum laid on to a sufficient thickness, appeared, with regard to the extent and distinctness of its projection of Fraunhofer's lines, as good as sulphate of quinine. Acetate of uranium and soda did not show fluorescence until its concentrated solution had been applied several times. Its solution also did not exhibit the epipolization (the bluish lustre) presented by solution of sulphate of quinine. Solution of protocyanide of potassium and platinum also does not exhibit this property when greatly diluted. The salts examined were,—

	Behaviour	
Compounds.	In the violet.	Behind the violet.
$U^2O^3, PO^5, 5HO$	Strongly yellowish-green.	Fluorescent greenish-yell.
$U^2O^3, AsO^5, 5HO$		
$(2U^2O^3+HO)PO^5+8HO$	Bluish-green.	Fluorescent greenish-yell.
$(2U^2O^3+HO)PO^5+6HO$	Very slight greenish-yellow.	Slight yellowish-green.
$(2U^2O^3+HO)PO^5+3HO$	No result.	
$(2U^2O^3+HO)PO^5$	Strongly yellowish-green.	Slight greenish-yellow.
$(2U^2O^3+HO)AsO^5+8HO$	No result.	Slight greenish-yellow.
$(2U^2O^3+HO)AsO^5+6HO$		
$(CaO+2U^2O^3)PO^5+8HO$	Strong sea-green.	Strong yellowish-green.
The same, as lime-uranite.		
$(CuO+2U^2O^3)PO^5+8HO$	No result.	
The same, as chalcolite.		
$(CuO+2U^2O^3)AsO^5+8HO$.	No result.	
$KO\bar{A}+2(U^2O^3\bar{A})+2HO$	No result.	Slight yellowish-green.
$NaO\bar{A}+2(U^2O^3\bar{A})$	Bluish-green.	Strong yellowish-green.
$(U^2O^3)^2, PO^5+(U^2O^3)^3, PO^5$	No result.	
$2(2U^2O^3, PO^5)+NaO, U^2O^3+3HO$		
$(NaO+2U^2O^3), AsO^5+5HO$		
U^2O^3, NO^5+6HO	Strong yellowish-green.	Strong greenish-yellow.

—*Journ. für Prakt. Chemie*, vol. lxv. p. 349.

ON THE ACETATES OF MANGANESE, CADMIUM, AND NICKEL.

BY M. C. VON HAUER.

The acetate of manganese assumes the form of rose-coloured rhomboidal lamellæ, generally with truncated edges; it contains four atoms of water, and is probably isomorphous with the acetate of magnesia. The acetate of cadmium is so difficult to obtain, that several chemists have denied its existence. It crystallizes in rather large prisms, with strongly brilliant planes, which M. Zepharovich has determined to belong to the augitic (hemiprismatic) system. The acetate of cadmium contains three atoms of water, and is identical in chemical character with the acetate of zinc. It is only obtained by leaving the acid solution to evaporate in absolute repose.

Acetate of nickel contains four atoms of water. Its crystallographic description has been recently given in M. Schabus's memoir, which obtained the prize offered by the Imperial Academy of Vienna. —*Proceedings of the Imperial Geological Institute of Vienna, March* 6, 1855.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1855.

Chiswick.—September 1. Clear and fine: cloudy. 2. Cloudy. 3. Clear: slight shower: fine. 4. Cloudy. 5. Fine: cloudy: clear and cold at night. 6. Clear: cloudy. 7. Very clear: cloudy: fine. 8. Dense fog: clear and very fine. 9. Very fine. 10. Foggy: fine: very clear. 11. Very fine. 12. Slight fog: very fine: clear: rain at night. 13. Rain. 14. Constant rain. 15. Uniformly overcast: fine. 16. Overcast: clear and fine. 17. Rain: hazy: overcast. 18. Hazy: overcast. 19. Slight haze: fine. 20—22. Foggy in the mornings: very fine. 23. Fine: cloudy: very clear. 24. Overcast. 25. Fine: cloudy: clear: frosty at night. 26. Slight haze: very dry air: frosty at night. 27. Foggy: very fine: cloudy. 28, 29. Cloudy: rain. 30. Hazy: cloudy: rain at night.

Mean temperature of the month	56°·11
Mean temperature of Sept. 1854	56 ·93
Mean temperature of Sept. for the last twenty-nine years	56 ·98
Average amount of rain in Sept.	2·503 inches.

Boston.—Sept. 1. Cloudy: rain P.M. 2—6. Cloudy. 7, 8. Fine. 9, 10. Cloudy. 11, 12. Fine. 13. Fine: rain A.M. 14—18. Cloudy. 19, 20. Fine. 21, 22. Cloudy. 23. Fine. 24. Fine: rain A.M. 25. Cloudy. 26, 27. Fine. 28. Fine: rain P.M. 29. Cloudy: rain A.M. 30. Cloudy.

Sandwick Manse, Orkney.—Sept. 1, 2. Bright A.M.: cloudy P.M. 3. Cloudy A.M. and P.M. 4. Bright A.M.: clear P.M. 5. Showers A.M.: clear P.M. 6. Drizzle A.M. and P.M. 7. Cloudy A.M.: showers P.M. 8. Clear A.M.: showers P.M. 9. Bright A.M.: showers P.M. 10. Cloudy A.M.: showers, aurora P.M. 11. Showers A.M.: cloudy, aurora P.M. 12. Rain A.M.: clear, aurora P.M. 13. Cloudy A.M.: clear P.M. 14. Cloudy, hoar-frost A.M.: clear, aurora P.M. 15. Bright, hoar-frost A.M.: cloudy P.M. 16. Showers A.M.: clear P.M. 17, 18. Showers A.M. and P.M. 19. Cloudy A.M.: drops P.M. 20. Cloudy A.M. and P.M. 21. Showers A.M.: drizzle P.M. 22. Damp A.M.: drizzle P.M. 23. Cloudy A.M.: clear, fine P.M. 24. Clear A.M.: fine, cloudy P.M. 25. Cloudy A.M.: fine, drops P.M. 26. Drops A.M.: fine, clear P.M. 27. Drops A.M.: cloudy P.M. 28. Drops A.M. and P.M. 29. Rain A.M.: fog P.M. 30. Fog A.M. and P.M.

Mean temperature of Sept. for twenty-eight previous years	52°·42
Mean temperature of this month	52 ·74
Mean temperature of Sept. 1854	55 ·07
Average quantity of rain in Sept. for fifteen previous years	2·81 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. Sept.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·346	30·282	29·83	30·34	30·50	68	45	62	55½	53	nw.	n.	nw.	....	....	·04
2.	30·315	30·146	29·88	30·42	30·34	67	45	60	56½	55	n.	nne.	w.	....	·04	....
☾ 3.	30·131	30·058	29·75	30·27	30·17	71	50	60	57	54	ne.	nne.	w.	·02	....	....
4.	29·997	29·962	29·60	30·06	30·16	68	44	61·5	55	46	nw.	nne.	nw.	·01	....	....
5.	30·018	29·987	29·60	30·23	30·29	64	42	59	48	44	ne.	n.	nw.	....	....	·07
6.	30·274	30·226	29·35	30·36	30·37	62	36	52·5	51	51	ne.	nnw.	w.	....	....	....
7.	30·445	30·385	30·05	30·22	30·30	66	28	50·5	53	54	ne.	nnw.	ssw.	....	....	·02
8.	30·429	30·199	29·90	29·85	29·90	74	33	53·5	56	52½	sw.	w.	w.	....	....	·05
9.	30·164	30·100	29·65	29·96	30·05	73	33	60	54	52	w.	n.	wnw.	....	....	·05
10.	30·122	30·050	29·69	29·98	29·86	70	36	57	58	53½	e.	nw.	ssw.	....	....	·03
● 11.	30·055	30·051	29·61	29·79	29·94	72	37	55	56	53	ne.	nw.	w.	....	....	·15
12.	30·161	30·059	29·71	29·91	30·04	73	39	54	54	49	w.	nw.	nnw.	·02	....	·21
13.	29·976	29·919	29·54	30·15	30·15	69	50	59	50½	44	sw.	s.	ne.	·10	·13	·08
14.	30·007	29·926	29·55	30·06	30·02	54	45	57	49½	42	ne.	nne.	ese.	·32	....	....
15.	30·093	30·090	29·68	29·93	29·82	64	45	52	52½	50	sw.	nw.	sse.	....	....	....
16.	30·133	29·954	29·47	29·72	29·78	68	54	62	52½	51½	w.	wsw.	w.	·04	....	·21
17.	30·026	29·944	29·58	29·71	29·70	62	55	56	54½	52½	e.	wsw.	w.	·06	....	·11
18.	29·952	29·941	29·46	29·60	29·61	69	54	61	55½	51½	sw.	s.	w.	....	....	·15
☽ 19.	30·042	29·965	29·50	29·78	29·70	68	47	61	52½	53	ne.	n.	s.	....	....	·18
20.	30·130	30·112	29·58	29·73	29·77	75	44	58	56	59	sw.	sw.	ssw.	....	....	·07
21.	30·256	30·242	29·72	29·82	30·02	74	45	59	59½	55	sw.	w.	calm	....	....	·05
22.	30·274	30·237	29·80	30·12	30·32	75	41	56	59½	54	e.	n.	wnw.	....	....	·05
23.	30·250	30·229	29·77	30·46	30·55	76	55	60	53	48	e.	nne.	nnw.	....	....	....
24.	30·436	30·413	29·96	30·55	30·46	65	42	57·5	52	49	ne.	nne.	sse.	....	·03	....
○ 25.	30·430	30·305	29·96	30·27	30·14	65	27	58	52½	55½	e.	sse.	sse.	....	....	....
26.	30·247	30·091	29·70	29·99	29·98	61	26	50	53½	51	e.	sse.	wsw.	....	....	....
27.	29·972	29·752	29·50	29·82	29·58	74	51	56	54	51½	sw.	sse.	se.	....	....	....
28.	29·669	29·446	29·18	29·32	29·45	72	56	60	57½	52½	sw.	sse.	nne.	·32	....	....
29.	30·024	29·538	29·13	29·60	29·72	73	55	61·5	54	55½	se.	s.	ese.	·08	·10	·21
30.	29·505	29·423	29·04	29·75	29·70	68	47	63	56	53	se.	e.	se.	·10	....	·01
Mean.	30·128	30·031	29·29	29·992	30·013	68·66	43·56	57 7	54·30	51·18				1·15	0·30	1·74

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LIII. *On the Transmission of Heat in Metals.*
By G. WIEDEMANN*.

1. *Conductibility of Zinc.*

AN investigation undertaken by myself and Dr. Franz showed that the values of the relative conductibility of different metals for heat and electricity were very nearly equal†.

With the same apparatus which served for the earlier experiments, I have recently undertaken a determination of the conductibility of zinc for heat.

A zinc wire, 4·4 millims. thick, was used; its surface was very carefully polished, but not silvered. The wire was investigated in air. Its temperatures were taken at points, which followed each other at distances of 2 inches from the warmest part of the wire (the zero point) to the coldest, and in degrees of the galvanometer described in the above paper, were as follows:—

	I.		II.		III.	
x.	*t*.	*q*.	*t*.	*q*.	*t*.	*q*.
0	205		160·5		129·5	
2	115·5	2·361	90·5	2·355	75	2·319
4	67·7	2·301	51·5	2·340	44·25	2·317
6	40·25	2·303	30·5	2·311	27·6	
8	25		19	2·290	—	
10	—		13			
	2·39	2·322		2·324		2·318

The numbers in the column *x* are the distances from the zero to the several points on the bar at which the temperatures were observed; under t are found the corresponding deflections of the galvanometer when the thermo-electric element used in measuring the temperatures was applied to those parts of the bar; under q the quotients are given which are obtained by dividing the t standing near the corresponding q, into the sum of the temperatures standing above and below it.

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* From Poggendorff's *Annalen*, vol. xcv. p. 337.

† Poggendorff's *Annalen*, vol. lxxxix. p. 497; and Phil. Mag. vol. vii. p. 33.

If the highest temperature observed in each series be assumed equal to 100, the other temperatures will be as follows:—

	I.	II.	III.	
x.	t.	t.	t.	Mean.
0	100	100	100	100
2	56·3	56·7	57·9	57
4	33·0.	32·1	34·2	33·1
6	19·6	19·0	21·3	20·0
8	12·2	11·8	—	12·0
10	—	7·2	—	7·2

From the mean value of the quotients $q=2{\cdot}321$, the relative conductibility of the zinc bar may be estimated at 20·3, if the conductibility of an equally thick silver bar be placed at 100. If the deflections of the galvanometer given in the first table be reduced to actual excess of temperature of the several points of the bar above the surrounding air, the conductibility of zinc against that of silver at 100 will be—

19·0.

Becquerel found that the conductibility of zinc for electricity was 24 (silver $=100$). Zinc takes, therefore, pretty much the same place in the list of metals, whether considered as a conductor of heat or electricity.

2. *Passage of Heat from one metal to another.*

In considering the relative conductibilities of the different metals for heat, the question suggests itself, what takes place on the passage of heat from one metal to another.

Besides the old experiments of Fourier*, made with the contact thermometer, from which, however, no certain conclusions can be drawn, this question has been treated by M. Despretz†, and not long since, briefly, by M. Angström‡.

M. Despretz pressed together the sections of a copper and a tin bar, and heated the free end of the copper bar. The temperatures were observed by means of thermometers sunk in different parts of the bars, and from these Despretz calculated the temperatures at the point of contact, and found that the temperature of the copper bar there was $1^{\circ}{\cdot}47$ C. higher than that of the tin bar. If a piece of paper were interposed between them, the difference amounted to 5°·5 C. Hence Despretz concludes that heat suffers a resistance in its passage from one metal to another. Besides the numbers here mentioned, Despretz gives none by which the entire process of the experiment can be realized.

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* Poggendorff's *Annalen*, vol. xiii. p. 327.

† Ibid. vol. xlvi. p. 484. ‡ Ibid. vol. lxxxviii. p. 165.

Angström has also instituted only isolated experiments. He heated a bar composed of lead, copper and tin, at the one or the other end, and observed the temperatures at different places by means of thermometers sunk in the bar. Angström found that the ratio of the temperatures of two parts of the bar, on opposite sides of a place of contact of two of its metallic components, was greater when the heat passed from the worse to the better conductor than in the reverse case. In any case this last method of experiment is far too complicated to allow any direct conclusions to be drawn as to the different facilities with which heat passes in either the one or the other direction. The bars being comparatively short, and hence the temperatures observed at points equidistant from each other not forming a geometrical series, the mathematical consideration of the question is by no means simple.

The experiments of Gore also give no certain results as to the passage of heat from one metal to another.

The following experiments are intended to settle the point in question, and to examine whether heat really experiences any resistance in its passage.

For this purpose the same apparatus was used which had been constructed for the determination of the relative conductibility of the metals for heat. Two metal bars, 5 millims. thick, whose conductibility had been already determined, were cut in two, their sections accurately fitted upon each other, and carefully joined by means of an extremely thin layer of solder. The bars thus joined were placed in the apparatus and heated. The experiments were made in air, and the temperatures read off by the application of a thermo-electric element at points 2 inches asunder, commencing at the colder and proceeding to the warmer parts of the bar. In a second series of observations, the temperatures were read off at points midway between the points at first observed, so that at last the temperature of the bars was obtained from inch to inch.

Of numbers of experiments made in this manner I will select only a few. In the following Table, the numbers under x are the distances of the several points of the bars commencing at the place of junction; those under t are the corresponding temperatures represented in degrees of the galvanometer. The points of the warmer bar, mentioned first in the table, are marked with +, and those of the colder with −.

x.	Copper-German silver. t.	Copper-Iron. t.	Silver-Iron. t.	Copper-Copper. t.	Brass-Silver. t.
+4	130·75	155	154·5	146	—
+3	117	140·5	140·75	124	138
+2	108·5	129·0	131·5	110·7	102
+1	103	118·2	124·75	97	71·1
0				87·5	
−1	52·75	71·5	78·25	77·5	39·5
−2	31·75	52·5	55·6	67·0	35·0
−3	19·75	37·5	40·5	59·5	31
−4	11·75	27·5	28·8	49·2	27
−5	8·25	20	20	—	23·8
−6	—	15	15·1	—	—
−7	—	11·5	10·75	—	—

At the place of junction a slight displacement of the thermo-electric element (with the exception of the case of the two last bars) causes a considerable deflection in the galvanometer.

In all cases the temperatures of the colder bars form almost a geometrical series, and the bars can hence be considered infinitely long.

If heat passes from one metallic rod into another equally thick but infinitely long, the two rods must show a change of temperature at their junction if the heat experience a resistance in its passage*.

If the temperature y_1 at the place of contact of the colder bar be calculated by multiplying the value under −1 with the mean ratio of the geometrical series which the temperatures of the colder bars form; if, further, this value be put equal 100, and the other temperatures of the bars calculated accordingly, we can, by an accurate delineation of the curve of heat corresponding to the warmer bar, find the value y of the temperature of the warmer bar at the place of contact.

The mean of the present and other series of experiments is found to be,—

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* *Vide* Poisson's *Théorie mathématique de la Chaleur.* If y and y_1 be the temperatures of the warmer and colder bars at their place of contact, and if r be a constant dependent upon the resistance to passage, and different for different metals, the quantity of heat, w, which passes in a unit of time through the point of contact is

$$w=\frac{1}{r}(y-y_1).$$

If the difference of temperature $y-y_1=0$, the constant of the resistance of passage r must also be equal to 0, for otherwise the quantity of heat passing through the point of contact would be $w=0$.

	y_1.	y.
Silver-Iron	100	111·5
Copper-German silver .	100	115
Copper-Iron	100	107·5
Copper-Copper . . .	100	100
Brass-Silver	100	99

According to this, a difference of temperature and a resistance of passage are only observed when heat passes from a better to a worse conducting metal. No such difference was observed in the passage of heat from a worse to a better conductor, nor in the passage through the place of contact of two metal rods of the same material.

These contradictions suggested some deficiency in the method of observation.

If the thermo-electric element be applied to equally warm points of heated bars of different conductibilities, the heat of the point touched communicates itself to the element. Without regarding the difference which results from the difference in the specific heats of both bars, more heat will pass to the part cooled by contact with the thermo-electric element in the case of the better than of the worse conducting bar. The first, therefore, will appear comparatively warmer. This error may cause the differences at the places of contact noted in the above Table, which are the more prominent the greater the difference between the conductibilities of the two metals. The warmer the place of junction is, the more these differences are seen, on which account they scarcely appear in the passage of heat from a worse to a better conducting metal, where the latter ought, properly, to appear warmer than the former.

This error can, however, exercise no disturbing influence on the relative conductibilities of the bars themselves. For on the supposition that the conductibilities of the bars do not vary at different temperatures, the changes of temperature at the several points of the same bar, caused by the application of the thermo-electric element, must be proportional to the temperatures themselves.

Hence the relative proportion of the temperatures observed at different parts of the bar remains unchanged, and this is alone decisive in calculating the relative conductibilities.

It was necessary, in obtaining reliable results as to the passage of heat, to make the loss of heat of the bars on the application of the thermo-electric element as small as possible. For this purpose the following arrangement of the experiments was made.

Round bars of different metals, of 13·2 millims. diameter and 157 millims. long, were made, and accurately ground plane at one end. A bar of iron and a bar of copper of the same diameter, but 666 millims. long, and a bar of bismuth equally thick but 265 millims. long, were ground plane at one end. These bars were then placed in a wooden frame with their polished ends free, &c., and the arrangement made, that to the plane end of each of the longer bars the plane end of one of the shorter was firmly screwed. At a distance of 2 millims. from the place of contact, and thence at distances of 21·4 millims., holes of 0·9 millim. wide and 8 millims. deep were bored. These holes were filled with oil. The end of each shorter bar was surrounded by a tin case, through which steam was passed for a long time. A screen protected the rest of the apparatus from the rays of heat. The whole apparatus was placed in a tin vessel surrounded by water, so that the loss of heat during the experiment should be as uniform as possible. After the apparatus had been constantly heated for two or three hours, and the heat in the joined bars had become constant, the temperatures were determined by sinking the thermo-electric element in the holes. The thermo-electric element was needle-formed, and consisted of two wires of iron and german silver, 0·3 millim. thick., placed together, but only soldered just at the end where they dipped in the holes. At the other ends copper wires were soldered, which communicated with the spirals of the galvanometer described in an earlier treatise*. The whole of the thermo-electric element, with the exception of the points dipping in the holes, was fastened in a glass tube with sealing-wax, which was cemented in a vessel of water in order to keep the posterior ends of the element at a constant temperature.

The following are some of the results obtained with the apparatus. Under x the series of holes is given, beginning with that nearest the place of junction. The holes corresponding to the warmer bars, which are first in the list, are marked with +, and the colder with −. Under t stand the corresponding temperatures. Each series contains the mean of several successive observations.

	Copper-Bismuth.	Zinc-Bismuth.	Copper-Iron.		Tin-Iron.		Iron-Iron.	Iron-Copper.
			I.	II.	I.	II.		
x.	*t*.	*t*.	*t*.	*t*.	*t*.	*t*.	*t*.	*t*.
+4	268·7	274·7	246·5	255·5	184·5	196·6	193	185
+3	261·2	252·5	235·7	243·7	155·5	161	158	—
+2	256	236·7	226	234	130	133·6	132	111·3
+1	252	226·7	218	226	107	110	108	81·5
−1	236·0	212	214·5	221	102·5	105	103·2	78·7
−2	120	108	178	181·2	84·5	86·8	85	74·7
−3	58·7	57·5	149·5	153	72	73·8	70·2	71
−4	32·2	33·5	126	130	61	63·5	59	67·5
−5	—	21·2	106	—	51·2	53·1	—	—

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* Poggendorff's *Annalen*, vol. lxxxix. p. 504.

If from these numbers the temperatures y and y_1 of the bars at their points of contact be calculated, we shall get—

	y.	y_1.	$y-y_1$.
Copper-Bismuth . .	252	252	0
Zinc-Bismuth . . .	226	226	0
Copper-Iron I. . .	217·7	217·5	0·2
Copper-Iron II. . .	225·5	225	0·5
Tin-Iron I. . . .	105	104·5	0·5
Tin-Iron II. . . .	108	107·1	0·9
Iron-Iron	105·5	105·1	0·4
Iron-Copper . . .	79·2	79·2	0

The difference of temperature of two bars in contact is hence very small. It is not greater in the case of two bars of different metals, whether the heat pass from the better to the worse conductor or the reverse, than in the case of two bars of the same metal.

Hence a resistance to the passage of heat through the place of contact of two bars cannot be proved within the limits of these observations*.

If the two bars are not perfectly in contact, a decrease of temperature at their place of contact is observed. The same happens when the bars are separated by a thin layer of some bad conductor.

Experiments made in this manner gave the following series of observations:—

	I.	II.	III.	IV.
	Zinc-Iron not fitting well on each other.	Iron-Copper not fitting well on each other.	Copper-Iron separated by a thin sheet of paper.	Zinc-Bismuth separated by a thin sheet of paper.
x.	t.	t.	t.	t.
+4	174·5	155·5	262	267
+3	153·5	125	253·7	245
+2	133·7	97	246	230
+1	125	71·7	239	220
−1	120	65·5	189·2	197
−2	99·7	61·5	157·2	100
−3	87·7	58	132·5	52
−4	71·5	55·1	113	29
−5	61	—	95·2	18

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* For an accurate calculation of the relative conductibilities of different metals the numbers in question are not fitted, since the quotient q of the sums of the temperatures of two points divided by the temperature of the middle point differs too little from 2. Hence a small error of observation changes materially the calculated result.

By calculating the temperatures y and y_1 of the warmer and colder bars corresponding to the places of contact, we have—

	y.	y_1.	$y-y_1$.
Zinc-Iron (not well fitted on each other)	123·5	122·2	1·3
Iron-Copper do.	68·4	66·1	2·3
Copper-Iron (separated by paper)	238·5	192·5	46
Zinc-Bismuth do.	219	211	8

These differences do not necessitate the existence of a resistance of passage. When the conductibility of the united bars at their place of junction is lessened by imperfect contact, or by the insertion of a piece of paper, a considerable decrease of temperature takes place at those points, by which a difference between the temperatures of the two bars at the place of contact could be easily explained.

LIV. *On the Principle of Isorrhopic Axes in Statics.*

By W. J. Macquorn Rankine, *C.E., F.R.SS.L. & E.*[*]

THEOREM. *Every equilibrated system of forces applied to a connected system of points, is capable of resolution into three rectangular independently equilibrated systems of parallel forces applied to the same points.*

Demonstration. Assume any set of rectangular axes, to which reduce the forces and the positions of their points of application; and let X, Y, Z be the components of the force applied to any point (x, y, z).

Let

$$\Sigma . \mathrm{X}x=\mathrm{A}; \quad \Sigma . \mathrm{Y}y=\mathrm{B}; \quad \Sigma . \mathrm{Z}z=\mathrm{C};$$

$$\Sigma . \mathrm{Y}z=\Sigma . \mathrm{Z}y=\mathrm{L}; \quad \Sigma . \mathrm{Z}x=\Sigma . \mathrm{X}z=\mathrm{M}; \quad \Sigma . \mathrm{X}y=\Sigma . \mathrm{Y}x=\mathrm{N}.$$

Conceive the surface of the second order whose equation is

$$\mathrm{A}x^2+\mathrm{B}y^2+\mathrm{C}z^2+2\mathrm{L}yz+2\mathrm{M}zx+2\mathrm{N}xy= \text{constant}.$$

Then if the forces and the positions of their points of application be reduced anew to the principal axes of that surface, we shall have

$$\mathrm{L}=0, \quad \mathrm{M}=0, \quad \mathrm{N}=0;$$

and consequently each of the three systems of component forces parallel to those three principal axes will be *in equilibrio*, independently of the other two systems. Q. E. D.

The above theorem, so far as I am aware, is new.

Glasgow, Oct. 26, 1855.

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[*] Communicated by the Author.

LV. *On the Composition and Phosphorescence of Plate-sulphate of Potash. By* DR. FREDERICK PENNY, *Professor of Chemistry, Andersonian University, Glasgow*[*].

THE subject of the present notice is a technical product from kelp. It is called "Plate-sulphate of Potash," and is so named from the circumstance of its being crystallized in thick plates or slabs, consisting of the aggregrated layers of successive crops of crystals.

It may be regarded as one of the chemical products peculiar to Glasgow, being manufactured only at one or two other places, and even there to a limited extent.

There are several points in the chemical history of this salt possessing a high degree of scientific interest. The most important are, the peculiarity of the process by which it is obtained in thick slabs or plates, the brilliant phosphorescence of its crystals at the time of their formation, and its remarkable composition. To these at least I shall limit my remarks in the present notice.

I. *Formation of the Plate-sulphate.*

In working kelp upon the large scale, several saline products are obtained, and among these there are two salts, containing sulphate of potash as the predominating ingredient. One is powdery or granular, and is distinguished as *soft* or *granulated* sulphate of potash, and the other is the *plate*-sulphate.

Soft sulphate is obtained by simply evaporating or concentrating the kelp-leys, when it falls as a heavy granular precipitate. It is a very impure substance, being largely intermixed with sulphate of soda, common salt, carbonate of soda, sulphocyanide of potassium, iodide of sodium, and with insoluble matter and water. The amount of sulphate of potash existing in soft sulphate is very variable. That from cut-weed kelp seldom contains more than 32 per cent. Good drift-weed kelp yields a sulphate not usually under 44 per cent., and occasionally as high as 65 per cent. It is used in making plate-sulphate, and for agricultural manures, particularly when of low strength.

Plate-sulphate is made in two ways: either from the impure soft sulphate, or directly from the liquors that result by steeping kelp in hot water. The process is called "plating," and the mode of working is peculiar. In the ordinary process of crystallizing salts by solution in water, each crop of crystals, when fully formed, is removed from the crystallizing vat or vessel after the mother-liquor has been run off; a second or third crop being obtained by evaporating the mother-liquor, or by dissolving in it, when heated, a further quantity of the salt. In plating sulphate of potash, a different mode of proceeding is adopted. The soft sulphate is dissolved either in weak ley from kelp, or in boiling water, until the liquor is about 44° of Twaddell. This is evaporated to 48° or 50°, and run into coolers to crystallize. When the crystals have fully formed and the mother-liquor has completely cooled, the latter is siphoned off, and being saturated with soft sulphate as before, it is again transferred to the coolers, and a second crop of crystals deposited on the first. These operations are repeated several times, and in this way from four to six, or even seven, successive and cohering crops are obtained in the form of a hard thick cake. The number of crops deposited depends on the success of the process, the richness of the soft sulphate, and the thickness of cake required.

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[*] Communicated by the Author.

In plating directly from kelp, the leys, or liquors obtained from steeping the kelp in water, are boiled down till the specific gravity of the liquor indicates that sulphate of potash is beginning to fall. The fires are then drawn, and after a brief reposure, to allow the floating impurities to subside, the liquor is run into the cooler and crystallized. In a few days a crop of plate-sulphate is found adhering to the sides. The mother-liquor is siphoned off, and a fresh quantity of concentrated kelp-ley being run into the cooler, a second crop of crystals is deposited on the surface of the first. A succession of crops is thus accumulated until the plate or cake is considered of sufficient thickness.

From this brief description of the process of plating, it is manifest that the salt, whether made from kelp-leys or soft sulphate, must be more or less charged with impurities. The appearance of the cakes, indeed, shows that it is far from being a chemically pure substance; and on examining it, the presence of minute quantities of carbonate of soda, common salt, and insoluble matter, may be readily detected. The large and well-defined crystals studded over the surface of the cakes are, however, nearly pure; and when carefully selected, contain very little foreign matter.

II. *Its Phosphorescence.*

The brilliant and sparkling light emitted from this salt at the time of crystallizing has been frequently observed. It may be witnessed by simply looking into a vat of liquor in process of cooling and crystallizing, and particularly when the temperature of the liquor is about 100° F. It is not a continuous luminosity or phosphorescence of the crystalline mass, but an emission of vivid sparks or scintillations as the crystals separate from the liquor, analogous to the phosphorescence of arsenious acid in the act of crystallizing from the hydrochloric acid.

The luminosity of salts in the act of crystallizing, and especially of sulphate of potash, both pure and mixed with various salts of soda, has been made the subject of repeated experimental investigation by several chemists. The extended and interesting researches by Rose* must be generally known. In Gmelin's 'Chemistry' we have a condensed but faithful account of the results observed by different experimenters. The subject is still involved in considerable obscurity.

I regret that I am as yet unable to give a satisfactory explanation of the phænomena in regard to the salt now under consideration, having reluctantly been obliged, by an enforced relaxation from all such work, to discontinue for a time the experiments undertaken to elucidate the subject. I may mention, however, the principal phænomena that have been observed in the trials already made.

1st. The phosphorescence is not visible so long as the temperature of the crystallizing liquor is much above 100° F.; but as the temperature falls below this, the scintillations become vivid and frequent.

2nd. When a rod of glass, of wood, or of metal, is drawn across the surface of the crystalline mass below the liquor, a line of brilliant light is produced in the track of the rod.

3rd. On breaking the layer of crystals that frequently forms on the surface of the liquor, vivid sparks are emitted as the detached portions sink to the bottom of the vessel.

4th. On plunging a mass of the crystals into hot liquor and immediately withdrawing it, a few scintillations are visible; but if the crystals are kept in the hot liquor for a few minutes, and then plunged into nearly cold liquor, the flashes are very brilliant, and diffused over the entire surface of the mass.

5th. The most beautiful effect, however, is produced when a quantity of moderately warm liquor is dashed upon a crop of crystals in a vat, from which the mother-liquor has some hours previously been drawn off. The crystals become instantaneously illuminated with myriads of beautiful and vivid coruscations. A second quantity of the same heated liquor thrown upon the mass gives a much less brilliant effect, and a third quantity scarcely any flashes. But if a bucket of perfectly cold liquor is dashed upon the crystals immediately after the hot liquor has ceased to give scintillations, the result is even more splendid and striking than in the last experiment.

When the heated liquor is dashed upon crystals of chloride of potassium, or upon wood, or iron, or ice, no visible effect is produced.

6th. In recrystallizing the salt, no phosphorescence is visible.

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* Poggendorff's *Annalen*, vol. lii.

The results of these few experiments, though affording very little insight into the cause of the phænomena, plainly indicate that the emission of light takes place within a limited range of temperature, and that actual contact of the crystals with the crystallizing liquor is essential. Agitation of the liquor with a rod produces no effect.

III. *Composition.*

I have already stated that the composition of this salt is remarkable. It has hitherto been viewed and described as a peculiar form or variety of neutral sulphate of potash, and as such it is regarded both by chemists and manufacturers. The results of my analyses do not confirm this view of its composition. They show unequivocally, that, instead of being a mere crystalline modification of the neutral sulphate, it is a double salt, consisting of sulphate of potash and sulphate of soda. I have analysed bold and well-defined crystals, carefully selected from numerous specimens obtained from different makers, and I find that they are perfectly uniform in composition. I have also analysed crystals of the same salt, procured by recrystallizations of the commercial salt.

In these analyses it never gave more than 42·5 per cent. of potash; and parties acquainted with this article commercially, know that in the bulk it never contains more than from 75 to 78 per cent. of sulphate of potash,—equal to about 41·5 per cent. of potash, whereas sulphate of potash contains 54 per cent.

It is not, I conceive, either desirable or necessary to give the details of the methods of analysis, or particulars of the several results. It will be sufficient to state, that the sulphuric acid was determined, as usual, in the state of sulphate of baryta, the potash by means of bichloride of platinum, and the soda indirectly by deduction. The only precaution requiring notice is, that the precipitated sulphate of baryta must be well washed *after ignition*, to remove the potash which is carried down with the first precipitate.

The mean of twelve analyses, the results of which accord very closely, gave the following proportion of potash, sulphuric acid and soda, in 100 parts:—

	Experiment.	Theory.
Potash	42·22	42·47
Sulphuric acid . .	48·24	48·19
Soda	9·54	9·34
	100·00	100·00

A carefully selected specimen gave 42·46 per cent. of potash.

These proportions correspond to the formula $3KO\ SO^3, NaO\ SO^3$, as shown in the third column of the above statement.

The presence of soda in the salt was easily established by decomposing a known quantity with nitrate of baryta, and crystallizing the solution filtered from the precipitate. Crystals of nitrate of soda mixed with those of nitre were abundantly deposited. In another experiment, a known quantity of the salt dissolved in water was decomposed by chloride of barium, and the solution, separated from the precipitate by filtering, was evaporated to dryness; and the mixed chlorides, after treatment with bichloride of platinum, were tested with the delicate and beautiful process for the detection of soda proposed by Dr. Andrews at the Meeting of the British Association in Belfast*.

It will be at once remarked, that the composition of this salt does not correspond to that of any other double salt of potash. The atomic proportions of its ingredients are at variance with our ordinary notions of the constitution of these compounds. I hope, however, to be able to explain the anomaly by the examination of other compounds of sulphate of potash, and of chromate of potash, which I have in process of preparation.

In Gmelin's 'Chemistry†,' it is incidentally mentioned that 1 part by weight of dry carbonate of soda, and 2 parts of bichromate of potash, give crystals having the same form as sulphate of potash, and containing $3KO\ CrO^3, NaO\ CrO^3$, which corresponds exactly with plate-sulphate of potash. It is also stated that crystals containing *about* $2KO\ SO^3, NaO\ SO^3$, may be prepared by fusing equal atomic proportions of the two sulphates, and crystallizing the mixture from water. Rose made several analyses of the salt, obtained by crystallizing together the sulphates of potash and soda; but the discrepancies in the results led him to doubt the definite composition of the crystals.

Dr. Gladstone‡ has made a series of experiments with a view to produce a definite double sulphate of potash and soda. He crystallized various mixtures of the two sulphates, and obtained a salt to which he ascribed the formula $5KO\ SO^3, NaO\ SO^3$. Such a compound would contain 46·44 per cent. of potash, and 47·43 of sulphuric acid.

In several of its physical characters, the plate-salt differs essentially from neutral sulphate of potash. It is, for example, more readily fusible, and it may be melted without much difficulty in a platina crucible with the ordinary gas-blowpipe; whereas sulphate of potash is only slightly softened at the edges, even when the blast from the bellows is urged to the fullest extent; it requires the full bright red heat of a furnace for perfect fusion.

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* Chemical Gazette, vol. x.

† Vol. i. p. 108.

‡ Quarterly Journal, vol. vi.

The density of the double sulphate is also greater than that of sulphate of potash. The specific gravity of its crystals, finely pulverized, taken in oil of turpentine at 59° F., was found to be 2·6682. After being fused, its specific gravity was 2·6708. The specific gravity of sulphate of potash, taken in the same way and at the same time, was in the crystallized state 2·6438; and in the vitreous or fused state, 2·6567. Dr. Lyon Playfair gives 2·644 as the specific gravity of sulphate of potash.

There is also a marked difference in the solubility of this salt in cold and boiling water, as compared with that of sulphate of potash. 100 parts of a saturated solution of $KO\,SO^3$, at 217° contain 22·5 parts of salt, or 100 parts of water at that temperature dissolve 29 parts; 100 parts of the solution of plate-salt at 217°·5 contain 28·9 parts, or 100 water will dissolve nearly 40·8 parts.

It is, I think, more than probable that certain specimens which have been figured as sulphate of potash, were crystals of this double salt. I have crystallized sulphate of potash under various circumstances, but have never obtained crystals in the forms figured by Phillips*, Brooke†, and others, unless a salt of soda has been present in the solution, causing, as I believe, the formation of the double salt. Both upon the large and small scale, sulphate of potash was deposited either in rhomboidal prisms or in hexahedral prisms, with bipyramidal summits, or in bipyramidal dodecahedrons.

In conclusion, I may mention that I have examined the successive crops of crystals obtained by recrystallizing the plate-sulphate. Several pounds of the salt were dissolved in water, and treated as usual, the crystallizing operations being continued to the 11th crop. The results were as follows:—

1*st Crop.* Sulphate of potash with a little plate-salt.

2*nd Crop.* Plate-salt, with short prisms of sulphate of soda.

3*rd Crop.* Chiefly sulphate of soda; crystals large and bold.

4*th Crop.* Layer of plate-salt with sulphate of soda above.

5*th*, 6*th* and 7*th Crops.* Sulphate of soda.

8*th*, 9*th* and 10*th Crops.* Almost wholly plate-salt.

11*th Crop.* Sulphate of soda.

It is therefore quite manifest, that, with care, this salt can be produced at pleasure by recrystallization from the commercial salt, or by crystallizing a mixture of sulphate of potash and sulphate of soda in equal atomic proportions.

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* Ann. Phil. 1822.

† Ibid. 1824.

LVI. *On the Theory of Diamagnetism. Letter from* Professor WEBER *to* Prof. TYNDALL.

MY DEAR SIR, Göttingen, Sept. 25, 1855.

ACCEPT my best thanks for your kind communication of the 3rd of September; I am gratified to learn that the apparatus executed by M. Leyser in Leipzig for the demonstration of diamagnetic polarity has so completely fulfilled your expectations. This intelligence is all the more agreeable to me, inasmuch as before the apparatus was sent away, it was not in my power to go to Leipzig and test the instrument myself.

It gave me great pleasure to learn that Mr. Faraday and M. De la Rive have had an opportunity of witnessing the experiments, and of convincing themselves as to the facts of the case.

It was also of peculiar interest to me to learn that you had succeeded in establishing the polarity of the self-same heavy glass with which Faraday first discovered diamagnetism. This is the best proof that these experiments do not depend upon the conductive power of bismuth for electricity.

I have read with great interest your memoir "On the Diamagnetic Force," &c. contained in the Philosophical Transactions, vol. cxlv. It has been your care to separate the *fact* of diamagnetic polarity from the *theory*, and to place the former beyond the region of doubt. Allow me, with reference to this subject, to direct your attention to a passage at page 39 of your memoir, which you adduce as a conclusion from my theory; the passage runs as follows:—

"The magnetism of two iron particles in the line of magnetization is increased by their reciprocal action; but, on the contrary, the diamagnetism of two bismuth particles lying in this direction is diminished by their reciprocal action."

This proposition is by no means a necessary assumption of my theory, but is rather a direct consequence of diamagnetic polarity, if the facts be such as both you and I affirm them to be. What, therefore, you have adduced against the above conclusion must be regarded as an argument against diamagnetic polarity itself. The *diamagnetic reciprocal action* of the bismuth particles in the line of magnetization is necessarily opposed to the *action of the exciting magnetic force.* The latter must be enfeebled, because the diamagnetic is opposed to the *magnetic reciprocal action* of iron particles which lie in the line of magnetization, through which latter it is known the action of the exciting magnetic force is increased. Hence also the *modification* produced in bismuth by magnetic excitement, whatever it may be, must be weakened, because the force of excitation is diminished.

(I believe, however, that this argument against diamagnetic polarity may also be surmounted. The phænomenon which you have observed must be referred to other circumstances, also connected with the compression of the bismuth. For the diamagnetic reciprocal action is, as I have shown, much too weak to produce an effect which could be compared in point of magnitude with the reciprocal action produced in the case of iron.)

I take this opportunity of adding a few remarks for the purpose of setting my theory of diamagnetic polarity in a more correct light.

My theory assumes:—1, that the fact of diamagnetic polarity is granted; 2, that in regard to magnetic phænomena, Poisson's theory of two magnetic fluids, and Ampère's theory of molecular currents, are equally admissible. Whoever denies the first fact, or rejects the theory of Ampère, cannot, I am ready to confess, accept my theory.

But supposing that you do not reject Ampère's theory of permanent molecular currents, but are disposed to enter upon the inner connexion and true significance of the theory, you will easily recognize that it is by no means *an arbitrary assumption of mine*, that in bismuth molecular currents are excited, when the exciting magnetic force is augmented or diminished; but that the excitation of such molecular currents is *a necessary conclusion from the theory of Ampère,* which conclusion Ampère himself could not make, because the laws of voltaic induction, discovered by Faraday, were unknown to him. In all cases where molecular currents *exist,* by increase or diminution of the magnetic exciting force *molecular currents must be excited,* which either add their action to, or subtract it from, the action of those already present.

Finally, permit me to make a few remarks on the following words of your memoir:—

"To carry out the assumption here made, M. Weber is obliged to suppose that the molecules of diamagnetic bodies are surrounded by channels, in which the induced currents, once excited, continue to flow without resistance."

The assumption of channels which surround the molecules, and in which the electric fluids move without resistance, is an assumption contained in the theory of Ampère, and is by no means added by me for the purpose of explaining diamagnetic polarity. *A permanent molecular current without such a channel involves a manifest contradiction,* according to the law of Ohm.

I may further observe, that I do not wonder that you regard a theory which is built upon the assumption of such channels, as "so extremely artificial that you imagine the general conviction of its truth cannot be very strong." In a certain sense I quite agree with you, but I only wish to convince you that this objection applies really to the theory of Ampère, and only applies to mine in so far as it is built upon the former. (You may perhaps find less ground for objecting to the speciality of such an assumption, if you separate the simple fundamental conception, which recommends itself particularly by a certain analogy of the molecules to the heavenly bodies in space, from those additions which Ampère was forced to make, in order to apply the mathematical methods at his command, and to make the subject one of strict calculation. He was necessitated to reduce the case to that of *linear* currents, which necessarily demand channel-shaped bounds, if every possibility of a lateral outspreading is to be avoided.)

To place my theory of diamagnetic polarity in a truer light, I am anxious also to convince you that this theory is by no means based upon new assumptions (hypotheses); but that it only rests upon such conclusions as may be drawn from the theory of Ampère, when the laws of voltaic induction discovered by Faraday, and the laws of electric currents by Ohm, are suitably connected with it. I affirm, that, even if Faraday had not discovered diamagnetism, by the combination of Ampère's theory with Faraday's laws of voltaic induction, and Ohm's laws of the electric current, as shown in my memoir, the said discovery might possibly have been made.

In respect, however, to the artificiality of the theory of Ampère, I hope that mathematical methods may be found whereby the limitation before mentioned to the case of linear currents may be set aside, and with it the objection against channel-form beds. All our molecular theories are still very artificial. I for my part find less to object to in this respect in the theory of Ampère than in other artificialities of our molecular theories; and for this reason, that in Ampère's case the nature of the artificiality is placed clearly in view, and hence also a way opened towards its removal.

To Mr. Faraday I beg of you to present my sincerest respect.

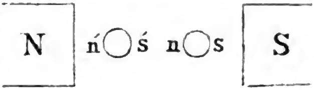
Believe me, dear Sir,

Most sincerely yours,

WILHELM WEBER.

Professor Tyndall.

The foregoing letter possesses more than a private interest, and I have therefore laid it before the readers of the Philosophical Magazine. On one point in it only would I ask permission to make a remark, and that is the proposition, that the diminution of the excitement of a row of bismuth particles in the line of magnetization by their reciprocal action is "*a direct consequence of diamagnetic polarity.*" M. Weber (I believe) founds this proposition on the following considerations:—Let a series of bismuth particles lie in the axial line between the magnetic poles N and S: the polarity excited in these particles by the direct action of the poles will be that shown in the figure, being the reverse of that of iron particles under the same circumstances. But as the end n of the right-hand particle tends to excite a magnetism like its own in the end s' of the left-hand particle, and *vice versâ*, this action is opposed to that of the magnet, and hence the magnetism of such a row of particles is enfeebled by their reciprocal action.



Now it appears to me that there is more assumed in this ingenious argument than experiment at present can bear out. There are no experimental grounds for the assumption, that what we call the north pole of a bismuth particle exerts upon a second bismuth particle precisely the same action that the north pole of an iron particle would exert. Magnetized iron repels bismuth; but whatever the *fact* may be, the *conclusion* is scarcely warranted, that *therefore* magnetized bismuth will repel bismuth. Supposing it were asserted that magnetized iron attracts iron and repels bismuth, while magnetized bismuth attracts bismuth and repels iron, would there be anything essentially impossible, self-contradictory, or absurd involved in the assertion? I think not. And yet if even the possible correctness of such an assertion be granted, the proposition above referred to becomes untenable. It will be observed that it is against a conclusion rather than a fact that I contend. With regard to the fact, I should be sorry to express a positive opinion; for this is a subject on which I am at present seeking instruction, which may lead me either to M. Weber's view or the opposite. Be that as it may, the result cannot materially affect the respect I entertain for every opinion emanating from my distinguished correspondent on this and all other scientific subjects.

J. T.

LVII. *On the Hypothesis of Molecular Vortices, or Centrifugal Theory of Elasticity, and its Connexion with the Theory of Heat.* By WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.SS.L. & E., F.R.S.S.A. &c.*

[Concluded from p. 363.]

SECTION SECOND.—*Relations between Heat and Expansive Power.*

(9.) *VARIATIONS of Sensible and Latent Heat: Fundamental Equation of the Theory.*—If the forms, positions, and magnitudes of the paths described by the revolving particles of the atomic atmospheres be changed, whether by a variation of mean density, or by a variation of temperature, an increase or diminution of the *vis viva* of their motion, that is to say, of the heat of the body, will take place in virtue of that change of the paths of motion; an increase when they are contracted, and a diminution when they are dilated.

Let $\delta \,.\, Q$ represent, when positive, the indefinitely small quantity of heat which must be communicated to unity of weight of a substance, and when negative, that which must be abstracted from it, in order to produce the indefinitely small variation of temperature $\delta\tau$ simultaneously with the indefinitely small variation of volume δV. Let $\delta \,.\, Q$ be divided into two parts,

$$\delta Q + \delta Q' = \delta \,.\, Q,$$

of which δQ, being directly employed in varying the *velocity* of the particles, is the variation of the *actual* or *sensible* heat possessed by the body; while $\delta Q'$, being employed in varying their *orbits*, represents the amount of the mutual transformation of heat with expansive power and molecular action, or the variation of what is called the *latent* heat; that is to say, of a molecular condition constituting a source of power, out of which heat may be developed.

The variation of sensible heat has evidently this value,

$$\delta Q = \mathfrak{k}\delta\tau. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Let δx, δy, δz be the displacements of the orbit of the particles of atomic atmosphere at the point (x, y, z). A molecule $\rho dx\,dy\,dz$ is acted upon by the accelerative forces (see equation 3A),

$$-2Q\frac{d\phi}{dx}; \quad -2Q\frac{d\phi}{dy}; \quad -2Q\frac{d\phi}{dz};$$

parallel to the three axes respectively.

The sum of the actions of those forces on the molecule $\rho dx\,dy\,dz$ during the change of temperature and volume, is

$$-2Q\left(\frac{d\phi}{dx}\delta x+\frac{d\phi}{dy}\delta y+\frac{d\phi}{dz}\delta z\right)\rho dx\,dy\,dz$$
$$=-2Q\delta\phi\rho dx\,dy\,dz.$$

The sum of such actions upon all the particles in unity of weight is equal in amount, and opposite in sign, to the variation of latent heat; that is to say,

$$\delta Q'=\frac{2Q}{M}\iiint_{(1)}\rho\delta\phi dx\,dy\,dz. \quad . \quad . \quad . \quad . \quad (23)$$

To determine the value of the variation $\delta\phi$, let it be divided into two parts, thus:—

$$\delta\phi=\delta\phi_1+\delta\Delta\phi,$$

where $\Delta\phi=\phi-\phi_1$.

First. With respect to $\delta\phi_1$, it is obvious, that because, according to equations (6, 7),

$$MV=kMV\int_{-\infty}^{\phi_1}e^{k\Delta\phi}\frac{\psi}{\psi_1}d\phi,$$

we must have

$$\delta V=kV\delta\phi_1 \text{ and } \delta\phi_1=\frac{\delta V}{kV}.$$

Hence the first part of the integral (23) is

$$\frac{2Q}{M}\delta\phi_1\iiint_{(1)}\rho dx\,dy\,dz=\frac{2\mu Q}{MkV}\cdot\delta V$$
$$=\frac{h\mu}{\kappa M}(\tau-\kappa)\frac{\delta V}{V}=N\mathfrak{k}(\tau-\kappa)\frac{\delta V}{V}. \quad . \quad (23\text{A})$$

To determine the second part of the integral, we have the condition, that the quantity of atomic atmosphere enclosed within each surface at which $\Delta\phi$ has some given value is invariable; that is to say,

$$\left(\delta\Delta\phi\frac{d}{d\Delta\phi}+\delta V\frac{d}{dV}+\delta\tau\frac{d}{d\tau}\right)\left(k\rho_1MV\int_{-\infty}^{\phi}e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}d\phi\right)=0.$$

Hence

$$\delta\Delta\phi=\frac{-\left(\delta V\frac{d}{dV}+\delta\tau\frac{d}{d\tau}\right)\left(k\rho_1MV\int_{-\infty}^{\phi}e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}d\phi\right)}{k\rho_1MVe^{k\theta\Delta\phi}\frac{\omega}{\omega_1}}.$$

The value of the second part of the integral (23) is now found to be

$$\frac{2Q}{M}\iiint_{(1)}\rho\delta\Delta\phi dx\,dy\,dz=\frac{2Q}{M}k\rho_1MV\int_{-\infty}^{\phi_1}e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}\delta\Delta\phi d\phi$$
$$=-\frac{2Q}{kM}\left(\delta V\frac{d}{dV}+\delta\tau\frac{d}{d\tau}\right)\left\{\rho_1MV\int_{-\infty}^{k\phi_1}\int_{-\infty}^{k\phi}e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}k^2d\phi^2\right\}.$$

In the double integral, let $\lambda = \log_e V$ be put for $k\phi$, G for ω, and H for the single integral, as in equation (9). Then the double integral becomes

$$\frac{1}{G_1}\int_{-\infty}^{\lambda_1} H\,d\lambda = -\frac{1}{G_1}\frac{dH_1}{d\theta} \text{ by equation (10)}$$

$$= -\frac{\kappa}{G_1}\frac{dH_1}{d\tau}.$$

Also because $\rho_1 MV = \frac{\mu G_1}{H_1}$ by equation (9), and $\frac{2Q'}{kM} = \frac{h}{\kappa}(\tau-\kappa)$, the second part of the integral (23) is found to be

$$\frac{h\mu}{M}(\tau-\kappa)\left(\delta\tau\frac{d}{d\tau}+\delta V\frac{d}{dV}\right)\frac{dH_1}{H_1 d\tau}. \quad . \quad . \quad (23\text{ B})$$

Hence, adding together (23 A) and (23 B), we find for the total variation of latent heat,

$$\delta Q' = \frac{h\mu}{M}(\tau-\kappa)\left\{\delta\tau . \frac{d^2\log_e H_1}{d\tau^2} + \delta V . \left(\frac{1}{\kappa V} + \frac{d^2\log_e H_1}{d\tau\, dV}\right)\right\}. \quad (24)$$

To express this in terms of quantities which may be known directly by experiment, we have by equations (10) and (9),

$$\frac{dH_1}{H_1 d\lambda} + \theta - \frac{G_1}{H_1} = 0,$$

that is to say,

$$\frac{d\log_e H_1}{dV} = \frac{G_1}{H_1 V} - \frac{\tau}{\kappa V} = \frac{M}{h\mu}p - \frac{\tau}{\kappa V};$$

and therefore

$$\log_e H_1 = \frac{M}{h\mu}\int p\,dV - \frac{\tau}{\kappa}\log_e V + f(\tau) + \text{ constant.}$$

$f(\tau)$ is easily found to be $= -\log_e \tau$ for a perfect gas, and being independent of the density, is the same for all substances in all conditions; hence we find (the integrals being so taken that for a perfect gas they shall $=0$),

$$* \ \frac{d\log_e H_1}{d\tau} = \int\left(\frac{M}{h\mu}\frac{dp}{d\tau} - \frac{1}{\kappa V}\right)dV - \frac{1}{\tau}$$

$$\frac{d^2\log_e H_1}{d\tau^2} = \frac{M}{h\mu}\int\frac{d^2p}{d\tau^2}\,dV + \frac{1}{\tau^2}$$

$$\frac{d^2\log_e H_1}{d\tau\, dV} = \frac{M}{h\mu}\frac{dp}{d\tau} - \frac{1}{\kappa V};$$

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* This coefficient corresponds to $-\frac{U}{\kappa}$ in the notation of my previous paper on the Mechanical Action of Heat.

and therefore

$$\delta Q' = (\tau - \kappa)\left\{\delta\tau \,.\left(\frac{h\mu}{M\tau^2} + \int \frac{d^2p}{d\tau^2}\,dV\right) + \delta V \,.\, \frac{dp}{d\tau}\right\} \quad . \quad (25)$$

is the variation of latent heat, expressed in terms of the pressure, volume, and temperature; to which, if the variation of sensible heat, $\delta Q = \mathfrak{k}\delta\tau$, be added, the complete variation of heat, $\delta Q + \delta Q' = \delta \,.\, Q$, in unity of weight of the substance, corresponding to the variations δV and $\delta\tau$ of volume and temperature, will be ascertained.

The only specific coefficient in the above formula has the following value:—

$$\frac{h\mu}{M} = N\mathfrak{k}\kappa. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25\ A)$$

It is obvious that equation (25), with its consequences, is applicable to any mixture of atoms of different substances *in equilibrio* of pressure and temperature; for in that case τ, $\frac{dp}{d\tau}$, and $\frac{d^2p}{d\tau^2}$ are the same for each substance. We have only to substitute for $\frac{h\mu}{M}$ the following expression,—

$$n_1\frac{h_1\mu_1}{M_1} + n_2\frac{h_2\mu_2}{M_2} + \&c. = \kappa \,.\, \Sigma(nN\mathfrak{k}), \quad . \quad . \quad . \quad (25\ B)$$

where n_1, n_2, &c. are the proportions of the different ingredients in unity of weight of the mixture, so that $n_1 + n_2 +$ &c. $= 1$.

Equation (25) agrees exactly with equation (6) in the first section of my original paper on the Theory of the Mechanical Action of Heat. It is the fundamental equation of that theory, and I shall now proceed to deduce the more important consequences from it.

(10.) *Equivalence of Heat and Expansive Power. Joule's Law.*—From the variation of the heat communicated to the body, let us subtract the variation of the expansive power given out by it, or

$$P\delta V = \{p + f(V)\}\delta V.$$

The result is the variation of the total power exercised upon, or communicated to, unity of weight of the substance, supposing that there is no chemical, electrical, magnetic, or other action except heat and pressure; and its value is

$$\delta\Psi = \delta Q + \delta Q' - P\delta V = \delta\tau \,.\left\{\mathfrak{k} + \frac{h\mu}{M}\left(\frac{1}{\tau} - \frac{\kappa}{\tau^2}\right) + (\tau - \kappa)\int \frac{d^2p}{d\tau^2}\,dV\right\}$$

$$+ \delta V \,.\left\{(\tau - \kappa)\frac{dp}{d\tau} - p - f(V)\right\}. \quad . \quad . \quad . \quad . \quad . \quad (26)$$

This expression is obviously *an exact differential,* and its integral is the following function of the volume and temperature :—

$$\Psi = \mathfrak{k}(\tau - \kappa) + \frac{h\mu}{\mathrm{M}}\left(\log_e \tau + \frac{\kappa}{\tau}\right) + \int\left\{(\tau - \kappa)\frac{dp}{d\tau} - p\right\}d\mathrm{V}$$

$$-\int f(\mathrm{V})d\mathrm{V}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

Accordingly, the total amount of power which must be exercised upon unity of weight of a substance, to make it pass from the absolute temperature τ_0 and volume V_0 to the absolute temperature τ_1 and volume V_1, is

$$\Psi(\mathrm{V}_1, \tau_1) - \Psi(\mathrm{V}_0, \tau_0).$$

This quantity consists partly of expansive or compressive power, and partly of heat, in proportions depending on the mode in which the intermediate changes of temperature and volume take place; but the total amount is independent of these changes.

Hence, *if a body be made to pass through a variety of changes of temperature and volume, and at length be brought back to its primitive volume and temperature, the algebraical sum of the portions of power applied to and evolved from the body, whether in the form of expansion and compression, or in that of heat, is equal to zero.*

This is one form of the law proved experimentally by Mr. Joule, of the equivalence of heat and mechanical power. In my original paper on the Mechanical Action of Heat, I used this law as an axiom, to assist in the investigation of the Equation of Latent Heat. I have now deduced it from the hypothesis on which my researches are based; not in order to prove the law, but to verify the correctness of the mode of investigation which I have followed.

Equations (26) and (27), like equation (23), are made applicable to unity of weight of a mixture, by putting $\Sigma n\mathfrak{k}$ for $\mathfrak{k}$, and $\Sigma n \frac{h\mu}{\mathrm{M}}$ for $\frac{h\mu}{\mathrm{M}}$.

The train of reasoning in this article is the converse of that followed by Professor William Thomson of Glasgow, in article 20 of his paper on the Dynamical Theory of Heat, where he proves from Joule's law, that the quantity corresponding to $\delta\Psi$ is an exact differential.

(11.) *Mutual Conversion of Heat and Expansive Power.* Carnot's *Law of the Action of Expansive Machines.*—If a body be made to pass from the volume V_0 and absolute temperature τ_0 to the volume V_1 and absolute temperature τ_1, and be then brought back to the original volume and temperature, the total power exerted (Ψ) will have, in those two operations, equal arithmetical values, of opposite signs. Each of the quantities Ψ

consists partly of heat and partly of expansive power, the proportion depending on the mode of intermediate variation of the volume and temperature, which is arbitrary. If the mode of variation be different in the two operations, the effect of the double operation will be to transform a portion of heat into expansive power, or *vice versâ.*

Let (a) denote the first operation, (b) the *reverse* of the second. Then

$$\Psi_b=\Psi_a.$$

The terms of Ψ which involve functions of τ only, or of V only, are not affected by the mode of intermediate variation of those quantities. The term on which the mutual conversion of heat and expansive power depends is therefore

$$\int\left\{(\tau-\kappa)\frac{dp}{d\tau}-p\right\}d\mathrm{V}(b)=\int\left\{(\tau-\kappa)\frac{dp}{d\tau}-p\right\}d\mathrm{V}(a),$$

or

$$\int\left(\frac{d\mathrm{Q}'}{d\mathrm{V}}-p\right)d\mathrm{V}(b)=\int\left(\frac{d\mathrm{Q}'}{d\mathrm{V}}-p\right)d\mathrm{V}(a).$$

Hence

$$\int\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}(a)-\int\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}(b)=\int pd\mathrm{V}(a)-\int pd\mathrm{V}(b);$$

which last quantity is the amount of the *heat transformed into expansive power,* or the total latent heat of expansion in the *double operation.*

Let

$$\int\frac{dp}{d\tau}d\mathrm{V}=\int\frac{1}{\tau-\kappa}\cdot\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}=\mathrm{F}.$$

Then, because

$$\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}=(\tau-\kappa)d\mathrm{F},$$

we have

$$\int_{\mathrm{V}_0}^{\mathrm{V}_1}pd\mathrm{V}(a)-\int_{\mathrm{V}_0}^{\mathrm{V}_1}pd\mathrm{V}(b)=\int_{\mathrm{F}_0}^{\mathrm{F}_1}(\tau-\kappa)d\mathrm{F}(a)-\int_{\mathrm{F}_0}^{\mathrm{F}_1}(\tau-\kappa)d\mathrm{F}(b)$$

$$=\int_{\mathrm{F}_0}^{\mathrm{F}_1}{}_a-\tau_b)d\mathrm{F}=\int_{\mathrm{V}_0}^{\mathrm{V}_1}\frac{\tau_a-\tau_b}{\tau_a-\kappa}\frac{d\mathrm{Q}'_a}{d\mathrm{V}}d\mathrm{V}, \quad . \quad . \quad . \quad . \quad (28)$$

in which τ_a and τ_b are the pair of absolute temperatures, in the two operations respectively, corresponding to *equal values of* F*.

This equation gives a relation between the heat transformed into expansive power by a given pair of operations on a body, the latent heat of expansion in the first operation, and the mode of variation of temperature in the two operations. It shows that the proportion of the original latent heat of expansion finally transformed into expansive power, is a function of the temperatures alone, and is therefore independent of the nature of the body employed.

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* F is what has since been called a "Heat-potential," and with the addition of the term $\int\left(\frac{k}{\tau-\kappa}+\frac{h\mu}{\mathrm{M}\tau^2}\right)d\tau$, a "Thermo-dynamic Function." (1855.)

Equation (28) includes Carnot's law as a particular case. Let the limits of variation of temperature and volume be made indefinitely small. Then

$$dp\,d\mathrm{V} = \frac{d\tau}{\tau-\kappa}\cdot\frac{d\mathrm{Q}'}{d\mathrm{V}}\,d\mathrm{V};$$

and dividing by $d\tau d\mathrm{V}$,

$$\frac{dp}{d\tau} = \frac{1}{\tau-\kappa}\cdot\frac{d\mathrm{Q}'}{d\mathrm{V}}.$$

This differential equation is also an immediate consequence of equation (25).

If $\frac{\mu}{\mathrm{J}}$ be put for $\frac{1}{\tau-\kappa}$, and JM for $\frac{d\mathrm{Q}'}{d\mathrm{V}}$, it becomes identical with the equation by which Professor William Thomson expresses Carnot's law, as deduced by him and by M. Clausius from the principle, that *it is impossible to transfer heat from a colder to a hotter body, without expenditure of mechanical power.*

The investigation which I have now given is identical in principle with that in the fifth section of my paper on the Mechanical Action of Heat, but the result is expressed in a more comprehensive form.

Equation (28), like (25), (26) and (27), is applicable to a mixture composed of any number of different substances, in any proportions, provided the temperature, the pressure, and the coefficients $\frac{dp}{d\tau}$, $\frac{d^2p}{d\tau^2}$ are the same throughout the mass.

(12.) *Apparent Specific Heat.*—The general value of apparent specific heat of unity of weight is

$$\mathrm{K} = \frac{d\mathrm{Q}}{d\tau} + \frac{d\mathrm{Q}'}{d\tau} + \frac{d\mathrm{Q}'}{d\mathrm{V}}\cdot\frac{d\mathrm{V}}{d\tau} = \mathfrak{k} + (\tau-\kappa)$$

$$\left\{\frac{h\mu}{\mathrm{M}\tau^2} + \int\frac{d^2p}{d\tau^2}\,d\mathrm{V} + \frac{d\mathrm{V}}{d\tau}\cdot\frac{dp}{d\tau}\right\}. \quad . \quad . \quad (29)$$

agreeing with equation (13) of my previous paper.

The value in each particular case depends on the mode of variation of volume with temperature. Specific heat at constant volume is

$$\mathrm{K}_{\mathrm{V}} = \mathfrak{k} + (\tau-\kappa)\left(\frac{h\mu}{\mathrm{M}\tau^2} + \int\frac{d^2p}{d\tau^2}\,d\mathrm{V}\right). \quad . \quad . \quad (30)$$

When the pressure is constant, we must have

$$\frac{d\mathrm{P}}{d\mathrm{V}}\,d\mathrm{V} + \frac{dp}{d\tau}\,d\tau = 0;$$

and, consequently,

$$\frac{dV}{d\tau} = -\frac{\frac{dp}{d\tau}}{\frac{dP}{dV}};$$

therefore specific heat at constant pressure is

$$K_P = K_V + (\tau - \kappa)\frac{\left(\frac{dp}{d\tau}\right)^2}{-\frac{dP}{dV}}. \quad . \quad . \quad . \quad . \quad (31)$$

This agrees with equation (16) of Professor Thomson's paper, if $\frac{J}{\mu}$ in his notation $= \tau - \kappa$.

If the body be a perfect gas, then

$$\left.\begin{aligned} K &= \frac{V_0}{\tau_0}\left(\frac{kM}{2\mu} + \frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2} + \frac{\tau-\kappa}{V}\cdot\frac{dV}{d\tau}\right) = \mathfrak{k}\left\{1 + N(\tau-\kappa)\left(\frac{\kappa}{\tau^2} + \frac{dV}{Vd\tau}\right)\right\} \\ K_V &= \frac{V_0}{\tau_0}\left(\frac{kM}{2\mu} + \frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right) = \mathfrak{k}\left\{1 + N\left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right)\right\} \\ K_P &= K_V + \frac{V_0}{\tau_0}\left(1 - \frac{\kappa}{\tau}\right) = \frac{V_0}{\tau_0}\left(\frac{kM}{2\mu} + 1 - \frac{\kappa^2}{\tau^2}\right) = \mathfrak{k}\left\{1 + N\left(1 - \frac{\kappa^2}{\tau^2}\right)\right\} \end{aligned}\right\} . (3$$

The fact that the specific heats of all nearly perfect gases for unity of weight are inversely proportional to their specific gravities, shows that $\frac{kM}{2\mu}$ is the same for each of them*.

(13.) *Velocity of Sound in Fluids.*—Let a denote the velocity of sound in a fluid, and $d\,.\,P$ the total differential of the pressure. Then

$$a = \sqrt{\left(g\,.\,\frac{d\,.\,P}{d\,.\,\frac{1}{V}}\right)} = \sqrt{\left\{gV^2\left(-\frac{dP}{dV} - \frac{dP}{d\tau}\cdot\frac{d\tau}{dV}\right)\right\}}. \quad . \quad (33)$$

If it were possible to maintain the temperature of each particle of the fluid invariable during the passage of sound, this velocity would be simply

$$\sqrt{\left(g\,.\,\frac{dP}{d\,.\,\frac{1}{V}}\right)}.$$

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* The fact, recently ascertained by M. Regnault, that the specific heat of atmospheric air at constant pressure is sensibly constant throughout a great range of temperature, proves that at ordinary temperatures $\frac{\kappa^2}{\tau^2}$ is an inappreciably small fraction. (December 1853.) The specific heat of atmospheric air was accurately predicted by means of these equations, three years before M. Regnault ascertained it experimentally.

But we have reason to believe that there is not time, during the passage of sound, for an appreciable transfer of heat from atom to atom; so that for each particle

$$dQ + dQ' = 0; \text{ or, } K = 0 \text{ in equation (29).}$$

To fulfil this condition, we must have

$$\frac{d\tau}{dV} = -\frac{\tau - \kappa}{K_V} \cdot \frac{dp}{d\tau}.$$

Consequently,

$$a = \sqrt{\left\{ g V^2 \left(-\frac{dP}{dV} + \frac{\tau - \kappa}{K_V} \cdot \left(\frac{dp}{d\tau}\right)^2 \right) \right\}};$$

or by equation (31),

$$a = \sqrt{\left(g \frac{dP}{d \cdot \frac{1}{V}} \cdot \frac{K_P}{K_V} \right)}. \qquad (34)$$

That is to say, *the action of heat increases the velocity of sound in a fluid, beyond what it would be if heat did not act, in the ratio of the square root of the specific heat at constant pressure to the square root of the specific heat at constant volume.*

This is Laplace's law of the propagation of sound; which is here shown to be applicable, not only to perfect gases, but to all fluids whatsoever*.

General Note, September 1855.

All that is said in articles (9.) and (10.) of mixtures of atoms of different substances *in equilibrio* of pressure and temperature, is applicable also to mixtures of portions of the same substance in two different conditions, *e. g.* the liquid and the vapourous; and thus from equation (25) it is found, that the latent heat of evaporation of unity of weight of a given fluid is represented by

$$\delta Q' = (\tau - \kappa) \cdot \frac{dp}{d\tau} \delta V,$$

where p is the pressure of the vapour in contact with its liquid at the absolute temperature τ, and δV is the increase of volume undergone by unity of weight of the fluid in the act of evaporating. If for $\tau - \kappa$ be put $\frac{J}{\mu}$, as before, this equation is transformed into that deduced by Messrs. Clausius and Thomson from Carnot's principle.

The recent experiments of Mr. Joule and Professor William

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* For experimental verifications of this law, see the Philosophical Magazine for June 1853, and the Transactions of the Royal Society of Edinburgh, vol. xx. pp. 588, 589. (1853.)

Thomson (Phil. Trans. 1854), have shown that the quantity denoted by κ is practically inappreciable; so that both this quantity, and the terms containing h as a factor, may be neglected in equation (25) and those which follow it, until there is experimental evidence of their having sensible values.

In the preceding paper, the laws of the expansive action of heat are deduced from a mechanical hypothesis, called that of Molecular Vortices. Those laws are capable of being expressed and proved independently of any hypothesis (see Phil. Trans. 1854, part 1, and a paper "On the Science of Energetics," Edinb. Phil. Journ. July 1855); but it is nevertheless considered, that a molecular hypothesis which has already led to the anticipation of some laws subsequently confirmed by experiment, may possibly lead hereafter to the anticipation of more such laws, and may at all events be regarded as interesting in a mathematical point of view; although its objective reality, like that of other molecular hypotheses, be incapable of absolute proof.

LVIII. *On the Felspars occurring in the Granites of the Dublin and Wicklow Mountains. By the* Rev. Joseph A. Galbraith, *Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the last Number of your Journal, Mr. Jennings observes that I am incorrect in stating that it is necessary to determine the quantities of silex, alumina, &c. in a rock or mineral in order to obtain the alkalies which it contains. I beg leave to say that I never made such a statement; I certainly did observe, in reference to a single and incomplete analysis which he furnished in order to settle an important question, that where accuracy was an object, it was most desirable to have a complete analysis, in which each constituent was weighed, and this for the obvious reason, that if all the parts, reduced to per-centages, on being added together yielded one hundred, the utmost confidence might be placed in a result which thus checked itself: in the justice of this statement I am sure that every chemist who reads your Journal will acquiesce.

With regard to the mineral constitution of the granite rocks of the Dublin and Wicklow chain, the question at issue is that which was raised about three years ago by Sir Robert Kane, when he stated to the Royal Irish Academy that the soda in these granites was so preponderant over the potash, as to lead to the conclusion that the potash should be referred to the mica which the granite contained, and that the felspar was an albitic or soda felspar. Considerable doubts having been expressed as to the accuracy of this statement, I undertook to search this mountain chain for felspar crystals, and having selected seven specimens from widely distant localities, I submitted them to analysis, and sent the results to your Journal, in the January Number of which for the present year they were published; the average ratio of the potash to the soda in these felspars was 4·5 : 1, a result which differed so widely from Sir Robert Kane's statement, that I expressed my inability to conceive how the felspar which entered into the constitution of the rock could be destitute of potash. I have preserved the specimens, and shall have great pleasure in allowing Mr. Jennings to examine them at any time he chooses.

With a view to the direct determination of this question, Professor Haughton undertook the analysis of a series of *rock specimens* selected from different localities in the range, and arrived at a result totally at variance with Sir Robert Kane's assertion; the average result of his analyses, as published in the July Number of your Journal, gives for the ratio of the potash to the soda in the rock specimens taken from the main chain, 4·822 : 2·967, or in round numbers 5 : 3. Professor Haughton's analyses prove that the felspar, as found in large crystals, differs from that which enters into the constitution of the rock in this respect, namely, that while potash predominates in both cases over the soda, the ratio in the latter case is less than in the former.

The single analysis furnished by Mr. Jennings, although it does not fall in perfectly with Professor Haughton's results, must be pronounced to be equally inconsistent with Sir Robert Kane's statement, which requires us to believe that the felspar of the rock is destitute of potash. This is a complete statement of the question so far as facts have come to light; and I am inclined to think that if Sir Robert Kane still remains silent, the scientific public, unable any longer to withhold their judgement, will pronounce this question to be settled against him. Why he should maintain this reserve on a purely scientific question I am at a loss to understand, as he has had ample time to make good the promise which he volunteered to the Royal Irish Academy in February 1853. I find, moreover, on referring to public documents, that he has made considerable progress in this investigation. In the Report of the Museum of Irish Industry, made 26th January, 1854, he says, "The chemical officers of the Museum have been occupied with a very extensive series of analyses (still in progress) of the felspathic and other alkaliferous rocks of the mountain ranges south of Dublin; of this investigation, twenty-four detailed rock analyses have been already completed." In a return to an order of the House of Commons on a motion made by Mr. George Alexander Hamilton, bearing date 3rd May, 1854, I find the following statement:—"Considerable progress has been made with a very extended investigation into the composition of the felspathic and hornblendic rocks of the county Wicklow; the analytical portions of this work, perhaps the most laborious and extensive of its kind ever undertaken, will include between 400 and 500 quantitative analyses of rock specimens." In the Report to the Board of Trade, 16th January of the present year, it is again represented that "in the chemical department of the museum the examination of the felspathic rocks, &c. has made considerable progress."

When we consider that this investigation was undertaken by Sir Robert Kane in a public capacity, and in conjunction with the officers of the Geological Survey, it must be allowed that the public have a right to ask for information on the subject; and when we consider that Sir Robert Kane publicly pledged himself to produce his analyses, we must conclude that he is bound as a man of science to redeem his pledge and substantiate his statement.

I remain, Gentlemen,
Your obedient Servant,
JOSEPH A. GALBRAITH.

Trinity College, Dublin,
November 14, 1855.

LIX. *On Reciprocal Molecular Induction.*

To Prof. W. Thomson, F.R.S.

MY DEAR SIR, Royal Institution, Nov. 26, 1855.

THE communication from Professor Weber which appears in the present Number of the Philosophical Magazine, has reminded me, almost too late, of your own interesting letter on the same subject published in the April Number of this Journal. A desire to finish all I have to say upon this question at present induces me to make the following remarks, which, had it not been for the circumstance just alluded to, might have been indefinitely deferred.

With reference to the mutual action of a row of bismuth particles, you say that "it is perfectly easy to demonstrate that it *must* be such as to impair the 'diamagnetization' when the line of the row is parallel to the lines of force" (the 'must,' you will remember, is put in italics by yourself). From this you infer, that in a uniform field of force a bar of bismuth would set its length along the lines of force. Further on it is stated that this action is "demonstrated with as much certainty as the parallelogram of forces;" and you conclude your letter by observing that "the assertions which I [yourself] have made are demonstrable in every case without special experiment, ... and are absolutely incontrovertible, as well as incapable of verification, by experiment or observation on diamagnetics."

Most of what I have to say upon this subject condenses itself into one question.

Supposing a cylinder of bismuth to be placed within a helix, and surrounded by an electric current of sufficient intensity; can you say, *with certainty*, what the action of either end of that cylinder would be on an external fragment of bismuth presented to it?

If you can, I, for my part, shall rejoice to learn the process by which such certainty is attained: but if you cannot, it will, I think, be evident to you that the verb "*must*" is logically "defective."

We *know* that magnetized iron attracts iron: we *know* that magnetized iron repels bismuth: this, so far as I can see, is your only experimental ground for *assuming* that magnetized *bismuth* repels bismuth, and yet you affirm that an action deduced from this assumption "is demonstrated with as much certainty as the parallelogram of forces." Do I not state the question fairly? I can, at all events, answer for my earnest wish to do so.

It is needless to remind one so well acquainted with the mental experience of the scientific inquirer, that the very letters which you attach to your sketch, page 291, may tempt us to an act of abstraction—a forgetfulness of a possible physical difference between the *n* of iron and the *n* of bismuth—which may lead us very wide of the truth. The very term 'pole' often pledges us to a theoretic conception without our being conscious of it. You are also well aware of the danger of shutting the door against experimental inquiry on an unpromising subject; and when you apparently do this in your concluding paragraph, I simply accept it as a strong way of expressing your personal conviction, that the action referred to is too feeble to be rendered sensible by experiment.

Believe me, dear Sir,
Most truly yours,
JOHN TYNDALL.

LX. *On the supposed Influence of the Hot-blast in augmenting the quantity of Phosphorus in Pig-iron. By* DAVID S. PRICE, *Ph.D., F.C.S.; and* E. CHAMBERS NICHOLSON[*].

THE employment of the hot-blast in the smelting of iron is admitted to occasion the production of pig-iron of inferior quality, that is to say, contaminated with larger amounts of foreign elements than that smelted with cold-blast.

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[*] Communicated by the Authors. This paper was read in part at the British Association, Liverpool, 1854.

In the present communication it is not our intention to enter upon the discussion of this subject generally, as we intend reserving this for a future communication, but to limit our remarks to the consideration of the supposed influence of hot-blast in augmenting the quantity of phosphorus, an element of almost constant occurrence in pig-iron, and to the presence of which in bar-iron the peculiar property of the metal known as *cold shortness* is attributed.

In a paper published in the Quarterly Journal of the Chemical Society*, and also in one read before the British Association in 1849†, by Mr. Wrightson, several analyses of cast iron, the produce of Staffordshire furnaces worked by hot, warm, and cold blast, are given to prove that a larger amount of phosphoric acid is reduced when hot-blast is employed.

The increase in the per-centage of phosphorus in the iron smelted with hot- over that with cold-blast is exhibited in the following series of Mr. Wrightson's results:—

	1.	2.	3.	4.	5.	6.	7.	8.
Hot-blast	0·51	0·55	0·50	0·71	0·54	......	0·07	0·40
Cold-blast	0·47	0·41	0·31	0·20	0·21	0·36	0·03	0·36
Increase of P in hot-blast......	0·04	0·14	0·19	0·51	0·33	......	0·04	0·04

The ores from which the iron was smelted were also analysed, and found to contain the following per-centages of phosphoric acid:—

Binds.	Blue flats.	Penny earth.	Gubbin.	White iron stone.	White free.	Black free.
traces.	traces.	1·00	0·32	0·95	0·90	trace.

The increase in the amount of phosphorus indicated in the above table as occurring in hot-blast iron, does not, in the absence of a knowledge of the relative quantities of the respective ores employed, which vary as much as 1 per cent. in the quantity of phosphoric acid which they contain, and also of an examination of the blast-furnace slag produced, appear to afford sufficient proof in support of this opinion.

Karsten ‡, in speaking of bog-iron ore, says that, when smelted, the whole of the phosphate of iron is reduced to the state of phosphide and absorbed by the iron. Berthier§, from experiments conducted on a small scale, does not coincide with this view, and considers that the difficulty of detecting small quantities of phosphoric acid must account for its not appearing amongst the constituents in the various analyses of blast-furnace slags which he has described in his work.

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* Vol. i. p. 330. † Chem. Gaz. vol. vii. p. 478.

‡ *Handbuch der Eisenhüttenkunde*, vol. ii. § 368. Berlin, 1841.

§ *Traité des Essais par la Voie sèche*, vol. ii. p. 262.

From experiments made by assaying pure hæmatite with variable per-centages of phosphate of lime* and suitable proportions of flux and charcoal, we have, by analysing the buttons of metal and slags obtained, taking care to select those only in which the operation was perfect, that is to say, in which a colourless slag and graphitous button were produced, also corroborated Berthier's results, as will be apparent from the following table:—

Pure peroxide of iron.	Phosphate of lime.	Flux.	Coke.	Per-centage of phosphorus in button.	
				Calculated.	Found.
1. 10·0 grms.	0·25 grms.	10·0 grms.	2·25 grms.	0·60	0·56
2. 10·0 ...	0·75 ...	10·0 ...	2·25 ...	1·83	1·60
3. 10·0 ...	2·50 ...	10·0 ...	2·50 ...	6·20	2·60
4. 10·0 ...	5·00 ...	10·0 ...	2·50 ...	12·60	6·00

In the assay of ores, and also of scoriæ from forge- and mill-furnaces, which contain a large amount of phosphoric acid, results widely different to the foregoing were arrived at. The following, selected from a great number of experiments, are marked illustrations of this:—

		Lime.	Coke.	Flux.	Per-centage of phosphorus in button.	
					Calculated.	Found.
	grms.	grms.	grms.	grms.		
5. Welch black-band ordinary quality—calcined	10·0	3·5	1·75	......	0·82	0·81
6. Argillaceous iron ore—calcined, very rich in phosphoric acid	10·0	1·5	1·20	......	6·60	6·41
7. Brown hæmatite, also rich in phosphoric acid	10·0	......	......	10·0	6·90	6·70
8. Scoria from puddling-furnace	10·0	......	2·0	10·0	13·6	12·5
9. Scoria from balling-furnace...	10·0	......	2·0	10·0	2·27	2·25

The time occupied in performing the assays, and the furnace conditions under which they were conducted, were about the same in all cases. Where flux was employed, it consisted of two parts of clay-shale and one part of lime.

It is necessary to state, that the phosphoric acid in the ores above cited was in combination with lime. In the cinders it existed as phosphate of iron.

In experiments Nos. 1 and 2, it will be seen that the quantity of phosphorus found agrees pretty well with the theoretical amount, whilst in 3 and 4 it falls considerably short. That this is owing to the length of time during which the reduction process is carried on we have no doubt, as we have frequently repeated the assays with the same proportions, and have found the amount of phosphorus in the button to vary considerably, never having succeeded in obtaining more than four-fifths of the total quantity. If, however, the cementation were prolonged for a sufficient length of time, it is very probable that the whole of the phosphoric acid would be reduced.

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* The amount of phosphoric acid in the phosphate of lime was previously determined.

We have undertaken many experiments upon the large scale with the view of deciding this point. For this purpose we have determined the amount of phosphorus in iron that had been smelted from argillaceous ores by cold-blast, and by a blast heated to 600° F.

The following are the results:—

	I.	II.	III.	IV.
Hot-blast . . .	0·74	0·68	0·71	0·58
	V.	VI.	VII.	VIII.*
Cold-blast . . .	0·81	0·62	0·68	0·63

The iron in both cases was what is known as good No. 2 foundry pig.

In two instances it will be seen that the per-centage of phosphorus is higher in the cold-blast iron than in the hot; but the difference in these and in the other two is so slight, that it may fairly be attributed to the variations in the composition of the ore.

The slags produced simultaneously with four of the above irons were examined for phosphoric acid by the usual methods, as well as by molybdate of ammonia; and it was only by the latter reagent that we were able to find minute traces. We append the analyses of the slags:—

	I.	II.	V.	VI.
Silicic acid	39·95	40·20	41·64	42·94
Alumina	17·41	16·45	13·20	16·29
Lime	29·64	30·00	35·91	31·10
Magnesia	6·47	7·29	4·21	4·16
Protoxide of iron . . .	0·24	0·57	0·11	0·34
Protoxide of manganese .	0·91	0·84	0·74	0·51
Sulphide of calcium . .	3·60	2·71	2·19	2·16
Alkalies	1·46	1·30	1·70	1·87
Phosphoric acid . . .	trace	trace	trace	trace
Loss	0·32	0·64	0·30	0·63
	100·00	100·00	100·00	100·00

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* These correspond with the per-centage amounts of phosphorus calculated from the analyses of the ores, that the pig-iron should contain if all the phosphoric acid were reduced.

As far as our experience enables us to judge, we incline to the opinion that when the process of reduction is complete, or nearly so, that is to say, when no oxide of iron or small quantities only pass off with the slag, that then the whole of the phosphoric acid is reduced, and the phosphorus absorbed by the iron independently of the temperature of the blast. The analyses above given prove such to be the case with the iron smelted from the ores ordinarily employed in this country, in which the amount of phosphoric acid seldom exceeds 1 per cent.

With ores and scoriæ containing large amounts of phosphoric acid, we have also had opportunities of proving that when smelted with hot-blast, all the phosphoric acid is reduced, and the phosphorus absorbed by the iron; this we have found to be the case with ores containing from 2 to 3 per cent. of phosphoric acid, and with scoriæ with as much as from 8 to 10 per cent; but we have not had an opportunity of examining a product smelted with cold-blast from similar materials.

The following exhibits the per-centage of phosphorus, (IX.) in gray pig-iron smelted with hot-blast from pisolitic iron ore, (X.) of gray pig-iron smelted with hot-blast from puddling furnace scoriæ and clay-shale:—

IX.	X.
2·56	6·94

The slags respectively made with these products had the following composition:—

	IX.	X.
Silicic acid	45·64	41·11
Alumina	10·84	9·46
Lime	35·01	37·90
Magnesia	3·16	2·11
Protoxide of iron . .	0·71	0·39
Protoxide of manganese .	trace	1·61
Sulphide of calcium . .	3·30	6·41
Alkalies	0·82	0·71
Phosphoric acid . . .	trace	trace
Loss	0·52	0·30
	100·00	100·00

Phosphoric acid is present in blast-furnace slags when white iron is being smelted, that is, when the slag contains appreciable quantities of protoxide of iron, as will be seen by the following analyses.

	XI.	XII.
Silicic acid	41·11	37·84
Alumina	13·45	13·20
Lime	29·82	20·68
Magnesia	4·75	2·93
Protoxide of iron	6·44	20·83
Protoxide of manganese	0·66	0·80
Alkalies	1·84	1·08
Sulphide of calcium	1·34	0·87
Phosphoric acid	0·15	1·77
Loss	0·44	0·05
	100·00	100·00

No. XI. is that of a slag resembling black bottle-glass in appearance, and from its liquidity when melted is termed by the workmen a *scouring slag*. It was from argillaceous ore.

No. XII. from pisolitic ore when the working of the furnace was much deranged. This slag was exceedingly heavy, of a pitch-black colour, with the surface of the blocks in the *tap wagons* of the dull, dark red, bronze-like aspect characteristic of very bad furnace slags.

From these results we must regard the ore as being melted up (not smelted) and flowing away with the slags, although in very different degrees in the two examples given.

The analyses of crystalline slags by Percy and D. Forbes*, in all of which phosphoric acid was sought, lead to the same inference, phosphoric acid having been discovered and estimated in only one instance, and that in a slag similar to XI. It contained FeO 4·94, and PO^5 0·19.

In conclusion we will briefly recapitulate the results of our experiments.

1st. That in assaying ores, all the phosphorus of the phosphates will be found in the button.

2nd. That when the ordinary iron ores, such as the argillaceous ores, black bands, hæmatites, &c. are smelted, the iron produced, if it be gray, will contain all the phosphorus of the ore, whether the furnace be driven with hot or cold blast.

Lastly. That the slag may contain phosphoric acid in determinable quantity when white iron is being smelted.

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* British Association Report, 1846; and Chem. Gaz. vol. v. p. 29.

LXI. *Some Considerations respecting the Lunar Origin of Aërolites.* *By* R. P. Greg, *Esq.**

DR. LAWRENCE SMITH of Louisville University, U.S., has just published in the American Journal of Science, Second Series, vol. xix. May 1855, an interesting memoir on Meteorites, which deserves attention even from those who may not agree with the theory of their *lunar* origin. He directs attention to the physical, chemical as well as mineralogical, characteristics of aërolites, pointing out the volcanic and igneous nature which some of them possess. He agrees with me in the importance of separating these bodies from shooting stars and periodic luminous meteors, a circumstance which no astronomer except Olmsted has noticed or valued. He rejects their atmospheric origin, and considers them as certainly belonging to, or as having proceeded from, a larger whole, and not to have resulted from the condensation of so many independent cosmical particles. He then comes to the *lunar* theory, and after giving its history and naming the principal advocates of it†, lays down the following propositions:—

"1st. That all meteoric masses have a community of origin.

"2nd. At one period they formed parts of some large body.

"3rd. They have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes.

"4th. That their source must be deficient in oxygen.

"5th. That their average specific gravity is about that of the moon.

"From what has been said under the head of common characters of meteorites, it would appear far more singular that these bodies should have been formed separately from each other, than that they should have at one time or another constituted parts of the same body; and from the character of their formation, that body should have been of great dimensions. Let us suppose all the known meteorites assembled in one mass, and regarded by the philosopher, mindful of our knowledge of chemical and physical laws. Would it be considered more rational to view them as the great representatives of some one body that had been broken into fragments, or as small specks of some vast body in space that at one period or another has cast them forth? The latter, it seems to me, is the only opinion that can be entertained in reviewing the facts of the case.

"As regards the igneous character of the minerals composing meteorites, nothing remains to be added to what has already been said; in fact no mineralogist can dispute the great resemblance of these minerals to those of terrestrial volcanoes, they having only sufficient difference in association, to establish that, although igneous, they are extra-terrestrial. The source must also be deficient in oxygen, either in a gaseous condition or combined as in water: the reasons for so thinking have been clearly stated as dependent upon the existence of *metallic iron* in meteorites; a metal so oxidizable, that in its terrestrial associations it is almost always found combined with oxygen, and never in its metallic state.

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* Communicated by the Author.

† It was proposed by an Italian philosopher, Terzago, in 1660, and has been at different times, and for various reasons, supported by Olbers, Biot, Brandes, Poisson, Quetelet, Arago, Benzenburg and Laplace.

"What then is that body which is to claim common parentage of these celestial messengers that visit us from time to time? Are we to look at them as fragments of some shattered planet whose great representatives are the thirty-three asteroids between Mars and Jupiter, and that they are 'minute outriders of the asteroids' (to use the language of Mr. R. P. Greg, in a late communication to the British Association) which have been ultimately drawn from their path by the attraction of the earth? For more reasons than one this view is not tenable; many of our most distinguished astronomers do not regard the asteroids as fragments of a shattered planet; and it is hard to believe if they were, and the meteorites the smaller fragments, that these latter should resemble each other so closely in their composition; a circumstance that would not be realized if our earth was shattered into a million of masses large and small.

"If then we leave the asteroids and look to the other planets, we find nothing in their constitution, or the circumstances attending them, to lead to any rational supposition as to their being the original habitation of the class of bodies in question. This leaves us then but the *moon* to look to as the parent of meteorites, and the more I contemplate that body, the stronger does the conviction grow, that to it all these bodies originally belonged."

Dr. Smith then notices the similarity existing between the respective densities of the moon and aërolites, but does not lay great weight on that point; though he thinks their chemical composition a strong ground in favour of their lunar origin. He goes on to say,—

"Laplace's view of the matter was connected with present volcanic action in the moon, but there is every reason to believe that all such action has long since ceased in the moon. This, however, does not invalidate this theory in the least, for the force of projection and modified attraction to which the detached masses were subjected, only gave them new and independent orbits around the earth, that may endure for a great length of time before coming in contact with the earth.

"The various astronomers cited concur in the opinion, that a body projected from the moon with the velocity of about 8000 feet per second, would go beyond the mutual point of attraction between the earth and moon, and already having an orbital velocity, may become a satellite of the earth with a modified orbit.

"The important question then for consideration is, the force requisite to produce this velocity. The force exercised in terrestrial volcanoes varies. According to Dr. Peters, who made observations on Ætna, the velocity of some of the stones was 1250 feet a second, and observations made on the peak of Teneriffe gave 3000 feet a second. Assuming, however, the former velocity to be the maximum of terrestrial volcanic effects, the velocity with which the bodies started (stones with a specific gravity of about 3·00) must have exceeded 2000 feet a second to permit of an absorbed velocity of 1250 feet through the denser portions of our atmosphere. Now suppose the force of the extinct volcanoes of the moon to have equalled that of Ætna, the force would have been more than sufficient to have projected masses of matter at a velocity exceeding 8000 feet a second; for the resistance to be overcome by the projectile force, is the attractive force of the moon, which is from five to six times less than that of the earth, so that the same projectile force in the two bodies would produce vastly greater velocities on the moon than on the earth, discarding of course atmospheric resistance, of which there is none in the moon*."

In the following, I think Dr. Smith, as he does in several of his arguments, rather begs the question in his anxiety to make the moon's position and physical powers favour as much as possible the production and discharge of aërolites to the earth. It would appear to me more reasonable and consistent with our ideas of terrestrial volcanoes to suppose, that the total absence of water and atmosphere in the moon is favourable to the notion, that volcanic action there is of a less violent and explosive character than on the earth. Dr. Smith says, however,—

"But doubtless, were the truth of the matter known, the projectile force of lunar volcanoes far exceeded that of any terrestrial volcanoes extinct or recent, and this we infer from the enormous craters of elevation to be seen upon its surface, and their great elevation above the general surface of the moon, with their borders thousands of feet above their centre; all of which point to the immense internal force required to elevate the melted lava that must have at one time poured from their sides. I know that Prof. Dana, in a learned paper on the subject of lunar volcanoes (Am. J. Sci. [2] ii. 375), argues that the great breadth of the craters is no evidence of great projectile force, the pits being regarded as boiling craters where force for lofty projection could not accumulate. Although his hypothesis is ingeniously sustained, still, until stronger proof is urged, we are justified, I think, in assuming the contrary to be true, for we must not measure the convulsive throes of nature at all periods by what our limited experience has enabled us to witness.

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* The editors of the American Journal here insert the following in a foot-note:—

"It would require at the moon the same force to produce an *initial* velocity of 8000 feet a second as at the earth; and the difference of rate at the end of the first second would be slight (discarding from consideration the atmosphere).—Eds."

"As regards the existence of volcanic action in the moon without air or water, I have nothing at present to do, particularly as those who have studied volcanic action concede that neither of these agents is absolutely required to produce it; moreover, the surface of the moon is the strongest evidence we have in favour of its occurring under those circumstances."

Doubtless volcanic action has been highly developed at the surface of the moon, but in the absence of all water, we may conclude that lava floods have rather been emitted from her volcanoes, than discharges of stones and ashes.

It is still considered by some astronomers that the moon is not altogether without an atmosphere; though that be of small extent, it may nevertheless be rich in oxygen. It would be difficult to suppose that the extensive volcanic action which has evidently taken place in the moon, could have been exerted or maintained without the presence of oxygen; and if we admit that aërolites come from that body, we must necessarily concede there the existence of oxygen, since most aërolites contain a considerable quantity of silica, magnesia and alumina.

After all, the scarcity of oxygen where meteorites originate, a fact in itself highly probable, as based on the non-oxidation of the iron, nickel and phosphorus, if true, does not *prove* they proceed from the moon, but merely that they come from some place deficient in oxygen.

If meteoric masses of native iron really come from the moon, their non-oxidation might arise more from the absence of aqueous vapour than from an absence or deficiency of oxygen. While fairly admitting that some aërolites have proceeded from lunar volcanoes, because such have all the characters of erupted volcanic rocks, as those of Juvenas, Weston and Bishopville, it by no means follows, as Dr. Smith would argue, that *all* meteoric masses, even iron ones, also come from the same source or place; for in many aërolites and meteoric irons there is little if anything of a volcanic character.

We cannot reasonably suppose that lunar volcanoes have ejected enormous masses of iron, whether in a pure or oxidized state, when iron occurs in such small quantity and so rarely as the product of terrestrial volcanoes, and then most frequently deposited by sublimation. The density of the moon, as given by Dr. Smith, is only 3·6, while that of the earth is 5·6; this renders it still more improbable that substances of *greater* density, as the metals, are more abundantly ejected from the volcanoes in the moon, than from those of the earth, the latter body having the greater average density. I would also observe, that the metal *nickel*, present in almost all known aërolites and iron masses, has never yet been observed as a direct product of our volcanoes; this is not, however, an argument of much moment one way or the other, especially as that metal, in the form of red nickel, is known to occur in grauwacke at Reichelsdorf in Hessia*.

The argument, however, against the first proposition of Dr. Smith, that "all meteoric masses have a community of origin," (and militating therefore against his conclusion that that common origin is the moon's volcanoes,) which may be most forcibly illustrated, is where we consider the case of an iron mass weighing from 10 to 20 tons, as those from Durango and Rio de la Plata; for it is at once evident that no ordinary initial volcanic force could ever project such ponderous masses beyond the point or limit of the mutual attractions of the moon and earth. The calculations which have been made respecting the velocity of stones projected from Ætna or Teneriffe, are based on the supposition that such stones are of moderate size, having a density of only 3·0, or nearly three times less than that of iron.

I believe I am speaking within bounds when I state, that no stone weighing more than 100 lbs. has ever been ejected from the above-named volcanoes by a force, which, if exerted at the moon's surface, would allow of its reaching the desired point of neutralized attractions.

Dr. Daubeny states that the stones which overwhelmed Pompeii did not weigh more than 8 lbs.; and I myself can bear witness that the largest erupted blocks which crop out from under the lava of Mount Somma, and much nearer the central cone therefore than Pompeii, seldom exceeded 50 lbs. in weight. It can assuredly then only be stones of very moderate size, say of some 5 or 10 lbs., which could in any case, reasonably and practically speaking, reach or pass that limit where the *superior* attraction of the moon herself is lost.

It surely would make a material difference in our calculations, whether a mass projected from a volcano in the moon weighed 30 lbs. or 30,000 lbs.; but such a difference Dr. Smith seems entirely to have overlooked; it is sufficiently great, however, in the present state of our knowledge of this subject, to over-rule the possibility that the larger iron meteoric masses can have a *lunar origin*.

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* I have a fine specimen in my cabinet of minerals from that locality, the matrix very much resembling some meteoric stones.

Dr. Smith gives us the result of some interesting experiments, to prove the fallacy of judging of the actual size of meteors by their apparent size; I shall again quote his own words:—

"In my experiments, three solid bodies in a state of vigorous incandescence were used: 1st, charcoal points transmitting electricity; 2ndly, lime heated by the oxy-hydrogen blow-pipe; 3rdly, steel in a state of incandescence in a stream of oxygen gas. They were observed on a clear night at different distances, and the body of light (without the bordering rays) compared with the disk of the moon, then nearly full, and 45° above the horizon. Without going into details of the experiment the results will be tabulated.

	Actual diam. as seen at 10 in.	Apparent diam. at 200 yards.	Apparent diam. at ¼ mile.	Apparent diam. at ½ mile.
Carbon points	$\frac{3}{10}$ of an inch,	$\frac{1}{2}$ the diam. moon's disc,	3 diam. do.	$3\frac{1}{2}$ diam. do.
Lime light . .	$\frac{4}{10}$ ...	$\frac{1}{3}$ ...	2 ...	2 ...
Incandes. steel	$\frac{2}{10}$ ...	$\frac{1}{4}$ ...	1 ...	1 ...

"If then the apparent diameter of a luminous meteor at a given distance is to be accepted as a guide for calculating the real size of these bodies, the

Charcoal points would be	80	feet in diam. instead of	$\frac{3}{10}$	of an in.
Lime	50	...	$\frac{4}{10}$	...
The steel globule ...	25	...	$\frac{2}{10}$	...

"I need not here enter into any explanation of these deceptive appearances, for they are well-known facts, and were tried in the present form only to give precision to the criticism on the supposed size of these bodies."

Dr. Smith is evidently anxious to reduce to a minimum the size of the lunar aërolites, and proceeds a little further on to say:—

"This then will conclude what I have to say in contradiction to the supposition of large solid cosmical bodies passing through the atmosphere, and dropping small portions of their mass. The contradiction is seen to be based; first, upon the fact that no meteorite is known of any very great size, none larger than the granite balls to be found at the Dardanelles along side of the pieces of ordnance from which they are discharged; secondly, on the fallacy of estimating the actual size of these bodies from their apparent size; and lastly from its being opposed to all the laws of chance, that these bodies should have been passing through an atmosphere for ages and none have yet encountered the body of the earth."

It is not strictly true that no meteorite is known of any great size, *i. e.* not larger than the well-known cannon-balls of granite at the Dardanelles, for one or two of the larger meteoric iron masses have been described by travellers as being 7 feet in length and weighing 15 tons. It would be improbable that such a mass could be projected beyond the mouth of either a lunar or terrestrial volcano, much less reach a height of several thousand feet.

The following calculations will show that a mass of iron, having a spherical form, and weighing 20,000 lbs., could not reasonably have a greater velocity than 372 feet in a second if projected from a lunar volcano. The calculations are based on the following premises.

A stone having 5·6 inches in diameter, with a density of 3·0, and weighing 10 lbs., is assumed to be projected from a lunar volcano at the rate of 9000 feet in a second, *i. e.* with a velocity more than sufficient, according to Dr. Smith and others, to allow it to pass the limits of mutual attraction between the moon and the earth.

Taking the sp. gr. of iron 8·0, and bearing in mind that the areas are as the squares and the masses as the cubes of the diameters, we arrive at the following results. A mass of iron (globular) to weigh 20,000 lbs., sp. gr. 8, will be a little over 50·9 inches in diameter; found thus:—

$$\sqrt[3]{\left(\frac{20{,}000\text{ lbs}\times 16\text{ oz.}\times 1728}{8000\text{ sp. gr.}\times{\cdot}5236}\right)}=\sqrt[3]{132009}=50{\cdot}9\text{ in. nearly.}$$

A similar mass of stone to be the same weight, and sp. gr. 3·0, must be 70·6 inches in diameter; found thus:—

$$\sqrt[3]{\left(\frac{132009\times 8}{3}\right)}=\sqrt[3]{352024}=70{\cdot}6.$$

Now if a piece of stone 5·6 inches in diameter, weighing 10 lbs., be projected with a velocity of 9000 feet per second, a mass 70·6 inches diameter, and weighing 20,000 lbs., could only be projected with a velocity of 715 feet per second, because the weights would increase so much faster than the sectional area.

	Lbs.		Diam.		Velocity.		
Thus as	$\frac{20{,}000}{10}$	:	$\frac{(70{\cdot}6)^2}{(5{\cdot}62)^2}$	::	9000;		
or as	2000	:	$\frac{4984{\cdot}36}{31{\cdot}36}$	::	900	:	715 feet.

The mass of iron has a less sectional area than the stone because of its greater specific gravity, viz. in the proportion of $(50\cdot9)^2$ to $(70\cdot6)^2$; it would therefore only be projected with a velocity of 372 feet per second; or as

$$498436 : 259081 :: 715 : 372.$$

That is, a velocity more than 20 times too small to allow of the larger known meteoric masses to reach the earth, if projected from a lunar volcano.

The exclusion from a lunar origin of the larger meteoric masses, especially iron ones, though not perhaps altogether subversive of the lunar theory generally, is yet injurious to its stability.

The physical constitution and internal appearance of some aërolites also, as those of Barbotan, Weston, Juvenas, and Bishopville, are entirely opposed to the idea either of an atmospheric origin, or of any consolidation of homologous, or nebulous particles existing in interplanetary space. They are evidently *parts*, as Dr. Lawrence Smith likewise justly insists on, of some larger whole, and are not unfrequently true igneous, if not volcanic rocks. Physically speaking, there is little choice left to us but to consider some of them certainly as having true geological and mineralogical characteristics; either proceeding from volcanoes in the moon, or portions of a broken satellite or planetary body: there may indeed be difficulties and objections to either supposition.

In a previous Number of this Journal, I endeavoured to bring forward some arguments and facts in favour of the latter idea, and I barely alluded to the lunar theory, but since the publication of Dr. Lawrence Smith's elaborate essay in its favour, I have now ventured to state more fully some reasons, not without their weight, against a theory in many respects so plausible.

LXII. *On a New Ore of Silver.* *By* H. J. Brooke, F.R.S..*

I RECEIVED some years since from Mexico a specimen of an ore of silver, said to be carbonate. It occurs in small, compact, irregular-shaped, earthy-looking masses, imbedded in carbonate of lime and quartz accompanied by crystallized blue carbonate of copper. Its colour is dull, dark gray; it is entirely devoid of lustre, and its hardness appears to differ in different parts of the specimen.

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* Communicated by the Author.

It was examined in a very cursory manner by the late Richard Phillips; and the portion he examined being found to effervesce with acid and to contain silver, he was led to regard it as a carbonate, and particularly as it so much resembled the carbonate of silver described by Selb.

A recent examination of it, however, by Mr. Richard Smith, in the metallurgical laboratory at the Museum of Practical Geology in Jermyn Street, shows it to be a very different compound, and one new to mineralogy; and there can be no doubt that the carbonic acid which deceived Mr. Phillips was derived from the intermixed carbonates of lime and of copper.

The analysis of two small portions of the earthy part of the substance separated from the matrix gave the following results per cent.:—

	I.		II.
	a.	*b.*	
Silver	16·09		17·18
Antimony	7·82	7·50	7·28
Sulphur	1·41		1·84
Selenium	2·81		3·58
Chloride of silver	1·26		2·67
Oxide of copper	10·46		8·61
Silica	45·56		41·81
Alumina	2·06		4·04
Peroxide of iron	2·21		
Lime	1·72		2·83
Carbonic acid	2·92	3·04	
Combined water	2·31		
Hygroscopic water	·99		
	97·61		

The whole of the copper contained in the mineral is dissolved out by acetic acid; from this we may infer that it is not present in the form of sulphide or selenide. The acetic acid solution was found to contain lime, but did not give any precipitate with the addition of hydrochloric acid, nitrate of silver, or chloride of barium.

LXIII. *Notices respecting New Books.*

HUGHES'S *Reading Lessons. First Book.* London: Longman, Brown, Green, and Longmans. 1855.

THOSE who are practically engaged in the education of youth will be best able to appreciate the value of such reading lessons as Mr. Hughes intends his to be, as well as the difficulties which have to be surmounted in realizing his ideal. In order to form a fair estimate of the success of his undertaking, and to make these difficulties understood, it will be well to consider briefly what is required in such reading lessons.

In order to suit all tastes, the subjects should embrace the several departments of literature, science, and art. The writers should be men of known ability, accustomed to original research in their several departments, so that what they write may be quickened by that peculiar freshness and vigour which a direct acquaintance with their subjects alone can give. Each lesson should be a creation, and not a compilation. Each author should strive by all possible means to arouse and maintain the interest of his reader; for this purpose he should avoid too much detail, and should be less anxious to increase the number of his facts, than to surround each fact with its own atmosphere of interest. All looseness of expression and insufficiency of explanation should be carefully avoided, their consequences upon boys being extremely disheartening and injurious. The strictest accuracy in facts and expressions should be preserved, and all definitions should be as clear, decided, and concise as possible.

The difficulty of Mr. Hughes's task will now be evident. To find a number of men to co-operate with him in the fulfilment of the above demands is not easy, and it will not, therefore, be surprising to find that the success which has crowned his efforts, though considerable, is not complete. As far as this first book is concerned, however, we shall see that its success is certainly sufficient to encourage him to persevere in his praiseworthy task; and we sincerely hope, that whilst preserving the many good qualities of the first, his forthcoming books will be free from its defects.

This is not the place to speak much of the departments of literature and art. We shall merely remark, that the lessons on Mental Culture, by Mr. Edward Purcell, contain many sound thoughts and much good practical advice; they will be read by serious youths of more advanced age with profit and interest. The Biographies, which form an admirable feature in a boy's reading book, are on the whole well written. We know nothing more calculated to rouse a boy's faculties into activity, and induce him to be diligent and persevering, than well-written lives of men who have demonstrated by their works what diligence and perseverance can accomplish. In the forthcoming books we hope to see this feature more fully developed. The selections of poetry are not as well chosen as we could wish, neither can we say that the lessons on Music and Fine Arts are likely to prove quite successful. The chapters on English Literature by Mr. George L. Craik are well written, and will find many readers. Believing, as we do, that these lessons may do something towards cultivating a love for English literature in boys, we shall not stop to inquire whether the rival claims of English authors are there quite justly stated; for it is far more important to develope in boys the *power* of judging for themselves, than to furnish them with the *opinions* of other judges. Everybody knows the attractions which natural history possesses for young people, and we doubt not that

Mr. Patterson will be fully appreciated by them. Want of space will not allow us to do more than notice, that the departments of Ethnology and of Animal and Vegetable Physiology are in the hands of the well-known writers Latham, Mann, and Lankester. We hasten to consider more carefully those parts of the book with whose subjects we are more immediately connected.

Mr. Hughes himself is the writer of the lessons on Physical Geography, and, as might be expected from the originator of the whole scheme, has performed his task well. We wish we could say the same of Mr. J. Beete Jukes, the writer of the pages on Geology, or rather Mineralogy. From a director of the Geological Survey of Ireland one naturally expects to have interesting articles, or at least, articles free from gross inaccuracies; but justice, and the very fact of his high position, compel us to say that his productions are neither interesting nor free from the most egregious blunders; indeed the book would be considerably improved if his part of it could be completely suppressed. We are quite aware that a sentence so severe as this should not be lightly pronounced, nor should it be unaccompanied by proofs; no other consideration except this would induce us to give greater prominence to errors which we feel sure a careful perusal would have prevented. The articles are uninteresting on account of their sterility, and of the almost entire absence of that freshness and life which we believe to be indispensable. The uninteresting descriptions of minerals, unenlivened, as they to a great extent are, by interesting associations, are far more calculated to check than to awaken in the reader any love for the science.

But the facts brought forward are themselves often inaccurate, and the author's ignorance or unpardonable carelessness will unfortunately either confuse or misinform the attentive reader. For instance, on page 72 the reader is informed by Mr. Jukes, that "common salt is formed by the combination of soda with hydrochloric acid," and on page 217, by Prof. Frankland, that it is formed by the combination of sodium and chlorine. Which is he to believe? Mr. Jukes calls fluor-spar a fluorate of lime. No such compound as the latter is known to chemists,—fluor-spar is fluoride of calcium. Iodine and bromine are placed among the seven substances which, according to Mr. Jukes, "when pure, are always found in the state of gas or aëriform fluid;" now every chemical book we have seen tells us that iodine is a dark-coloured *solid* having a metallic lustre, and that bromine is a *liquid* of a very deep brownish-red colour, and almost three times heavier than water. Again, we are told that platinum "is the heaviest of all metals except gold." Now the specific gravity of platinum, although it varies a little with its structure, is about 21·5, whereas that of gold is only 19·5. More blunders might be exposed, but the above surely warrant our severe sentence. With the hope, therefore, that Mr. Jukes will either be more cautious, or decline writing altogether in such books, we pass on with pleasure to Professor Frankland's lessons on Chemistry.

These lessons have many good qualities, and from the spirited and pleasant manner in which they are written, they will no doubt, as they deserve, have many admirers. The facts are here accurate, and the manner in which they are introduced, as well as the peculiar treatment of the whole subject, leave no doubt upon the mind that the author is perfectly at home, and speaks from his own experience. It is only as critics that we feel it to be our duty to draw attention to a slight absence of strictness and philosophic accuracy in some of the theoretical questions introduced; before doing so, however, let us admit that these defects diminish but little the value of the lessons; indeed, their general excellence *suggests*, in some measure, that closer examination which has led to the detection of what we deem to be faults. For instance, great and, we think, undue stress is laid upon the elective character of the chemical force, by which it is thought to be distinguished from all other natural forces. "If," says the author, "we place a piece of silver and a piece of iron in a tumbler of water, we notice after the lapse of a few days that the iron has lost its lustre and become covered with rust, whilst the brilliant surface of the silver remains unchanged. Here one of the constituents of the atmosphere, oxygen, which was dissolved in the water, finding itself in the presence of silver and iron, made a selection between the two metals, and preferring the iron, united exclusively with that metal." This is quite true, but it can scarcely be considered characteristic of the chemical force; for Professor Frankland knows quite well, that if a magnet were presented to the same two pieces of metal, it would manifest precisely the same preference, and make exactly the same selection. Again, the term *elasticity* is used in a rather obscure manner. Let us suppose that the young reader has turned to the very useful appendix at the end of the book, and found elasticity defined, very properly, as "the property which some bodies have of recovering their form and dimension after the force which changed their shape and appearance is withdrawn," and then imagine his confusion when he reads the following sentences:—"*Heat* is the principle of *elasticity*."—"Just as cohesion modifies chemical affinity, &c., so *heat* or *elasticity* produces analogous results by the formation of volatile compounds, and their expulsion in the gaseous form." Or lastly, "the mode of decomposition being here determined by the *elasticity* of the carbonate of ammonia, which is expelled from the mixture in the form of white vapours." It may be objected, that in chemistry the term elasticity has a different meaning from the one above given; but if so, why has not Mr. Connon noticed it in his appendix to the article on chemistry? The fact is, however, that the term has, or ought to have, but one meaning, and in the lessons on chemistry other words would certainly have conveyed the author's meaning more distinctly. These defects, however, are not serious ones, and we are of opinion that the lessons on chemistry are amongst the most successful of the series: we heartily recommend them to the attention of all young readers.

Mr. Purcell's lessons on Mechanics are also well and clearly written.

Professor Tyndall's, on Natural Philosophy, are in every respect admirable. The style is attractive, lucid, and vigorous. The collision of bodies, and the laws of falling bodies, are generally somewhat difficult questions for boys, but they are here made so remarkably clear and simple that no boy of ordinary intelligence can fail to understand them. All we will add is, that if the boy not only reads, but studies these lessons, and then tries to solve the problems proposed therein, his success will be certain; and it will bring with it so much pleasure and encouragement, that he cannot fail speedily to overcome far greater difficulties.

The last lessons we shall notice are those by the Rev. Robert Main, on Astronomy, which may also be pronounced successful. The explanations are usually clear, though there are some obscure sentences. For instance, in the following there is surely some misprint:—"The real motion of the earth from west to east produces an exactly equal apparent motion of the sun in the same direction." In the following there must be some omission: "Since the ecliptic and equator are both *great circles*, that is, since both pass through the visible sphere of the heavens, their planes will cut each other in a line, which is a diameter of the sphere." We disagree with the introductory sentence, too, inasmuch as we cannot understand "why the grandeur of its objects" can make astronomy "the noblest of all physical sciences;" neither are we aware that astronomers are more "completely separated from the petty cares and concerns of our present existence" than other physicists. All this is merely accredited nonsense, usually uttered thoughtlessly, and not worthy of being uttered at all. It forms no part of Mr. Main's *lessons*, however, which are really good.

In conclusion, we wish Mr. Hughes every success; his first book is a good one, and we hope his future books will be better. We have given unusual space to our review because we deem his project an important one, and because it is almost the first of its kind in which the principle has been recognized, that only the ablest men can expound the elements of a science successfully, and that the task is in every respect worthy of their valuable time.

A Treatise on Conic Sections. By the Rev. GEORGE SALMON, *A.M.* Third Edition. London: Longman, Brown, Green, and Longmans. 1855.

It is a source of considerable satisfaction to find, amongst the crowd of very imperfect educational books which are daily issued from our press, a treatise so truly valuable as the present; and it is also cheering to learn that the public has so far recognized its merits as to demand a third edition. The book is now sufficiently well known, otherwise its title might mislead many; for although it is true that the greater part of it is devoted to an examination of the properties of the conic sections, yet this being done from an analytical point of view chiefly, it was necessary to prepare the reader by a similar investigation of the properties of the line and circle; so that, in fact, the whole constitutes a very efficient treatise on the elements of analytic geometry, such as may with advantage be placed in the hands of every student who has mastered the elements of Euclid, plane trigonometry, and algebra. We venture to assert, that amongst students already acquainted with its merits, this is one of their favourite text-books; for the treatment throughout is admirably clear, strict, and elegant,—in fact, such as can be achieved only by one who, besides that perfect mastery of the subject which can only be acquired by original research, possesses also that unacquirable talent of lucid exposition, and is guided by that knowledge of the difficulties usually encountered by students, which experience only can give.

This third edition is revised and enlarged, and contains many improvements upon the former two, with respect to its type, arrangement, and its well-chosen and numerous selection of examples. The student will do well to work all these conscientiously, and to pay particular attention to those interesting chapters on "abridged notation." The last chapter contains a short but clear account of the most important modern geometric methods, amongst which is that beautiful method of reciprocal polars first introduced by Poncelet, an acquaintance with which may be said to form an epoch in the history of every young mathematician.

The importance of these methods may be estimated from the fact, that in the hands of a Steiner, with scarcely any help from algebra, they have become the most powerful instruments of discovery, and have given him a wonderful insight into the nature and properties of curves; an insight which is, perhaps, more thorough and direct than any attainable by one who is accustomed to call in the more mechanical aid of algebraic calculation.

We do not wish here to revive the old and useless discussion on the comparative merits of the algebraic and geometric methods; both have undoubtedly their advantages, and both are indispensable. In the greater part of the present treatise, however, the former of these methods is adopted; and in drawing attention to the importance of the last chapter, we would merely remark, that if his object be to obtain a thorough knowledge of the properties of conics, the student will do well to combine the two methods to a greater extent than is here done; for the fact cannot be disputed, that the very facility with which results can be obtained algebraically, may indirectly prevent that intimate acquaintance with the properties of curves which a rigid geometrical investigation always secures.

LXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Concluded from p. 389.]

June 21.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Existence of a Magnetic Medium. By T. A. Hirst, Esq.

In a note on the above subject, communicated to the Royal Society on March 16, 1855, Professor Williamson objects to a certain conclusion deduced by Professor Tyndall in a letter addressed by the latter to Mr. Faraday and recently published in the Philosophical Magazine. Professor Tyndall's conclusion was that, according to the hypothesis of the existence of a magnetic medium in space and of the identity of magnetism and diamagnetism, a compressed diamagnetic cube ought to be less repelled when the magnetic force acts on the line of compression than when it acts at right angles to that line; a result which his own experiments have contradicted. Against the legitimacy of this conclusion Professor Williamson urges that "Dr. Tyndall seems to have assumed, that on the compression of an aggregate of particles of a diamagnetic substance, the medium is not displaced by the particles in their change of position." We shall be better able to estimate the value of this objection by recalling the steps of Professor Tyndall's argument.

A magnetic cube was taken which had *already* been compressed: its deportment before a magnet was experimentally examined, and deductions drawn concerning the changes that would occur in that deportment by merely conceiving the magnetic capacity of the material particles to be diminished, without in any way altering the distances between those particles, and consequently without in any way displacing the magnetic medium in the interstices of the body.

Instead of the assumption attributed to Dr. Tyndall, he might, with greater justice, be accused of having disregarded the possible presence of the medium within the body; but in his own defence he may with perfect justice reply, that in Mr. Faraday's experiments, which originally gave rise to the discussion, no such interpenetration of two media existed.

Admitting, however, that the interstices of a body are occupied by the medium, it may be interesting to inquire whether, from an argument similar to Professor Tyndall's, the same decided conclusion could, with equal accuracy, be deduced. To answer the inquiry, it must be remembered that the force of the argument in question depends essentially upon the justness of the supposition that a diamagnetic cube may, theoretically, be produced from a magnetic one by conceiving the magnetic capacity of the particles of the latter to be sufficiently diminished. It is evident that the total attraction of the cube by a magnet will be equal to the sum of the attractions of the material particles, and of the medium contained in its interstices. If this sum be greater than the attracting force upon the quantity of medium which the cube *and its contents* displace, the substance is called magnetic, for it will be drawn towards the magnet; if less, it is called diamagnetic, for it will be repelled from the magnet. But in our present knowledge of the properties of the medium there is nothing incompatible with the supposition that the density of the internal medium may so far exceed that of the external, that the attraction of the former by the magnet is *itself* greater than the attraction of the medium displaced by the cube and its contents. If so, however, no conceivable diminution of the magnetic capacity of the material particles could possibly render such a cube diamagnetic.

This is sufficient to show that, admitting the presence of the medium within the cube, the method of argument adopted by Professor Tyndall would not be strictly applicable, unless the density of the internal medium were subjected to limits which the advocates of its existence might possibly be unwilling to grant.

But it may be asked, if, whilst admitting that the medium may exist in the interstices of the body, it be granted that a diamagnetic may be produced from a magnetic cube in the manner assumed by Professor Tyndall, does it still follow, necessarily, that attraction is always greatest—repulsion least—when the force acts in the line of compression? In other words, can a conclusion contradictory to experimental facts be *then* legitimately deduced?

In attempting a reply to this question, it will, perhaps, be best to employ the following symbols. Let W represent the attracting force of the magnet upon the medium displaced by the cube and its contents. The value of W will, of course, be unaltered, no matter whether the force acts in, or at right angles to the line of compression. When the force acts in the line of compression, let P_1 represent the attracting force upon the particles, W_1 the attracting force upon the internal medium, and let F_1 be proportional to the resultant attraction of the cubical mass towards the magnet. Let P_2, W_2 and F_2 have similar significations when the force acts at right angles to the line of compression. Then we may put

$$F_1 = P_1 + M_1 - W,$$
$$F_2 = P_2 + M_2 - W.$$

Now, in a compressed magnetic cube, experiment proves that

$$F_1 > F_2,$$

or

$$P_1 + M_1 > P_2 + M_2,$$

i. e.

$$P_1 - P_2 > -(M_1 - M_2).$$

As long as we are ignorant of the properties of the medium within the body, we will, for the sake of completeness, consider the following three distinct cases.

I. The attracting force upon the medium within the cube is the same when the force acts in either the one or the other of the two directions, with respect to the line of compression. Here

$$M_1 = M_2,$$

hence

$$P_1 > P_2.$$

II. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is also greatest in the same direction. Here, according as P_1 is greater or less than P_2, M_1 is greater or less than M_2; hence, inasmuch as

$$P_1 + M_1 > P_2 + M_2,$$
$$P_1 > P_2 \text{ and } M_1 > M_2.$$

III. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is greatest in the direction perpendicular to the same. Here, according as P_1 is greater or less than P_2, M_1 is less or greater than M_2, so that the two hypotheses

$$(1)\quad P_1 > P_2 \text{ and } M_1 < M_2, \text{ and}$$
$$(2)\quad P_1 < P_2 \text{ and } M_1 > M_2$$

are both compatible with the sole condition,

$$P_1 + M_1 > P_2 + M_2.$$

In order to test the applicability of Professor Tyndall's method of argument in each of these cases, let us conceive, with him, that the magnetic capacity of the particles is so far diminished that their attractions P_1 and P_2 are reduced to p_1 and p_2. This may evidently be done by making $p_1 = aP_1$ and $p_2 = aP_2$, where a represents a positive fraction whose magnitude can be diminished indefinitely. If, under this supposition, the resultant attractions F_1 and F_2 become f_1 and f_2, then it can easily be proved that, however small the value of a may be, we shall always have

$$f_1 > f_2$$

in the cases I., II. and III. (1). That is to say, with the distributions of the internal medium assumed in these cases, the resultant attraction of the cubical mass will always be greatest, or repulsion least, when the force acts in the line of compression, no matter how diamagnetic the cube may have become by diminishing the magnetic capacity of its particles. It may just as easily be proved, however, that in case III. (2) a value of a may be chosen sufficiently small to make

$$f_1 < f_2,$$

that is to say, with the distribution and properties of the internal medium here supposed, it is quite possible so far to diminish the magnetic capacity of the particles as to obtain a cube which will be attracted least, or repelled most strongly when the force acts in the line of compression. This conclusion involves nothing contradictory to experimental facts, whereas the former one does.

I will not here enter into the question of the relative probability of these three cases, supposing the medium to exist. My sole object has been to show that, although the method of argument adopted by Professor Tyndall is strictly applicable in a great number of cases, even when the medium is supposed to fill the interstices of the body, yet it is possible to attribute properties to this medium of such a nature as to avoid the conclusion, contradictory to fact, which he has deduced. This may be done in two ways. *First*, the density of the internal medium may be such as to render it impossible to produce a diamagnetic from a magnetic cube in the manner assumed, *i. e.* by diminishing the magnetic capacity of the material particles. *Secondly*, granting that a diamagnetic cube may be so produced, the distribution and properties of the internal medium may still be such as to cause the cube to be attracted least, or repelled most strongly when the force acts on the line of compression, and thus, if the substance be diamagnetic, to cause it to agree, in its deportment, with experimental results. On the other hand, if these hypothetical properties of the internal medium be discarded as artificial or inadmissible, then at present I see no way of escaping the conclusion of Professor Tyndall's argument.

With regard to the explanation given by Professor Williamson, it will be observed that he pursues a path quite different from that of Professor Tyndall, when he considers the effects produced by compressing a number of particles surrounded by a magnetic medium. This compression, he states, may alter the attraction of the mass by a magnet in two ways;—"first, by altering the density of the matter; secondly, by altering the density of the medium." By the term '*density of matter*' is usually understood the relation which exists between the quantity of matter which a body contains, and the volume of the space enclosed by its external surface. But in the present case, where a comparison is instituted between the matter of the body and the medium which is supposed to fill all its pores, we must, I imagine, understand by the term 'density of matter,' the relation which exists between the sum of the masses of the particles, and the sum of their volumes; but if so, then, the particles themselves being incompressible, it is clear that compression could not alter the 'density of matter.'

As to the second effect of compression, viz. an alteration of the density of the medium, it may be quite conceivable, although I do not find that Professor Williamson has any where taken it into consideration. The effects of compression may, therefore, be more correctly described as either—*first*, a diminution of the interstices of a body, without altering the density of the medium which fills them; or *secondly*, a diminution of the interstices, accompanied by an alteration of the density of the medium within them.

Let us assume, as Professor Williamson has virtually done, that the first of these effects takes place; then, if we admit that "in a cubical mass of carbonate of iron the material particles are more magnetic than the medium which they displace, and the force with which it is attracted is proportional to this excess," we can by no means admit that, because "it becomes more magnetic by compression, we must conclude that the loss of magnetic medium *from its interstices* is more than supplied by the magnetic matter which takes its place;" for, according to what has already been advanced, the excess of the attraction of the material particles above that of the medium they displace is the same after, as it was before compression; inasmuch as compression merely changes the relative *situations* of the particles, by bringing them closer together, but does not in the least alter their volume, and consequently does not in the least alter the quantity of medium they displace.

With respect to carbonate of lime, Professor Williamson's conclusion is, of course, untenable, because it is based upon the foregoing one. He says, "when these particles are brought closer together by pressure, with diminution of the intervening spaces occupied by the medium, the mass becomes more diamagnetic, because a certain quantity of the magnetic medium is thus replaced by the less magnetic matter." It is, however, manifest that exactly the same quantity of magnetic medium is displaced by the less magnetic matter after, as there was before compression. Why, then, should diamagnetic action be increased by compression?

Lastly, with respect to the crystals of carbonate of iron and carbonate of lime, Professor Williamson's explanation, although ingenious, is liable to the same objections as those already mentioned. It cannot, in fact, be said that the functions of matter predominate most strongly over those of the medium they displace in any one direction, merely because the particles may be closer together in that direction; for, as long as each particle is surrounded by the medium, the predominance of that function of the particles with which we are concerned, *i. e.* their attraction, over that of the medium they displace will be the same, whatever may be the distances of the particles asunder.

From the foregoing remarks, therefore, it is manifest that, if Professor Tyndall has not yet succeeded in demonstrating that the hypothesis of the existence of a magnetic medium and of the identity of magnetism and diamagnetism is *necessarily* at variance with experimental facts, neither has Professor Williamson succeeded in proving that this hypothesis is in accordance with those facts. The question of the existence of a magnetic medium is still an open one, and will continue to be so until the many important principles which it involves, but which have not been introduced into the present discussion, have been further elucidated by new investigations and new thoughts.

"Anatomical Notices." By Professor Andrew Retzius, of Stockholm. Extracted from a Letter to Dr. Sharpey, dated 10 May, 1855. (Translation.)

"1. *On the Form of the Cranium in the Embryo.*

"So far as I am aware, due attention has not hitherto been given to the different forms presented by the cranium in its earlier stages of growth. In the skeletons of early human embryos to be seen in most museums, the imperfectly ossified cranium is for the most part shrunk up and disfigured. To obtain a correct view of the form of the cranial cavity, I first remove the skin, fascia and muscles; I then, by injecting water through the vertebral canal, thoroughly wash out the soft brain and spinal cord; and lastly, fill the cerebro-spinal cavity with quicksilver or with melted tallow, taking care not to distend it over-much. The opening in the vertebral canal is to be stopped with a little plug of wood, and the preparation allowed to dry.

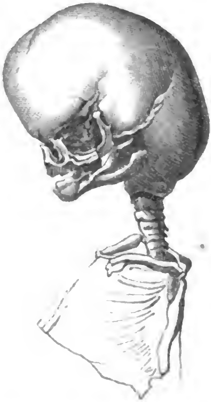
"In the skeleton of a human embryo of the fourth month, prepared in this way, the occipital bone was found to have the form of a funnel, the narrow part of which passed into the vertebral canal, as represented in the accompanying figure 1.

"It thus appears that the human occipital bone, in its early condition, approaches in form to the vertebral canal, and in this respect also it resembles the occipital bone in several quadrupeds, which so obviously represents the first cephalic vertebra.

"For the sake of comparison, I divided the skull of another embryo of the same age into two halves by a vertical median section, washed out the brain and examined the preparation while it lay immersed in weak spirit of wine in a shallow glass capsule. The occipital bone had the same funnel-shape as in the former case. As development advances, the funnel-like form is gradually lost, whilst, on the other hand, the bone appears more deepened or tubular the earlier the embryo to which it belongs. The same is true of quadrupeds.

"In the beautiful figure of the embryo-skull, given in Kölliker's 'Microscopische Anatomie,' B. II. taf. 3. fig. 2, the downward prolongation of the occipital bone appears much less, but whether less than it ought to be I cannot venture to say, as I happen to have no specimen of that age (one month older than the one I have given) with which to compare the figure.

Fig. 1.

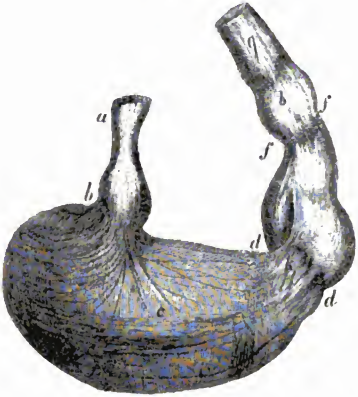


"The early form of the occipital bone I have here described becomes easily intelligible when we remember that the shape of the skull is regulated by that of the brain; and that as the latter is at first greatly elongated, and its ganglia imperfectly covered, the cranium must then also have a correspondingly elongated form.

"2. *On the Antrum Pylori.*

"I have now for some time directed my attention to the determination of the true form of the stomach, and have become more and more convinced that the antrum pylori of the older anatomists ('Pförtnerhöhle' of the Germans) is really a special compartment of the general cavity. I have had occasion to make numerous examinations of the stomach in the bodies of middle-aged women who died in the hospital, and found the form to be nearly as represented in figure 2, where *d d*, *f f* indicate the antrum pylori. This part is also distinguished by greater thickness of its muscular coat, more strongly developed glands, and the presence of the well-known plicæ fimbriatæ. The commencement of the duodenum also forms a special rounded cavity, which I should propose to name the ***antrum duodeni***, and which is characterized internally by the absence of valvulæ conniventes, and by the dense array of Brunner's glands beneath its mucous membrane. This part constitutes what has been called the fourth stomach of the Porpoise, and some other Cetaceans. I may observe that the so-called ligaments of the pylorus are connected with the formation of the antrum pylori.

Fig. 2.



" 3. *On the Fascia Superficialis.*

" The accounts usually given of the fascia superficialis are for the most part very imperfect. As far as I can judge, this fascia is in many parts of the body a constant membrane, and really appertains to the skin, as may be particularly well seen in the integuments of the back. The cutaneous muscle of quadrupeds, in most cases, probably arises out of this fascia; the muscular fibres being deposited, as I conceive, in the midst of its substance, and finally becoming covered by it as their perimysium. In this way too, I imagine the platysma myoides and epicranius to be formed."

" On the Effect of Local Attraction upon the Plumb-line at stations on the English Arc of the Meridian, between Dunnose and Burleigh Moor; and a Method of computing its Amount." By the Venerable Archdeacon Pratt.

The author states that in a former communication he had pointed out a method for calculating the deflection of the plumb-line at stations on the Indian arc, caused by the attraction of the Himalayas and of the vast regions beyond, with a view to the correction of the astronomical amplitudes of the measured subdivisions of the arc before they are applied to the determination of the ellipticity of the earth.

The same subject is taken up in the present paper, but in reference to the English arc between Dunnose and Burleigh Moor; and a different method of calculating the attraction is given.

The paper consists of three parts. In the first, the ellipticity of the English arc is calculated without taking account of attraction. The arc is divided into five parts, and the lengths and amplitudes assigned to them in Mudge's Trigonometrical Survey of England, vols. ii. and iii., are made the basis of the calculation. These portions of the arc are compared two and two, and ten values of the ellipticity thence deduced; the mean of which is $-\frac{1}{43{\cdot}8687}$. The ten values, of which this is the mean, differ considerably among each other, indicating that there is some disturbing cause, like local attraction, affecting the plumb-line, and therefore the apparent latitudes. The variations of the observed amplitudes are then discussed; and the necessity of calculating the local attraction pointed out.

In the second part a formula is obtained for calculating the attraction. The method is different from that given by the author in his first communication. The curvature of the earth is neglected, as this would have no sensible effect on the results in the British Isles. The attracting mass is divided into a number of smaller masses standing on rectangular bases at the sea-level, and the height of each is taken equal to the average height of the surface above the sea-level. The dimensions of the bases may differ from each other, and are determined by the contour of the surface in such a way that the average height in each mass may not depart materially from the height of any part of it. The investigation leads to the following Rule for determining the horizontal attraction deflection of the plumb-line caused by any one of these Tabular Masses (as the author calls them):—

RULE.—*Take the origin of coordinates at the station where the plumb-line is. Let the plane of* xy *be horizontal, and the axis of* x *in the vertical plane in which the amount of deflection is to be found.*

Write down the coordinates XY xy *of the furthest and nearest angles of the Tabular Mass from the origin;* Y *is always to be considered* $+^{\text{ve}}$, *and* y $+^{\text{ve}}$ *or* $-^{\text{ve}}$ *accordingly.*

Form four ratios, by first dividing each ordinate by the abscissa not belonging to it, and then by dividing each ordinate by its own abscissa, viz.

$$\frac{Y}{x},\ \frac{y}{X},\ \frac{Y}{X},\ \frac{y}{x}.$$

Look in a Table of Tangents for the four angles of which the tangents equal the above ratios.

Form four more angles by adding (subtracting if they be negative) half of each of these angles just found to (or from) 45°.

From the sum of the log-tangents of the first two of these angles subtract the sum of the log-tangents of the second two.

This result, multiplied by H *feet and by* $\frac{1''}{369}$, *will give the required deflection of the plumb-line in seconds of a degree*—H being the height of the Tabular Mass above the sea-level, and its density being taken equal to half the mean density of the earth, which is that of granite.

The only restriction to be attended to in the application of this Rule is, that the ratio of the height of the attracted station above the sea to each of the horizontal coordinates of the nearest angle of the Tabular Mass, must be so small that its square may be neglected.

If any part of the attracting mass is nearer to the station than this, it must be divided into vertical prisms and the attraction of each found; for which the author gives a formula in a note.

In the third part the Rule is applied, for the purpose of illustration, to obtain an approximate value of the deflection of the plumb-line at Burleigh Moor, the north station of the arc under consideration, situated on the north coast of Yorkshire. The deflection is found to be 3″·644 to the south. The data upon which this calculation is based are gathered by the author from the Map which accompanies General Mudge's account of the English Survey, and the heights marked down on that map.

The deflection at several other stations is deduced from this result of calculation, by using the amplitudes given in Mudge's work, and also in Captain Yolland's 'Astronomical Observations made with Airy's Zenith Sector,' published in 1852, and by supposing the curvature of the meridian to be the mean curvature of the whole globe as laid down by Mr. Airy in his article on the Figure of the Earth. Thus the deflection at Black Down on the Dorsetshire coast (one of the places mentioned in Captain Yolland's volume), the author finds to be 5″·886 to the north, a quantity which tallies well with the deflection assigned to Burleigh Moor on the Yorkshire coast, if the relative heights of the two coasts are compared. This affords a satisfactory evidence of the correctness of the principles laid down in the paper; and, as the author thinks, makes the subject well worthy the attention of those who are interested in the English Survey, and who have it in their power to furnish the most accurate data for the application of the Rule he lays down. The subject is also no less important to the mathematician in his investigation of the figure of the earth.

In a Postscript the author makes the following remarks upon the Astronomer Royal's method of accounting for the large amount of deflection on the Indian arc deduced by the author in his former communication:—

"Since the above was written, I have had the opportunity of seeing a notice of the communication of the Astronomer Royal on the

Density of Table-lands supposed to be supported by a dense fluid or semi-fluid mass; and the use he makes of his suggestions to remove the discrepancy, pointed out in my first communication, between the values of the deflection of the plumb-line in India, as determined by calculating the attraction of the Himalayas, and as indicated by the results of the Great Trigonometrical Survey. The following difficulties occur to me in the way of this highly ingenious and philosophical method of removing the discrepancy:—

"1. It assumes that the hard crust of the earth is sensibly lighter than the fluid or semi-fluid mass, imagined to be a few miles below the surface. But I know of no law, except the unique law of water and ice, which would lead us to suppose that the fluid mass in consolidating would expand and become lighter. One would rather expect it to become denser, by loss of heat and mutual approximation of its particles.

"2. There is, moreover, every reason to suppose, that the crust of the earth has long been so thick, that the position of its parts relatively to a mean level cannot be any longer subject to the laws of floatation. If the elevations and depressions of the earth's surface have always remained exactly what they were at the time when the laws of floatation ceased to have an uncontrolled effect, then the same reasoning would no doubt apply in our case, as if they still had their full sway. But geology shows that other laws are in constant operation (arising most probably, as Mr. Babbage has suggested, from the expansion and contraction of the solid materials of the crust), which change the relative levels of the various parts of the earth's surface, quite irrespectively of the laws of floatation. If Mr. Hopkins's estimate of the thickness of the crust be correct, viz. at least 1000 miles, these laws of change in the surface must have been in operation for such an enormous interval of time, as quite to obliterate any traces of the form of surface which the simple principles of hydrostatics would occasion. Indeed, it seems to me highly probable that the elevation of the Himalayas and the vast regions beyond may have arisen altogether from the slow upheaving force arising from this cause.

"I am inclined to think that the only explanation of the discrepancy between my calculations and the results of the Indian Survey, is to be found in the greater curvature of the Indian Arc."

"Contributions to the History of Aniline, Azobenzole and Benzidine." By A. W. Hofmann, Ph.D., F.R.S.

The transformation of nitrobenzole into aniline by the action of sulphuretted hydrogen is attended with such difficulties and requires especially so much time, that chemists hitherto have generally preferred to prepare this base from indigo. Lately a new modification of Zinin's process has been adopted by M. A. Béchamp*, which consists in submitting nitrobenzole to the reducing action of acetate of protoxide of iron. This process—M. Béchamp simply uses a mixture of iron and acetic acid—is applicable to all nitro-compounds and has been extensively tried with the most perfect success in the laboratory of the Royal College of Chemistry. The facility of the process, its rapidity, and the large amount of product it yields in most cases, cannot fail materially to assist the study of the volatile organic bases.

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* Chem. Gaz., March 1, 1855, p. 81.

During these experiments several observations were made, which I beg leave to bring under the notice of the Society.

When employing about double the amount of iron which is recommended by M. Béchamp (2·5 instead of 1·2 of iron to 1 part of nitrobenzole), Mr. Alfred Noble found that the latter portion of the distillate solidified partly in the receiver and partly in the condenser. Washed with hydrochloric acid from adhering aniline, and once or twice recrystallized from boiling alcohol, the solid matter was obtained in fine crystals of a yellowish-red colour, and fusing below the boiling-point of water. These crystals possessed all the properties of azobenzole, which was moreover identified by combustion.

0·260 gramme of substance gave 0·755 grm. of carbonic acid and 0·134 grm. of water.

The well-established formula of azobenzole, $C_{12} H_5 N$, requires the following values:—

	Theory.		
	Equivalents.	Per-centage.	Experiment.
Carbon	12	79·12	79·12
Hydrogen	5	5·49	5·76
Nitrogen......	1	15·39	
		100·00	

The azobenzole obtained in this manner is so readily purified that this process is greatly preferable to the action of alcoholic potassa upon nitrobenzole, since the latter process simultaneously produces several substances which can be separated only with difficulty from the azobenzole.

A portion of the azobenzole obtained in the above process was converted by means of sulphuretted hydrogen into benzidine. Of the beautifully crystallized compound, a platinum salt was made which was analysed by Mr. Noble.

0·268 grm. of the salt left 0·088 grm. =32·88 per cent. of platinum. The formula $C_{12} H_6 N, HCl; Pt Cl_2$ requires 33·09 per cent. of platinum.

Benzidine exhibits a very interesting deportment with nitrous acid; gently warmed in the gas of this acid, obtained by treatment of starch with nitric acid, it gives rise to a powerful reaction. The substance assumes an orange-red colour, and exhibits, after treatment with water, when recrystallized from alcohol, all the properties of azobenzole.

The reproduction of this body from benzidine was moreover fixed in the following numbers:—

0·215 grm. of substance gave 0·623 grm. of carbonic acid, and 0·112 grm. of water.

	Theory. ($C_{12}H_6N$).	Experiment.
Carbon	79·12	79·02
Hydrogen	5·49	5·78
Nitrogen	15·39	
	100·00	

The simplest formulæ of azobenzole and benzidine only differ by one equivalent of hydrogen,—

Azobenzole...... $C_{12}H_5N$
Benzidine ...... $C_{12}H_6N$,

a relation which sufficiently explains the transformation and reproduction of azobenzole.

"On the Formation and some of the Properties of Cymidine, the Organic Base of the Cymole Series." By the Rev. John Barlow, F.R.S., Sec. R. Inst.

The object of this memoir is to detail the process by which an organic base, provisionally named *Cymidine*, was obtained from the hydrocarbon, cymole, and to describe some of its properties, and certain phænomena attending its production.

The substitution-product, nitrocymole, was procured by acting on cymole by strong nitric acid, both liquids being kept at the temperature $-17\frac{7}{9}$° Cent. (0° Fahr.). From nitrocymol, cymidine was obtained by Béchamp's modification of Zinin's process, and results of analyses, made by combustion of the platinum salt, and likewise by a silver determination of the hydrochlorate, were found to coincide with the formula $C_{20}H_{15}N$. In the formation of cymidine a neutral oil occurred, having the same boiling-point with cymole. From this hydrocarbon a substitution compound was derived, apparently isomeric with, but possessing a less specific gravity than nitrocymole. This nitro-compound was also subjected to the process of reduction already described, and a basic substance was formed from it, which was identified by a platinum determination with cymidine. Some qualitative experiments, made with cymidine, were also described, and the memoir concluded with the following synoptical table of the homologues of the benzole series.

Hydrocarbons.		Nitro-substances.		Bases.	
Benzole	$C_{12}H_6$	Nitrobenzole	$C_{12}H_5NO_4$	Aniline	$C_{12}H_7N$
Toluole	$C_{14}H_8$	Nitrotoluole	$C_{14}H_7NO_4$	Toluidine	$C_{14}H_9N$
Xylole	$C_{16}H_{10}$	Nitroxylole	$C_{16}H_9NO_4$	Xylidine	$C_{16}H_{11}N$
Cumole	$C_{18}H_{12}$	Nitrocumole	$C_{18}H_{11}NO_4$	Cumidine	$C_{18}H_{13}N$
Cymole	$C_{20}H_{14}$	Nitrocymole	$C_{20}H_{13}NO_4$	Cymidine	$C_{20}H_{15}N$.

Letter from Dr. Herapath to Professor Stokes, "On the Compounds of Iodine and Strychnine."

June 20, 1855.

My dear Sir,—Will you do me the favour to announce to the Royal Society, that I have been engaged, during some months past, in investigating the optical and chemical properties of some crystalline compounds of iodine and strychnine which appear to be strongly indicative and peculiar? One of these bodies, from the analysis hitherto made, would seem to have a formula not very different from the following, viz. $C^{42} H^{22} N^{2} O^{4} + I^{3}$, and crystallizes in hexagonal prisms, passing by the ordinary replacement planes to the acute rhombohedron and other forms, all apparently derived from the rhombohedric system; some of these crystalline forms are very strange and unusual. This substance possesses "double absorption" in a very evident degree, and when examined by vertically plane polarized light, the hexahedral prisms are all obstructive of the polarized beam when the length of the prisms lies parallel to the plane of the incident ray; in this position they appear dark sienna-brown in colour; when the long axis of the prisms lies perpendicular to the plane of primitive polarization the crystals transmit a lemon-yellow tint, passing through greenish yellow to sherry-brown.

The other substance appears to be the sulphate of iodo-strychnine and has a decidedly metallic green reflexion, crystallizes in stellate aggregations of prisms, brilliantly green by reflected light, but having a deep blood-colour by transmission; these also possess double absorption and are very peculiar, as a slight increase in thickness renders them wholly impervious to light.

When these matters have been more carefully worked out, I hope to have the pleasure of communicating the results to the Society: in the mean time the present notice will be sufficient for the object in view.

I remain, my dear Sir,
Yours very truly,
W. BIRD HERAPATH.

Professor Stokes, F.R.S.

"On the Constitution and Properties of Ozone." By Thomas Andrews, M.D., F.R.S.

The conflicting views which have so long existed as to the true constitution of ozone, induced the author to undertake a careful investigation of the subject, particularly as he had reason to doubt the accuracy of the only quantitative experiments which have yet been made to elucidate this difficult question. According to the experiments referred to, two substances have been confounded under the name of ozone, one a compound body having the formula HO_3, the other an allotropic variety of oxygen. To ascertain whether, in conformity with this statement, ozone obtained in the electrolysis of water contains hydrogen as a constituent, the author made two series of experiments. In the first series, he followed nearly the same method of investigation by which its compound nature is supposed to have been established, but modified so as to avoid a source of error, which, if neglected, vitiates altogether the results. Electrolytic oxygen, unless very great precautions be taken, is always accompanied by a small but appreciable quantity of carbonic acid, which is liable to be partially absorbed by the potassa set free when a neutral solution of iodide of potassium is decomposed by ozone. By adding a little hydrochloric acid to the solution of iodide of potassium before the commencement of each experiment, this error may be avoided.

The method of performing the experiment was to conduct a stream of electrolytic oxygen through a compound apparatus previously weighed, which contained on one side an acid solution of iodide of potassium, and on the other sulphuric acid; the former to decompose the ozone, the latter to prevent the escape of moisture. The increase in weight of this apparatus gave the entire weight of the ozone; the iodine set free, when reduced to its equivalent in oxygen, the weight of the active oxygen. The precautions to be taken in conducting this experiment are fully described in the communication.

The following are the numerical results of five experiments performed according to the above method:—

Volume of electrolytic oxygen.	Increase in weight of compound apparatus.	Active oxygen deduced from iodine set free.
litres.	grm.	grm.
10·20	0·0379	0·0386
2·72	0·0107	0·0100
2·86	0·0154	0·0138
6·45	0·0288	0·0281
6·80	0·0251	0·0273
Total	0·1179	0·1178

The agreement in these numbers proves that the active oxygen is exactly equal to the entire weight of the ozone, and is therefore identical with it.

In the next series of experiments the author shows that no water is produced in the decomposition of electrolytic ozone by heat. Large quantities of electrolytic oxygen, containing from 38 to 27 milligrammes of ozone, were decomposed by heat, but no water was obtained in a weighed absorption apparatus, in which the gas was exposed, not only to the action of sulphuric acid, but was also passed through a tube containing anhydrous phosphoric acid.

Having confirmed by new experiments the fact that ozone is formed by the action of the electrical spark on pure and dry oxygen, the author proceeds to institute a comparison between the properties of ozone derived from different sources. These he finds to be in every respect the same. Thus ozone, however prepared, is destroyed, or rather converted into ordinary oxygen, by exposure to a temperature of about 237° C., and catalytically, by being passed over peroxide of manganese, no water being formed in either case; it is not absorbed by water, but when sufficiently diluted with other gases, is destroyed by agitation with a large quantity of water; it is also, contrary to the common statements, destroyed by being agitated with lime-water and baryta-water, provided a sufficient quantity of those solutions be used; it has always the same peculiar odour; it bleaches without producing previously an acid reaction; it oxidizes in all cases the same bodies, &c.

From the whole investigation the author draws the conclusion, "that ozone, from whatever source derived, is one and the same substance, and is not a compound body, but oxygen in an altered or allotropic condition."

LXV. *Intelligence and Miscellaneous Articles.*

ON THE HEAT PRODUCED BY THE INFLUENCE OF THE MAGNET UPON BODIES IN MOTION. BY M. LÉON FOUCAULT.

IN 1824, Arago observed the remarkable fact of the attraction of the magnetic needle by conducting bodies in motion. The phænomenon appeared very singular, and remained without explanation until Faraday announced the important discovery of currents of induction. It was then proved, that in Arago's experiments the motion gave rise to currents, which, by reacting upon the magnet, tended to associate it with the moveable body and draw it in the same direction. It may be said, in general terms, that the magnet and the conducting body tend towards a state of relative repose by a mutual influence.

If, notwithstanding this influence, the motion continue, a certain amount of force must be constantly furnished, the moveable part be, as it were, pressed by a break, and this force necessarily produces a dynamic effect, which, according to the new doctrines, I have thought must be found in heat.

We arrive at the same consequence by taking into consideration the currents of induction which succeed one another in the interior of the body in motion; but an idea of the quantity of heat produced would only be acquired with great difficulty by this mode of regarding the affair, whilst by considering this heat as due to a transformation of force, it appeared certain to me that a sensible elevation of temperature would be easily produced in a decisive experiment. Having ready to my hand all the elements necessary for a prompt verification, I proceeded to its execution in the following manner.

Between the poles of a strong electro-magnet I partially introduced the solid of revolution belonging to the apparatus which I have called a *gyroscope*, and which was previously employed in experiments of a very different nature. This solid is a ring of bronze connected by a toothed pinion with an apparatus of wheels, by the action of which, when turned by the hand, it may revolve with a rapidity of 150 or 200 turns in a second. To render the action of the magnet more effective, two pieces of soft iron added to the helices prolonged the magnetic poles, and concentrated them in the vicinity of the revolving body.

When the apparatus is going with the greatest rapidity, the current of six Bunsen's couples, passed into the electro-magnet, stops the movement in a few seconds, as though an invisible break had been applied to the moving body; this is Arago's experiment, as developed by Faraday. But if the handle be then pushed on, so as to restore to the apparatus the movement which it has lost, the resistance experienced requires the application of a certain amount of force, the equivalent of which reappears and accumulates in heat in the interior of the revolving body.

By means of a thermometer inserted in the mass we may follow the gradual elevation of temperature. Having, for example, taken the apparatus at the surrounding temperature of 60°·8 F., I saw the thermometer rise successively to 68°, 77°, 86°, and 93°·2 F.; but the phænomenon had previously become sufficiently developed to ender the employment of the thermometer unnecessary, as the heat produced had become sensible by the hand.

If the experiment appears worthy of interest, it would be easy to arrange an apparatus to reproduce and exaggerate this phænomenon. There is no doubt that by means of a machine properly constructed, and composed only of permanent magnets, high temperatures might be produced, so as to place before the eyes of the public assembled in lecture rooms a curious example of the conversion of force into heat.—*Comptes Rendus*, Sept. 17, 1855, p. 450.

METEOROLOGICAL BSERVATIONS FOR OCTOBER 1855.

Chiswick.—October 1. Fine: showery: fine. 2. Foggy: cloudy: foggy: rain. 3. Dense fog: rain. 4. Densely clouded: heavy rain. 5. Fine: heavy rain. 6. Very fine: cloudy: rain. 7. Fine: foggy at night. 8. Rain: very fine. 9. Foggy: very fine. 10. Fine. 11. Rain. 12. Cloudy and boisterous: rain. 13. Very fine. 14. Foggy: cloudy. 15. Dense fog: fine. 16. Very fine: rain at night. 17. Rain, with fog. 18. Overcast: fine. 19. Light clouds and fine. 20. Fine: cloudy: fine. 21. Cloudy. 22. Foggy: very fine: cloudy. 23. Foggy: uniformly overcast. 24. Fine: clear: fine. 25. Cloudy and boisterous. 26. Very boisterous: overcast: fine. 27. Exceedingly fine. 28. Very clear: fine. 29. Rain: drizzly. 30. Constant heavy rain. 31. Rain: cloudy: rain at night.

Mean temperature of the month	50°·28
Mean temperature of Oct. 1854	48 ·20
Mean temperature of Oct. for the last twenty-nine years	50 ·00
Average amount of rain in Oct.	2·686 inches.

Boston.—Oct. 1. Cloudy. 2. Cloudy: rain A.M. 3. Cloudy. 4. Rain A.M. and P.M.: heavy thunder-storm. 5. Fine: rain P.M. 6. Fine. 7. Cloudy: rain A.M. 8. Foggy. 9. Cloudy: rain P.M. 10. Fine. 11. Rain A.M. and P.M. 12. Cloudy. 13. Fine. 14. Cloudy. 15. Foggy: rain P.M. 16. Fine. 17. Cloudy: rain A.M. 18. Fine. 19. Cloudy: rain P.M. 20. Fine. 21. Cloudy: rain P.M. 22. Cloudy. 23. Cloudy: rain A.M. and P.M. 24. Fine. 25. Cloudy. 26, 27. Rain A.M. and P.M. 28. Fine. 29. Foggy: rain A.M. and P.M. 30, 31. Rain A.M. and P.M.

Sandwick Manse, Orkney.—Oct. 1. Fog A.M.: clear P.M. 2. Drizzle A.M.: damp P.M. 3. Showers A.M.: rain P.M. 4. Rain A.M. and P.M. 5. Clear A.M. and P.M. 6. Bright A.M.: cloudy P.M. 7. Rain A.M. and P.M. 8. Hazy A.M.: drizzle P.M. 9. Cloudy A.M.: sleet-showers, aurora P.M. 10. Cloudy A.M.: dry; rain P.M. 11. Showers A.M. and P.M. 12. Showers A.M.: clear P.M. 13. Fine A.M.: clear, fine, aurora P.M. 14. Fine, hoar-frost A.M.: clear, fine, aurora P.M. 15. Fine, frost A.M.: clear, fine P.M. 16. Sleet-showers A.M.: clear P.M. 17. Showers A.M.: rain P.M. 18. Showers A.M.: sleet-showers, aurora P.M. 19. Drops A.M.: clear, fine P.M. 20. Rain A.M.: drizzle P.M. 21. Showers A.M.: clear P.M. 22. Cloudy A.M. and P.M. 23. Rain A.M.: sleet-showers P.M. 24. Hail-showers A.M.: sleet-showers P.M. 25, 26. Rain A.M.: showers P.M. 27. Snow-showers A.M.: clear, aurora P.M. 28. Clear, frost A.M. and P.M. 29. Cloudy A.M.: hazy P.M. 30. Bright A.M.: clear P.M. 31. Bright A.M.: cloudy P.M.

Mean temperature of Oct. for twenty-eight previous years	47°·64
Mean temperature of this month	45 ·72
Mean temperature of Oct. 1854	46 ·39
Average quantity of rain in Oct. for fifteen previous years	5·08 inches.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1853. Oct.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·529	29·496	29·00	29·62	29·57	64	40	59	54	50½	sw.	ssw.	ese.	·02	....	·01
☾ 2.	29·641	29·564	29·10	29·63	29·77	66	40	57	50½	44½	w.	n.	e.	·12	·03	·01
3.	29·707	29·517	29·25	29·71	29·56	62	50	58	46	52	s.	se.	ne.	·36	....	·03
4.	29·369	29·284	28·86	29·34	29·08	68	47	59	50	53	sw.	s.	se.	·70	·25	·22
5.	29·351	29·331	28·86	29·09	29·10	56	50	54	55	49	sw.	ssw.	se.	·82	·28	·30
6.	29·351	29·282	28·92	29·16	29·28	65	50	52·5	51	53½	s.	ssw.	ese.	·11	·10	....
7.	29·370	29·250	28·76	29·28	29·36	64	39	57·5	54½	53½	s.	sw.	e.	·03	·20	·47
8.	29·546	29·467	29·00	29·48	29·53	67	36	50	53½	52½	sw.	calm	nne.	·09	....	·45
9.	29·512	29·488	29·04	29·58	29·61	62	41	49·5	46½	41	w.	w.	nnw.	....	....	·12
10.	29·786	29·678	29·26	29·56	29·28	58	39	41	43½	46	nw.	nw.	ssw.	·02	·17	·08
● 11.	29·697	29·573	29·14	29·20	29·17	61	52	52	46½	47	sw.	w.	w.	·25	·20	·30
12.	29·644	29·590	29·14	29·24	29·27	62	37	52	47	42	sw.	w.	calm	·33	·12	·16
13.	29·649	29·585	29·20	29·37	29·39	60	39	45	47	38½	w.	w.	calm	·01	....	·07
14.	29·502	29·448	29·10	29·36	29·36	58	31	46·5	41	38½	nw.	calm	calm	·01	....	....
15.	29·597	29·412	29·05	29·35	29·37	59	32	39·5	40	43½	s.	calm	sw.	·09	....	....
16.	29·733	29·703	29·32	29·44	29·70	58	38	39·5	45	39½	sw.	sw.	n.	·56	·15	·15
17.	29·841	29·705	29·40	29·56	29·38	60	38	48	44	50	ne.	e.	w.	·10	·05	....
☽ 18.	30·012	29·863	29·45	29·34	29·58	64	41	42·5	51	46	w.	wsw.	wnw.	....	·04	·19
19.	30·134	30·045	29·60	29·67	29·87	63	39	49	46	41	sw.	w.	s.	....	....	·06
20.	30·177	30·123	29·68	29·50	29·56	61	50	50	48	52½	sw.	sw.	w.	....	·02	·44
21.	30·169	30·088	29·50	29·50	29·83	62	45	59	53	48	sw.	sw.	w.	·01	....	·33
22.	30·188	30·045	29·74	29·86	29·63	66	46	48	50½	50	sw.	w.	s.	....	·02	·04
23.	29·966	29·841	29·40	29·22	29·33	61	40	55·5	52	41	sw.	ssw.	ssw.	....	·03	·14
24.	30·052	29·901	29·40	29·17	29·51	55	33	46	38	40	w.	wsw.	nw.	....	·02	·47
○ 25.	29·961	29·545	29·47	29·03	28·69	55	50	46	42	49	sw.	ssw.	ssw.	·38	....	·42
26.	29·202	29·121	28·64	28·52	28·93	56	31	50	42	40	sw.	ssw.	nw.	·28	·32	·49
27.	29·639	29·344	29·00	29·30	29·74	54	26	43	37	36½	sw.	nne.	n.	....	·12	·33
28.	29·714	29·685	29·40	29·78	29·70	53	40	42	33½	33½	n.	nw.	se.	·03	·03	....
29.	29·523	29·210	29·22	29·59	29·81	52	46	42	44	44	ne.	nw.	e.	·63	....	....
30.	29·217	29·083	28·80	29·92	29·86	53	46	50	44	43	ne.	n.	nne.	·98	·80	....
31.	29·528	29·300	29·05	29·87	29·89	47	34	44	42½	37½	n.	n.	n.	·22	·75	....
Mean.	29·687	29·567	29·18	29·427	29·732	59·74	40·83	49·2	46·40	45·04				6·15	3·70	5·28

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